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THE

LONDON LANCET.

A JOURNAL OF

BRITISH AND FOREIGN MEDICAL,

SURGICAL AND CHEMICAL SCIENCE,

CRITICISM, LITERATURE AND NEWS.

FOR THE YEAR 1872.

EDITED BY

JAMES G. WAKLEY, M.D., M.R.C.S.

New York:
1872.

37401

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THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 1. NEW YORK, JANUARY, 1872.

Introductory Address

DELIVERED AT

ST. MARY'S HOSPITAL MEDICAL SCHOOL,

October 2nd, 1871,

By Alfred Meadows, M.D.,

Physician-Accoucheur to the Hospital, and Lecturer on Midwifery at the School.

Gentlemen,—It has been said by a distinguished political leader that institutions are now upon their trial, and that those only which will bear the test of scrutiny can hope to maintain their ground. Now the delivery of an introductory address at the opening of the medical session is an institution older, I believe, than the memory of any here present; but, as in other and weightier matters, age is not sufficient to protect this or any institution from the ruthless scrutiny of the nineteenth century. Accordingly we find that already at one of our oldest metropolitan schools the practice in question has been summarily tried and condemned. Those of us, therefore, who have any conservative feeling in this matter must be prepared to defend our practice; for undoubtedly there is some danger —a danger which is, I fear, inherent in all novelties—that the example thus set may spread. It is very possible that, if I had consulted my own love of ease and convenience only, I might, perhaps, for this year at least, have been tempted, contrary to my general principles, to become a very radical, not to say revolutionary reformer, in this respect. But I feel very strongly that occasions like the present ought not to be lightly cast aside; and if my personal inconvenience had been multiplied a hundredfold, I should still have felt amply compensated by the pleasure which this opportunity gives me of once more welcoming back the older students of St. Mary's to the home of their medical labor, and of stretching out the right hand of fellowship to those who now for the first time sit on these benches as enrolled or intending students of this school. On behalf, then, of the authorities of St. Mary's Hospital, of my colleagues at this school, as well as in my own name, I give you all a hearty welcome; and may I venture to express the hope that the present session may be to each one of you the commencement of a happy and successful career?

One obvious use, I think, of these introductory lectures is the tendency which they have to keep alive that bond of Freemasonry which ought to exist between all the members of a profession such as ours, and notably between the pupils and teachers of each medical school. It is almost the only occasion in the academical year on which, being free from other collegiate duties, we, teachers and pupils of all years, meet together for the special purpose of being together, and for the mutual interchange of thought with thought; this, I am sure, is a strong plea in its favor. We teachers are not tied on these occasions to the limits of the particular subject which we have to teach; while you, on the other hand, are not necessarily here to learn, but may hope to be interested or amused—though, to avert disappointment, I trust that your hopes in this respect are not very exalted in the present instance.

Before passing to the general subject of my address, let me say a few words in regard to some changes which have taken place in this school during the past year—changes which illustrate very forcibly and painfully the fluctuating character of all human institutions. By the mere lapse of time, we may indeed be thankful it is from no other cause, we have to regret the partial loss of three of our most distinguished colleagues—Dr. Sibson, Dr. Tyler Smith, and Mr. Lane. I say partial loss, because their names are still on our staff, their hearts are yet in the place and in the work, and their spirit still energises the labors of their successors. We have also to regret the loss of Dr. Russell as Lecturer on Chemistry, and of Dr. Payne as Lecturer on Pathological Anatomy. But we can generously afford to congratulate the schools of St. Bartholomew and St. Thomas, which have gained their services, while we welcome their successors, Dr. Wright to the Chair of Chemistry, and Dr. Reginald Stocker as Medical Tutor and Pathologist, an appointment now for the first time made in this school. In the latter, we believe that a colleague has been found to whom you will feel more than ordinarily grateful, if you will use him as he would wish you to do. And here let me take this opportunity of saying a few words in regard to the office of medical tutor which Dr. Stocker now fills. I believe such an appointment may be of the greatest possible benefit to you all. In my early days the system of apprenticeship was still in full force, and I think it is much to be regretted that that practice has almost completely died out; while the only attempt at a substitute for it has been made entirely optional by the College of Surgeons, which recognises as part of the prescribed curriculum twelve months' instruction by a legally qualified practitioner, provided that he holds some public hospital or dispensary appointment. I am convinced that a great deal of most valuable information may be learnt during a short apprenticeship to a general practitioner, or in the manner indicated by the College of Surgeons—information, too, which cannot be obtained so well in any other way, unless it be by the method which we now propose to institute through the medical tutor. His special object will be to help you to use your eyes, your ears, your hands, and, above all, your understandings, so that you may profit to the fullest extent by the teachings afforded in the lecture-room and wards of the hospital. This system will be carried on by individual personal application, and I am confident that the good which he is capable of effecting with each one of you cannot be over-estimated. Often one of the greatest hindrances to advancement in knowledge, by which a great deal of time, labor, and strength are frittered away, is the want of a definite method of working, and an inability to turn to good account the knowledge which is acquired by study and observation. All this we hope Dr. Stocker will obviate, and for your sakes we hail his appointment as a great boon. In the wards of the hospital he will help you to perfect your methods of diagnosis by instruction in the various modes of physical examination of the sick, and on the best way of elucidating and interpreting symptoms. In the dead-house he will demonstrate to you the consequences of disease, while at other times he will test by *vivâ voce* examinations the progress which you are making in the general course of your studies.

Another novelty, which has been introduced here for the first time this year, is the foundation of three open scholarships in Natural Science, each of the annual value of £40, and tenable for three years, together with an exhibition in Natural Science of the value of £20. For all these, as well as for the appointment of a Medical Tutor, you have to thank the governors of the hospital, who, in the interests of the school, have generously granted the money for these special purposes; and in doing so I am sure they have done a good work—one which will be productive of lasting benefit to the future generations of St. Mary's men, for no one can doubt the enormous value to the practitioner of medicine of a through acquaintance with natural science, and there is no time when this knowledge can be so well acquired as at the commencement of medical study. I believe these scholarships will prove a very happy augury for the future career of the successful candidates; and I have great pleasure now in congratulating Mr. Edwards, a student of the University of London, upon the success which he has achieved in gaining the first of these scholarships. I have pleasure also in stating that he has obtained 502 marks out of a total of 650 in all the examination papers. My congratulations are also due to Mr. G. M. Giles, to whom has been awarded the first exhibition, he having obtained 438 marks out of the 650. To both these gentlemen I venture to express the hope that their future may be as bright and prosperous as their beginning. It is, I think, very satisfactory to know that three of the candidates obtained more than the minimum of marks which would have entitled them to the scholarship; so that they lost it, not because they were not worthy of it, but because they were honorably beaten in a fair fight.

There is another subject to which I must allude for a moment, as it may perhaps be a source of needless anxiety to those who are just entering upon their studies: I mean the alterations in the curriculum prescribed by the College of Surgeons, which requires certificates of having attended separate courses of lectures in Practical Surgery, Practical Physiology, Pathology, and Forensic Medicine. At first sight it might appear as through the physiological course had been greatly extended, but in reality it is not so; on the contrary, the number of lectures on this subject which are now required is actually less than formerly. For whereas formerly five lectures a week were required during the whole of two winter sessions, now you are required during your first winter session to attend only three lectures a week on Practical and Experimental Physiology, and during the second winter session the same number on General Physiology. Your work is therefore lessened during the first two years, and though attendance on rather more lectures is required in the later years, you have then more time at disposal, and your minds will have expanded so as to be capable of taking in much more and with less effort; moreover, the subjects will be of a decidedly practical character. I do not therefore imagine that you will feel particularly burdened, or have any reasonable cause of complaint.

If I may venture on yet one more topic, which partakes somewhat of a personal character, I think you will agree with me that it is a subject for congratulation that we have been able to add something to the comfort and appearance of these school buildings; the older students will I am sure appreciate this, and the new will, I trust, be none the less conscious of it though ignorant of what there was before.

So much, then, on what I may term local matters. Let me now turn to what I regard as one great object of an Address like this—namely, the consideration of some subject in which, as students of science, and as men seriously engaged in the study and practice of one of the highest, noblest, and most important of the professions, we may one and all feel an interest. But here, at the outset, arises an apparent difficulty; for it may well be asked, What subject can a lecturer choose which shall interest an audience so varied in tastes, acquirements, and scientific knowledge, as the one I have to address? As members of a liberal profession, however, our interests are so wide and varied that the difficulty I have referred to is more apparent than real. There is no profession which shares so largely the common interests of humanity as ours, nor is there any which can compare with it in general scientific attainments, for there is scarcely a department of science to which we are indifferent, and with most of its branches we are more or less intimately concerned. Physiology, or biology, for instance, is so closely related to the various physical sciences that the advocates of what has been termed "the physical basis of life" recognise no distinction between it and them. On the other hand, the upholders of the vitalistic theory, while insisting upon the presence of a distinct and separate force in all organic bodies, as the controller of the vital actions, agree nevertheless upon the importance of a thorough acquaintance with physical science as the very groundwork of physiology. This being so, there is an obvious fitness in the foundation of scholarships in natural science by a medical school; and St. Mary's may, I think, be fairly congratulated on having thrown these scholarships open to the whole body of medical students, thereby encouraging a study which is admitted to be indispensable to a proper knowledge of that without which all our other studies would be practically useless. Upon the subject of biology, regarded in its scientific and religious aspects, I shall have more to say presently; for I propose this evening, so far as time will permit, to consider the question of life theories: believing as I do that it is a subject of the first importance to us all —one, in fact, which underlies all our investigations into the phenomena of disease, which is the basis of our practice, the very groundwork of our therapeutics, and therefore a subject peculiarly fitted for such an occasion as the present. Moreover it is one which may, I think, be made intelligible, and I trust interesting, to the youngest of my hearers.

But if out profession has such varied interests in its scientific aspects, its relation to the great social problems of the day is scarcely, if at all, inferior. It may be said, in truth, that society itself could not exist independently of the profession of medicine. In political life, also, the medical profession is yearly growing in influence and importance, and no politician can with any hope of success aim at influencing the Government of the country if he be indifferent to the value of medical opinion in public affairs. Indeed, I have the best reasons for knowing that the political leaders of both sides of the House would gladly welcome a much larger number of medical men to the House of Commons than at present belong to it. Again, the relation of the clerical and the medical professions is a subject which might well occupy our attention for an hour, not only with interest, but also, I venture to think, with advantage to the members of both professions. We owe a great deal to the clerical profession in times gone by; and, working as we so constantly are side by side, I cannot but think that we might often help each other, and better help those to whom we minister in common, if we worked more hand in hand. Time was when there were no two professions of this kind, but the gift of healing was the special prerogative of the presthood, not only in Pagan but also in Christian times. In the Egyptian priesthood, and throughout Roman and Grecian history, this was especially the case, and the temples of Æsculapius and of Cos were the standing memorials of this union. Happily, however, that union has long been dissolved, for it was impossible that the science of medicine could advance if its study and practice were kept in the hands of men whose time was more than occupied if their priestly duties were properly discharged.

Thoughts like these, then, suggest many topics for consideration: the relation of medicine to science in general, to society, to politics, or to theology, would be interesting subjects for consideration; but it is impossible to compass all or any one of these fully in one short lecture. I can only therefore suggest some thoughts which seem naturally to rise in the mind when one considers with any degree of earnestness the nature of the work we have to do, and especially the relation of our art to the science of life, and the bearing of the latter on religious belief. I select this subject because I think that one chief value of an introductory address is the opportunity which it gives a teacher of so shaping the mind of the student at the very outset of his career that it may be, as it were, tuned to the work he has to do; and by being set, if I may so say, to the true scientific key, its future harmony with all that is most important in this life may be, to some extent at least, guaranteed. Nor is it to first-year's men or to students only that this thought is applicable; for in these days of rapid scientific advancement, and still more of reckless scientific, or supposed scientific, speculation—of sceptical inquiry and free thought, as it is called, it is well for us all now and again to pause and ask ourselves the question, Whither do our scientific investigations tend? Do they help to confirm, or are they subversive of the cardinal points of Christian belief? If the former, then we may be sure that our method of working is right, our science true, and the knowledge gained to the world will live as long as truth endureth. But if the latter, we may be equally sure that what we think we have descovered is no discovery at all, and will prove to be but the baseless fabric of a dream.

We hear a good deal nowadays, and some people seem to take a special delight in talking about the conflict between science and religion. Now, I am not one of those who believe that true science is or can be in any way or degree opposed, either in theory or in fact, to what is called Revelation. I have too strong a faith in the latter, and too high a love of the former, ever to believe in such a possibility, or to entertain the shadow of a doubt as to their complete oneness in all essentials. It has always appeared to me a simple impossibility that true science and Revelation should ever contradict each other. Speculative science, or, perhaps I ought rather to say, speculative thought, may, and very possibly will, be frequently at variance with revealed truth. But true science, real scientific thought, and most of all the demonstrative facts of science, never have done, and never will do, violence to the undoubted will of the Creator as exemplified in His revealed word. Reason herself revolts from such a thought. For what are we to understand by the terms Science and Revelation? It seems to me impossible to deny that, in regard to their relation to the Great Architect of the Universe, they are and must be absolutely identical. Science I take to be the discovery or demonstration of abstract truth in the laws which govern the natural world, and making deductions therefrom. But those laws must have had a lawgiver: there can be no such thing as self-made law; the very idea is an absurdity. Law, then, implies a lawgiver; and he who created the law must also have created that which the law governs; whether it be in the organic or inorganic world, in the animal or vegetable kingdom. In short, the whole realm of what we call Nature, governed as it is by law, witnesses to the Lawgiver as the author of its dynamical agency, while, at the same time, the material Universe, as we now see it, is a necessary consequence of the operation of law.

On the other hand, Revelation is nothing more than the proclamation, through certain specified channels, of the laws or the machinery, if I may say so without irreverence, by which God wills to govern the moral world, and to bestow upon man a higher existence than his present life. In fact, if I might venture upon a parallel, I should say that Revelation is the scientific authority for the moral law, just as the recorded observation of facts in Nature becomes the scientific authority for natural law. Both authorities are of equal value as bases of truth, and as expressions of the Divine will; and just as ignorance of natural science engenders among the proud and foolish contempt for scientific things, so ignorance of theological science is apt to engender, with the intellectually proud, indifference to sacred things.

In trying, then, to discover the secrets of Nature, to unfold her laws, and to lay bare her operations, we are all the while endeavoring, though we know it not, to extract as it were more and more of the Mind of the Creator as it lies hid in the works of His Hands. But what, I ask, is this but an attempt to complete another Revelation? And can we suppose that He, who is the author of the written Revelation of His Will, will contradict Himself in that other Revelation which man is permitted to read from the Book of Nature? If this contradiction were possible, then God would be proved to be a Being absolutely less trustworthy than many of His creatures. Such a thought is sufficient at once to negative the assumption, and therefore, I repeat, the idea of any opposition between true science and Revelation—in other words, between the observed Will of God in Nature and His recorded Will in Grace—bears on the face of it its own refutation. Conscious of this fact, the man of science works on patiently and fearlessly, with full confidence that any discovery he makes is in reality an addition to a new Revelation, which is quite as much of God as is Holy Scripture itself. Such a thought should make us very jealous for the truth of science, very careful and painstaking in our observations, temperate in our discussions of disputed points, and, above all, guarded in our speculations; lest, in advocating what is not true, we not only do our best to perpetuate what is false, but we, in fact, slander the Lawgiver, just as much as he does who misrepresents the thoughts, words, or actions of his fellow-men.

I ought, perhaps, to make some apology, or to offer some explanation, for occupying so much of your time with what may appear to you to be a purely theological argument. But no one who is an attentive observer of the general current of thought in these days can fail to see that, while this is an age characterised especially by great enthusiasm in science and in religion, there are not wanting indications of an attempt to bring these two branches of knowledge into opposition. It seems to me, therefore, that it is the duty of those who are in any way connected with the teaching of science to insist occasionally, when opportunity offers, upon the close relation which exists between the two. Their union makes thought perfect; separate them, and you divorce science from what can alone add happiness to knowledge.

The subjects to which I wish now to direct your attention afford a remarkable illustration of the way in which these two enthusiasms are very unnecessarily forced into collision. I mean the subjects of the Origin, and the Nature, of Life. How did life first come upon this earth? What is its exact nature? To neither of these questions probably can, or ever will, any precise and accurate answer be given. But the subject is none the less interesting on that account, and its consideration may teach us one or two lessons; while at the same time anything connected with biology must ever be of the highest importance to us, whose lives are spent in endeavoring to maintain life, and to remove all that tends to destroy it.

In regard to the origin of life, it is, I suppose, beyond question that the earliest known geological record of the existence of life on this earth is to be found in the Laurentian strata of rocks. These, which are the earliest of all the known rocks, belong to what is called the Præ-Cambrian Period; and in them, within the last few years, there has been found a small fossil the organic nature of which was at first questioned, but it is now, I believe, generally admitted that this little creature belonged to the class of Foraminifera, and it has been christened with the name *Eozoon Canadense.* This is undoubtedly the oldest known form of living creature on the earth. We may not, however, affirm dogmatically that, because the geology of that period affords no evidence as yet of life other than that I have mentioned, therefore none existed; such a statement belongs to what I call speculative science, and he is a rash man who would speculate upon the condition of life on this earth at such a remote period.

Tracing life onwards, so far as geological records enable us to do, although there are at present a great many gaps in the series, yet I am not, I think, over-stating the case when I say that from the *Eozoon* onwards there exists in the successive strata of rocks evidence of a gradually ascending series of animals up to almost the highest vertebrata. Whether or not these are related to one another, and man to them all—whether or not the whole animal creation, from beginning to end, is to be regarded as one gigantic family, whose ancestral origin dates from the earliest geological period—is a problem which I certainly cannot pretend to solve. But even if it be so, and should it be demonstrable, as is so confidently predicted by some authorities, that man originally sprang from a shapeless mollusc, the thought need certainly not distress us over-much, although it does not perhaps, at first sight, tend to raise our conception of self to be told that "man is descended from a hairy quadruped, furnished with a tail and pointed ears, probably arboreal in its habits, and an inhabitant of the old world"; nor will our pride be encouraged by knowing that "this creature, if its whole structure had been examined by a naturalist, would have been classed amongst the quadrumana as surely as would the common and still more ancient progenitor of the Old and New World monkeys," because, it is added by the writer I am quoting, "the quadrumana and all the higher mammals are probably derived from an ancient marsupial animal, and this through a long line of diversified forms, either from some reptile-like or some amphibian-like creature, and this again from some fish-like animal." But, in case this idea of the origin of our race is not enough to make us humble-minded, we are further told that "in the dim obscurity of the past we can see that the early progenitor of *all* the vertebrata must have been an aquatic animal, provided with branchiæ, with the two sexes united in the same individual, and with the most important organs of the body (such as the brain and heart) imperfectly developed. This animal seems to have been more like the larvæ of our existing marine ascidians than any other known form."

Well, as I have said, if all this be true, and I must say it is a long way from being proved as yet, it does not appear to me in the least degree to affect man's present dignity, or his responsibilities to his conscience and his God. These are in no way touched by his mode of origin; for they arise out of his present condition, and are the natural outcome of that mighty, that stupendous act, which was performed for him, and for him alone, when God became incarnate. Speculative science cannot reverse the history of the Christian era, neither can it explain away facts which eye-witnesses have written for our learning. People tell us sometimes that Christianity must be a myth, because the Bible makes man to be such a noble act of creative power, whereas science, they say, shows him to have descended lineally from a marine ascidian. Having, therefore, such a degraded origin, they tell us, the whole theory upon which his eternal salvation is supposed to be based must be visionary; for why should his salvation be of such importance, and how, in the face of all that science tells us of his origin, are we to draw the line between the race of men who are to be saved and the brutes who are not? To all which I can only reply that Christianity is certainly not based upon any exalted estimate of man's corporeal origin; on the contrary, the same authority which tells me how he is to be saved actually makes him of meaner origin than do these speculators: for in that book I read, not certainly that man came from a marine ascidian, but that he was formed out of the dust of the ground. If then the argument be worth anything at all on the scriptural side, *a fortiori* is it on the scientific.

But to return to my subject. Assuming for the present that the first living creature on this earth was the *Eozoon Canadense*, the question we have to consider is, Whence did that life come? What, in fact, was the origin of life here? Professor Tyndall, in an essay "On the Scientific Use of the Imagination," says: "Life was present potentially in matter when in the nebulous form, and was unfolded from it by the way of natural development"; and he thinks "there are the strongest grounds for believing that during a certain period of its history the earth was not, nor was it fit to be, the theatre of life." He believes, moreover, that the probabilities are in favor of the earth being then in a nebulous, not a molten, condition; and that what he calls "creative energy" waited till the nebulous matter had condensed, and until other changes had taken place which fitted the earth to become the theatre of life, and then it—that is "creative energy"—sent forth the fiat, "Let life be."

Now, that such an hypothesis as this should be gravely put forward in such terms by a man of Professor Tyndall's position, is, I think, a lamentable thing; and I would solemnly warn you who are just beginning your career of scientific study against being led away by such a flighty—for I cannot call it scientific—use of the imagination. What can be meant by the terms "natural development" and "creative energy" as employed in the passages I have quoted, except it be intentionally to exclude all idea of a Personal Creator in the first beginnings of life. Against such mischievous theorising I protest most earnestly; and I trust you will none of you be led away from the ancient landmarks of the Faith by reasoning such as this.

Sir William Thomson, in his recent address as President of the British Association, after remarking upon the condition of the earth when no life could exist upon it and subsequently when, though fit for life, none was there, says, "we must not invoke an abnormal act of creative power" if we can find any other "probable solution consistent with the ordinary course of nature." But I would remark, first, that the word "abnormal" is here singularly out of place; for what right have we to suppose that the first act of the Creator's will in originating life on this earth was in any true sense an "abnormal act"? and, secondly, it appears to me the merest folly to attempt to explain the very beginning of a new order of things, such as must have been the first occurrence of life on the earth, by what is termed "the ordinary course of nature," for up to the time when life was first made manifest there was no "ordinary course of nature" in respect to it with which any hypothesis could be "consistent." But when, further, Sir. William Thomson, unwilling apparently to recognise a distinct creative act, hazards the hypothesis that life may have been brought here by "countless seed-bearing meteoric stones moving about through space," and "carrying seed and living plants and animals," and that if no life previously existed on this earth "one such stone" might "lead to its becoming covered with vegetation," I think really, if scientific men can gravely put forward such "wild and visionary" notions as these, to use an expression which Sir William himself suggested, then the sooner we look elsewhere than to science for explanations of these phenomena the better will it be. It may be true, as he says, that such an hypothesis "is not unscientific;" all I can say is, if it be not, so much the worse for science.

But, you will no doubt ask, what view have I to propound in place of those which I reject? I reply, that I believe no strictly scientific answer can be given to the question at issue, because we have no sufficient data upon which to found an opinion. It seems to me impossible to separate the idea of mystery from the phenomena under consideration, and therefore equally so to find any explanation by a study of the laws of nature. At the time in question, as I have said, there was no Nature such as we understand it, and no reference to present phenomena can avail to explain what occurred when the circumstances were in no degree parallel. Any such attempt must be futile, and, inasmuch as we have no other data by which to judge, the inquiry must be barren and consequently useless. Life came; that I know. Nature does not explain it; that I also know; and no amount of induction from facts which we can now observe will avail to account for phenomena which occurred when no such facts existed. Revelation and Reason both alike tell me that God willed it, and it was so; but neither tells me how.

I am well aware that this does not satisfy natura craving for a more perfect knowledge. But we are surrounded by mysteries which we cannot fathom, and this I believe is one of them. There is often methinks, greater wisdom displayed in knowing what we do not know, and greater courage in candidly confessing it, than in assuming a knowledge which we have not, and mistaking audacity for courage. A lady once asked a distinguished member of the French Academy of Sciences, "What is the use of being an academician if you can't tell what comets are made of?" To which the learned man replied, "Madam, that I may be able to say, I don't know." Such a reply testifies, I think, far more to the true man of science than he who fritters away his imagination over impossible problems.

But if the origin of life be a difficult question, scarcely less so is that of its nature. What do we understand by Life? Is it merely a condition of matter, or is it a force? If the latter, can it exist apart from matter? Is it independent of other forces? or is it correlated with them? These are all most difficult, and, in more senses than one, most vital questions. I have only time briefly to indicate the answers which facts seem to warrant, and I shall not allow myself to wander in the realms of fancy. In all ages there has been, there is still, great diversity of opinion on this matter; but, in these latter days, the opponents may be said to range themselves into two hostile camps; the one advocating what is called "the physical basis of life"; the other contending for a vital force, principle, or essence, differing from and altogether independent of the ordinary physical forces. The former assert that the force or forces which operate in organised bodies, whether vegetable or animal, are the same that exist in the inorganic world: that, in fact, life is correlative with the other forces of nature, and may be, for aught we know, nothing but a modification of heat or motion. In like manner, according to this view, heat, light, electricity, motion, and chemical action, are to be regarded as physical forces when they operate in living bodies; but only as physical forces when met with elsewhere.

In opposition to this, it is contended by the vitalists that life is as much a separate force as motion, chemical action, electricity, or light; that its operations are entirely confined to organised bodies; that its existence cannot be conceived apart from these; and though its operation is never manifested without a corresponding physical change, or the evolution of some physical force, yet that it is altogether independent of these, not correlated with them, not subservient to, but in truth dominating them all, and acting sometimes in direct opposition to some of them, in a way in which no other physical force could act. Such I believe, is a fair statement of the opposing views of life which are entertained by the leading authorities in the present day, and you will not fail to note the relation which these views bear to the question I have already discussed—namely, the origin of life; and also to another question which is just now exciting great attention—namely, the doctrine of spontaneous generation. It is obvious that if life be mearly a correlation of physical force, spontaneous generation, and even the creation of organised beings by physical agencies, is not conceivably beyond the power of man to achieve. Therefore, again, it follows that the acceptance of this view does away with the necessity for a creative act, and makes it possible that the original beginning of life was due to the operation of ordinary physical forces.

But if this question is to be determined by observation and experiment, not by inference or imagination, then undoubtedly the Scotch verdict of "not proven" must be accepted as against the physical hypothesis; for as yet there is not on record, so far as I know, a single experiment or observation in support of it. On the other hand, the whole of the observed phenomena of life—nutrition, growth, reproduction, &c.—are totally unlike anything which is seen in the inorganic world; nay in many respects these processes take place in direct opposition to the laws which govern the other forces. For instance, gravitation has no necessary influence upon vital action; on the contrary, vital action is not unfrequently exerted in opposition to it. Growth is thought by some to have its counterpart in the formation of crystals, but it is surely not necessary that I should refute this statement; while nutrition has certainly no parallel in the inorganic world, neither is reproduction comparable to any known physical process. While, therefore, I am ready to allow that many of the changes which take place in organised bodies are of a purely physical character, and resemble the same changes as they occur in inorganic bodies, yet I cannot avoid the conclusion that the processes to which I have referred are brought about by the operation of a force or forces which are unknown in the inorganic world. Nor do I see why actions so striking, so manifest and peculiar should not be ascribed to the operation of a special distinctive force, just as electricity, chemical action, heat, motion, &c., are regarded as separate forces because of their manifest and peculiar actions; for aught we know, there may be other forces yet to be discovered in the inorganic world which will differ as much from the present known physical forces as these do from one another.

Again, the argument in favor of a distinct and separate force is, I think, greatly strengthened by the consideration of what takes place in an organic body when, as we say, life has fled. Then at once the physical forces, such as chemical action, which have been kept in check, if not by the vital force, at least during vital action, exercise an undisputed sway over the now dead body; and the result is that its component parts gradually but surely lose all trace of structural character, and are resolved into their ultimate chemical constituents: *construction* is at an end; *destruction* is rampant, and no power suffices to avert the catastrophe.

But you will of course ask, If the doctrine of the absolute indestructibility of force be true, what becomes of the so-called vital force when a living thing dies? Unless this can be explained, you may argue, the fact that various physical forces continue to operate in or upon the now dead body, though in a different manner to what they did in the living body, is presumptive evidence in favor of the purely physical, and against the vital, theory of life. I admit the cogency of the argument, and I frankly avow that I am not able to meet the objection definitely. It seems to me, however, either that the vital force, assuming for the moment its existence, differs from the physical forces in this respect; or else I must claim for it that, though a distinct and separate force, it may nevertheless be in some way correlated with them when the conditions of its action no longer exist. The latter view does not seem to me at all to militate against its individuality; and if this concession satisfies the physicists, then all I can say is they are welcome to it.

I often think, indeed, that the controversy is, after all, much more one of words than of ideas, for when a recent writer on the physical side describes "vital energy" as "a mode of motion," and yet declares that "it manifests itself in modes peculiar to itself, and different from any of the other modes of energy with which we are acquainted," and allows the use of the term vital force "to signify this special mode of manifestation," I do not see what more the vitalists can want,—although I submit that the idea of this vital force being only "a mode of motion" is but another illustration of the so-called "scientific use of the imagination."

While, then, it seems to me that the idea of a distinct and separate force governing the organic world, plant and animal alike, and called by the name "vital force," accords best with the actually observed phenomena of life, and may therefore be accepted as, if not true, at least highly probable; yet, on the other hand, even if "the physical basis of life," as it is called, could be actually proved, I should certainly dissent altogether from the view which has recently been put forward by a very distinguished physiologist, to the effect that, as a consequence of such proof, "a complete and widespread revolution in religious belief" must necessarily occur. The writer to whom I am referring expresses his conviction that such a doctrine would be "scarcely reconcilable with the idea that man is made in the image of God," and therefore he adds, "The idea of a God, of Divinity of every kind," must be abandoned, because, "in such a scheme neither a superintending Providence, nor a personal God, nor Christianity, could have place"; for, asks this writer, "how could I believe that I was, nevertheless, designed and created by the power and wisdom of God?"

Now, I think that such language as this, coming as it does, too, from a conscientious and thoughtful observer, is greatly to be deplored. I should, indeed, be sorry ever to make such a statement in regard to any scientific theory, no matter what its scope or tendency. The history of scientific beliefs is full of instructive lessons in this respect, and we cannot yet afford to dogmatise too narrowly on the connexion of science with Christianity in such a manner as to make the one dependent upon the other. This much, however, I do know, that on the principle which I have already enunciated, not even the clearest proof of the so-called physical basis of life, nor the actual demonstration of any single fact in science, however much it might seem to contradict my most cherished beliefs, would have the smallest influence in shaking my faith in the fundamental truths of Christianity. Regarded from a religious stand-point, it matters nothing to me whether the functions of my body or the growth of a tree, are performed in obedience to what is called a physical or a vital law; neither am I concerned to know whether I sprang from a marine ascidian or from nothing; for I know that all laws and all matter, whether in the animal or the vegetable kingdom, the organic or the inorganic worlds, most have originated from the great Lawgiver, and that He saw that they were good.

I have endeavored then, gentlemen, very sketchily and imperfectly I know, to give you some idea of the nature of that mysterious principle, force, power, property, or whatever you please to call it, which you are now called upon to study in its normal and abnormal manifestations in the human body; and which it will be your privilege and duty to guard, protect, and cherish, not only against the attacks of evil from without, but from those also within. We are too apt, I think, in the close study which we very properly give to the symptoms of disease, and the care with which we investigate its effects upon the tissues of the body, to lose sight of, or at least to undervalue, the fact that it is not so much material or organic as dynamical perfection which we should endeavor to maintain. Our whole course of study, dealing as it necessarily does with material objects, not only in physiglogy, but also in pathology and therapeutics, is apt to engender a too materialistic view of disease, and to obscure our view of the morbid processes by which disease is brought about, processes which are essentially deviations from the normal evolution of the life-force, whatever the nature of that force may be. It is so much easier to recognise the effects of what we call disease, the mind comprehends so much more readily what the eye sees and the hand can touch, that we are almost inevitably drawn to the recognition of the organic lesion as the alone cause of the patient's distress, and as the object towards which our remedial efforts should be directed. But it is certain that if we inquire into the matter, we shall find in most cases evidence of what is called functional, or as I would term it, dynamical derangement, long before the existence of organic lesion: and a little thought will suffice to convince us that functional derangement is but another term for the abnormal operation of life-force. For as the force or dynamical agency which is inherent in the life-germ secures the normal growth and development of the body of which it is the germ; so deviations from that force necessarily lead to abnormal tissue change, in other words, to disease, either during growth and development, or in subsequent maturity.

Here, then, we see the importance of studying the laws which regulate the operation of that force, and their effects upon the bodily structure. By observing these actions during health, we may in time hope to gain some clearer conception of the causes of disease—those causes which at present are enveloped in so much obscurity that we can hardly be said really to know the *modus operandi* of any one of them. It is through the subtle workings of the life-force that minute tissue changes are produced; and it is here, therefore, that we must look for the earliest indications of the coming disease, When the grosser manifestations of organic lesion are apparent, it is then too late to trace out the ultimate cause of the evil; and that which, if it could have been recognised and appreciated in its earliest stage, might, perhaps, have been combated successfully by a very simple remedy, now baffles and defeats all our efforts. Recent observations have shown how, by the thermometer, timely warnings of impending mischief are often given long before any other indications are apparent. It is in this direction that we must look for future advance in medicine, and each one of you may, if you will, be of infinite service in this work. There is yet an abundant harvest for the honest, painstaking, humble laborer in the medical vineyard. To this work I bid you go forth, full of bright hopes, and strong in your determination to overcome difficulties by patience, courage, and perseverance. But remember always that the one object of your lives should be to seek after and to maintain Truth. Be true to yourselves, and you will be true to your professional calling, than which there is nothing higher or grander in this life. Truth should penetrate every act of your professional, as of your private, life. It will make you careful in diagnosis, and sound in therapeutics, because it will enable you to sift the apparent from the real, the false from the true. It will help you to weigh symptoms, to estimate them at their true value, and by knowing accurately, that is, truthfully, the inward conditions, of which symptoms are but the outward expression, you will be able, with full confidence, to apply the true remedy.

But success of this kind is only obtainable by the persistent practice of truth in every minute particular of your daily life. You cannot play fast and loose with truth and falsehood; for if you do, the latter will in the end be sure to prevail. And remember that "Falsehood is never so successful as when she baits her hook with Truth; and that no opinions so fatally mislead us as those that are not wholly wrong—just as no watches so effectually deceive the wearer as those that are sometimes right." Bear in mind, too, that a man who is always truthful cannot commit sin: for truth and purity go hand in hand; but falsehood is sin incarnate. As men of science, then, prove yourselves worthy of the name by steadfast devotion to truth; and in purity of life follow her example to whose blessed memory this school is named; so that when the time comes that you must change this life for another, you may humbly bow your head in trustful submission to Him who well knows how to recompense those who in the cause of truth have done what they could.

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Introductory Address

DELIVERED AT THE

LEEDS ROYAL SCHOOL OF MEDICINE.

At the Opening of the Winter Session 1871–72.

By T. Clifford Allbutt, A.M., M.D., &c.,

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I think that I cannot better lead you to see the attitude of modern medicine than by asking you first to consider with me what reply we can give to-day to those two great questions which have been handed down from man to man, and from age to age, intensified and urged onwards by the secular cry of human suffering and cruel death: first, What is disease? secondly, Can we relieve it?

No man can hope adequately to handle such questions as these unless he has some fair knowledge of the history of his own science and art; nor do I know any book which at present is more to be desired than a short history of medical doctrine and practice, written by a physician and a philosopher, which should give the student some perception of that chain by which he is bound to those who have gone before him. Among other instructive matters with which that book would deal is the curious difference with which these two questions have been answered. To look at one side only of the matter: in some ages of the world these two questions have been regarded as correlative; latterly they have been set widely apart. Since the days of Bichat and the great biologists who have followed him there has been a growing sense of the infinite subtlety of the human frame and functions, a subtlety which lies so far beyond human ken that he were indeed a bold man who should venture to take this wonderful machine in hand and say, "Go to, now will I repair it."

The great and corresponding advance of pathology has contributed to a like result. Pathologists have found that, after all, they were physiologists; that pathology was but the study of the falling side of human life—the study of the slow condensing of that wonderful exhalation from the dust and ashes which seems as necessary and inevitable as the down-dripping of a jet of steam. Death, they have said, is no accident; it is essentially one with life, and an integral part of the scheme. How can the indivisible be put asunder, and how can I, by taking thought, divide death from life, as the light is divided from the darkness? Thus was medical interference as much crushed out of modern biological science as free-will was crushed out of modern philosophy; and our two questions, what is disease and what is cure, seemed hopelessly set apart.

But how different was it in earlier ages. All early people, being ignorant that disease and death were but the underside of the web of life, conceived that the two opposites were separable. They believed that in the golden time before themselves, when men were cast in a powerful mould, when they led pure lives, and when outer nature was more kindly, they set death at defiance, perhaps for ever, often for many centuries. To their minds, then, disease, and even death, were accidents—something engrafted upon the true stock; hence all their efforts were to get rid of the unwelcome element by some artificial means. A pure elixir of life was sought for on the one hand, and potent charms or medicines were sought for on the other hand, which should cast out the evil or keep it at bay. It was in medicine, indeed, as it was in morals: a man was losing himself in lust and wickedness because, one morning, on going to eat in his garden, he forgot to make the sign of the cross, and so unwittingly he swallowed a devil in a lettuce. The way for his restoration was clear: no health for his soul could be hoped for until the devil was exorcised, which of course he was duly by bell, book, and candle, and the sinner, now become holy, founded a religious house, and died in the odor of sanctity. Exactly so was it with medicine: purge out the offending cause, be it an acrid humour or three black crows, or, in more modern language, a morbid poison, and you will leave a sound man behind.

Again, there is one more direction in which men are strongly tempted to revert to older habits of thought. Even now we are all taught in the nursery, and our very language is so made that we cannot rid ourselves of the words which express such teaching, that man's body and his life are also somehow separable, as we pretend that his diseases are separable. Curiously enough, man's life is not likened to a clock which is self-acting, and moves by the inner virtues proper to the peculiar positions of its own molecules, but it is likened to the motion of a loom which needs the continuance of some outer activity to keep it agoing. This outer agent, or weaver, is called nowadays Nature; and all actions in the body are said to be the doings of Nature, whose proceedings are fortunately regarded as always beneficent, even when she bores a hole into your peritoneum or grows a handsome cancer in your bosom. This is, of course, an error of the same kind as the entity theory of disease; and although much less remains of it than formerly, Nature is still written with a capital N, and appealed to by professors of medicine and others in a way which shows that the old false conception of a distinctly good agent and a distinctly bad agent striving in the body together for mastery is not by any means extinct, and that it is our vile body which *will* go to the dogs, while Nature is always on the watch to drag it up again and perform a wonderful cure. Now, gentlemen, you must get this misleading way of thought out of your heads at once, though it will be a hard task, as you have brought it from your cradles. Let me try to tell you how I would regard the body, and how I want you to see it.

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[*] The author has omitted certain opening paragraphs which had a special interest only.

You know that in ourselves, as in the world about us, we see two great tendencies, which work together, and can only by an artifice of words be considered separately: the one is the tendency towards construction or development: the other is the tendency towards destruction or dissolution. On the one hand, you see the growing expansive forces of the earth building up new forms and new beauties day by day; on the other hand, there is bound up in the very same molecular activities the inevitable downfall of all things towards a final equilibrium. Now there are many forms of rest, or temporary equilibrium between rising and falling—in such a state, for instance, is the table on which I now lean; but there is also an equilibrium of things in motion, such, for instance, as of a pendulum or of the solar system, where molecules and masses play uniformly, and with compensation, about one centre of gravity, or about a system of associated centres. You know that things in such balance will admit of certain perturbation, and that this perturbation may be carried to a point scarcely consistent with the continuance of the balance, or it may be carried beyond that point, when we say that the thing or the system of things is overbalanced; then all the component parts fall away and sink into lower states, until they arrive at some new positions of equilibrium, which will be more or less permanent in their turn, and so on. If you thrust against a rocking stone you have the illustration in a simple form; if you interfere with the human body you disturb a very complex moving equilibrium, but the conditions are identical. The animal body is a complete system of units and parts moving in an equilibrated system: you may disturb this system partially and it will recover itself; you may disturb it, however, to a degree beyond its power of recovery and it falls to pieces, its components going each its own way to find new positions of rest. Or, again, a disturbance may act in another way: it may be so severe or so applied that the system, in what we call convalescence, cannot recover its old state of equilibrium, but it yet may find another nearly as good, or one much worse, but still to be held. Now what is true for these disturbances from without is equally true for those from within: some part of the system gives way, and yet the old equilibrium may be regained; or, again, a new position of equilibrium as good, or not so good, may be attained; or the whole system may fall to pieces. I do not think this is a hard matter; it is supported and illustrated by so many analogies that your minds once being placed in the right point of view, I think you will scarcely fail to hold to the right conception, and to remember that the so-called *vis medicatrix naturæ* is nothing more nor less than the tendency of all things, or system of things in equilibrium to recover the same under the pressure of lesser perturbations.

We have thus given, have we not, some kind of answer to the question, What is disease? It is a disturbance of the equilibrium of a complex system either by something from without, as the solar system would be disturbed by the collision of some other star with our own sun, or by the wandering among us of some foreign orb which would exercise new and strange attractions; or, on the other hand, by something from within, as the solar system again would be deranged or broken up by the crumbling away of one of its principal members. Art and observation in medicine must be directed to watch the functions of the body: so long as these are equable and undisturbed, we have that chief sign of health when the movement of the parts causes no perturbation, however slight, of the whole system. In more familiar words—health is that state when we are unconscious of all and any of our various functions.

Here we come to a most instructive deduction—which is, that the art of medicine must consist in the watching for and the description of these first departures from true running, and in the discovery of those means if, any, which will shoreup any parts which are giving way. The study of morbid specimens after life has its great uses; but you will see that to pore over the *débris* of dead bodies is no more to study the treatment of disease than to visit the ruins of Paris is to study the events of the late war. The mischief is now done; but you wish to understand the beginnings and the progress of those disturbances which caused the outbreak. No wonder, then, that profound pathologists often disbelieve in all treatment. Believe me, gentlemen, physicians are made at the bedside.

Well, then, in watching for any departure from the smooth workings of health, we may look either to see some particular part give way, or to see the effects of some subtle influence deranging the whole. This brings us to one of the most fertile and therefore most interesting results of modern observation—namely, those derived from an extended and accurate study of fever. It is unfortunate that the name "fever" has become strongly attached to certain diseases which happen to show this process most distinctly. We know now, and every day is widening this knowledge, that "fever" is a term signifying a thing of very wide generality; that, indeed, it is nothing more nor less than the expression of a disturbed balance between the disengagement and the dissipation of heat in the tissues of the body. Many of you know it is a physical law that disintegration is always attended with release of force in some form. It happens that the human body in health always preserves an exact balance between this release of heat and its dissipation; so that by measuring the heat of the body we obtain an exact knowledge of the state of its regulative powers. Thus by the thermometer we know whether the system is "swinging true"—whether, that is, the continual oscillations of heat-production are balanced by corresponding oscillations in its removal—whether, in short, the inner work of the tissues and the disposal of their energies are in equilibrium, or whether they are thrown out. It is evident that if, in the next place, we could command the position and preserve or restore this balance, we should be armed with great powers indeed. Now it is clear that we may try to restore the balance in two ways; first, by arresting the production of heat within the body; or, on the other hand, by cooling the outside of the body. The first hope has given rise to the admirable investigations upon the action of quinine, digitalis, and so forth, in fevers, by Professor Binz and others, and which you have seen illustrated in my wards by the use of large morning doses of quinine in the fever of phthisis, and the like; the second hope has given rise to the equally admirable investigations into the action of cool baths to the surface of the body, the latest and most brilliant triumph of which we have just seen in the almost miraculous cure of certain cases of so-called cerebral rheumatism by Dr. Wilson Fox. Thus you see, instead of encouraging Nature, as our forefathers did, and keeping our fever patients warm, we deliberately discountenance Nature and repress her amiable endeavors. Ventilation in fever chambers was first hinted at; this system was extended; then I think I may claim to have advanced the good cause by urging the treatment of fever by the free admission and encouragement of cold air; and latterly the system is carried up to the modern cold-water treatment, so clearly taught generations ago by our own Currie. In some combination of this with the quinine treatment of Binz will for the present be our stronghold in fever: that is, in all states where the balance of force in the body is disturbed by local or general causes. Such, gentlemen, is the practical use of true general conceptions or theories

It is, perhaps, in the region of those infectious diseases in which fever is a prominent symptom that the old and vicious mode of regarding disease as a thing to be expelled from the body most stoutly holds its ground. Dr. George Johnson is, perhaps, the most eminent supporter of the doctrine which is now called "the doctrine of elimination." This doctrine he urged upon us most vigorously in his recent Address in Medicine at Plymouth; and as he then complained—I fear with some little asperity—that I had spoken of him as the priest of a decaying faith, and as he in his turn called me, in a somewhat dyslogistic sense, one of a "very advanced school," I shall venture now to consider the value of the elimination theory, as it is of vital importance at the present time either to accept or deny it. The assumptions required for the elimination theory are these:—First of all, of course, a sick body—one, let us say, in typhus fever or measles; secondly, that this disturbance has occurred in consequence of the introduction of something from without; thirdly, that there is a weaver at the loom of life called Nature, spelt with a capital N (what the meaning of Nature can be except the whole of the bodily functions, good and bad, taken together, it is hard to say, but it is precisely where meaning fails that a word comes in most opportunely); fourthly, that this Nature is beneficent as against some other tendencies of the body which are not; fifthly, that we ascertain which are the beneficent tendencies and which the maleficent simply by taking arbitrarily such symptoms as may fit in with our own preconceptions, and claiming them as on our own side: thus in cholera it is the diarrhœa, in diphtheria I presume it must be the leathery exudation, in scarlatina the rash, in ague the sweat, and so on. The line of treatment grows clear enough: always encourage some one selected symptom, as you may be sure that Nature is somehow trying to get rid of what we nowadays call the "morbid poison." This logic is not confined to professors of medicine. You hear old women daily in our hospital with like stories; how poor Mrs. Jones was taken on dreadful till another old woman gave her some stuff, and she vomited as black as your hat, and has never looked behind her since. Now let us look seriously at all this; for if we are to encourage diarrhœa in cholera, rash in typhus and scarlatina, exudation in diptheria, sweating in ague or phthisis, and so on, it is well that we should know it. The eliminators astonish us by saying that we, their opponents, mistake the cause of disease for the disease itself—an astounding imputation which, for my part, I had serious thoughts of making against them. Dr. Johnson avows his belief in what he considers to be teleology; though his comparison of teleology with Mr. Darwin's exposition of Selection shows that he attaches a peculiar meaning to that term. He seems to hold that any discovery in animal life of an adaptation of means to ends is a proof of teleology. Not in the least is it so. It merely means that all the animals which did not enjoy the advantages of such an adaptation are dead. By "teleology" we properly mean that theory which affirms that sheep bear wool that we may wear coats; that cork-trees grow that we may stop our bottles; and so forth. It would certainly also be teleology to admit that there is some special machinery in man's body designed to expel poison, with the purpose of saving man's life. If Dr. Johnson will trust himself with me for a few hours on a glacier in Switzerland, he will find that Nature is sublimely indifferent to both his life and mine, even though we are both engaged in philosophical discussions. In truth, there is about as much provision for man's safety under one set of external conditions as the other; and there is about as much intelligent direction towards the elimination of a morbid poison from a man's body as there is towards the elimination of a bit of dirt from the works of the watch in his pocket. Do we, as a matter of fact, see the least intelligent effort of Nature to expel those poisons which we can see and weigh? Take a house-painter: where is the elimination of lead from his system till the doctor comes with a draught of iodide of potassium? And so on for any other tangible poison, were it worth while to multiply instances. But it is not worth while; for I shall proceed to show you that the so-called morbid poisons cannot be regarded as poisons in the sense that lead, arsenic, and strychnine are poisons—in the sense, that is, of foreign and separable substances which can be isolated from the tissues, and which act throughout as grains of sand or drops of acid would act upon watch works. We must remember that the contagia of the infectious fevers are transmitted from man to man by a process like generation, and that they infect the whole body, not after the manner of foreign substances or even of growing germs, but by a complete conversion of all or most of the bodily tissues into their own form of activity. The whole body is assimilated to them, so to speak; and the whole body can no more be eliminated from itself, than, reversely speaking, a man can escape from his enemies by jumping down his own throat. How profoundly these infectors modify the molecular constitution of the body, and how completely they convert this molecular constitution into some other form of activity like their own, is proved to us by the permanency of their effects. Expose one who has had small-pox to the same infection, and no or but little reaction is seen. Why? Probably because his tissues are converted into some isomeric form so like the infector that no further reaction can take place between the two; and this new molecular constitution, though it tends, as probably being less stable, to revert to its former position in long periods of time, yet is for the while so profoundly established that it is unquestionably propagated from parent to child as a transmitted protection. Take small-pox into a virgin population, and its ravages are like the plague; so, in like manner, we observe at home that the longer the interval between the epidemics of infectious fevers, the more terrible the reaction to these infectors when introduced, the reason being that there are fewer persons who have obtained or have inherited protection. How, in the face of such intimate changes as these, can we indulge in superficial talk about assisting Nature to deposit the enemy in the chamberpot! No, gentlemen, both philosophy and observation will teach you that no one prominent perturbation in diseases has more *a priori* claim to be regarded as conservative than another: the diarrhœa of cholera stands on no different footing to the collapse; and you will have in practice to repress the phenomena of disease, or, if Dr. Johnson prefers the word, to repress Nature as often as to favor her. Believe me, if you have a patient running up to a high temperature in scarlatina, let us say, or typhus, your duty will be, not to clothe the rash with imaginary virtues and to encourage its appearance, but to suffuse the skin thoroughly well with cold water in obedience to the teaching of positive investigation—that is, if you wish to cure your patient, for to aim at eliminating the original element of disturbance is about as wise and as profitable as to endeavor to put out a house on fire by eliminating the spark which set it ablaze. But enough of this matter. Let me next take occasion to say how it is that this great disengagement of heat is so much to be dreaded. It is not only formidable as a sign of disturbance, but, such is the complexity of disease, it becomes itself the direct cause of much evil. You are many of you aware that recent researches have almost made visible the very point of transition from structure into function. Modern observers have proved to us that not only the white corpuscles but also the walls of the capillaries are in the common and narrow sense alive, and perform independent motions which are of the highest importance to the body. But this vitality persists only within somewhat narrow limits of temperature, so that if the heat of the body be raised but a few degrees these corpuscles and these capillaries lose their activity or are killed. Hence arrest of the circulation in the intimate tissues, and with that the consequent arrest of their work. Dr. Bastian pointed out that plugging of the cerebral capillaries in fever was the visible cause of delirium, and I have found that uræmic symptoms may be due to a like arrest of white corpuscles in the capillaries of the kidneys, and that hypostatic pneumonia may be due to the same kind of plugging in the lungs. Hence you see the immense danger of allowing a high early temperature to continue in fever, for with it you are preparing death in the brain, death in the kidneys, death in the lungs, and probably serious mischief elsewhere—among other things, certainly a widespread destruction of muscular tissue, and probably a profound exhaustion of the ganglionic nervous system. Those of you who have attended my lectures on Fevers will remember that I urge you to direct your treatment intelligently to combat these following five classes of symptoms: firstly, the overheating by drugs such as quinine, acid drinks, digitalis, gentle purgatives, and the like internally, and by cold externally; secondly, to watch for and combat the first signs of carbonic acid poisoning by diffusible stimulants, by the use of invigorants (such as digitalis) to the heart, by bringing fresh air, and by promoting expansion of the lungs; thirdly, to watch for and combat uræmic symptoms due to imperfect excretion by the kidneys; fourthly, to watch the rapid destruction of muscle, with its dangers; and, fifthly, you will have to meet the special dangers of each particular disease, such as perforation in enteric fever, absorption of septic matters from the tonsils in scarlatina, and so forth. If you can carry out these aims with readiness and skill, you will deserve better of your patients than if you try to expel their diseases with a fork.*

And now, gentlemen, I have said enough, or two much, on this head, and I will next pass on to another point of view, though I wish still to urge upon you in another shape the same doctrine, that disease is the sum of the perturbations seen in any given individual, and that the days of the humoral pathology are numbered. Hitherto, in the specific fevers, we have considered especially the factor from without—the infecting element. Let us now consider rather the other factor—the person suffering, or the patient. In medicine, as all persons differ, be it ever so little, you cannot therefore go about with a formula for the treatment of a given disease in whomsoever it occurs, but you must vary your means with what is called the idiosyncrasy of each patient. It might almost be said, to speak very strictly, that no man ever treats the same disease twice, so different are those who suffer. But I think more might be done to make some classification of morbid tendencies; and this must be done chiefly by the accurate records not only of single cases, and not only by records of the course of individual lives, but by careful summing up of all the maladies as they occur in a series of persons related in blood. We should thus be enabled, perhaps, to establish serial affinities between diseases now unsuspected of having anything in common, and so diseases might, for the first time in the world, be classified on something like a natural system. In this, as in many other ways, syphilis is a great instructor. Owing to the accident of a peculiar infection which gives us a definite starting-point, we have been able to string upon this one thread a great number of diseases of the skin, mucous membrane, and viscera, which superficially have no mutual resemblances. I should deny, as you know, that this continuity is due to the tides in the blood of any morbid poison; it consists in a conversion of the whole molecular constitution—an influence as complete as the influence of a particular male has upon his offspring. Now, as we can in this case make a group of diseases apparently distinct, yet by nature closely allied, so no doubt might we construct many other such groups or series. Some of them are half made out already. Take the scrofulous family of diseases, for instance, and see by investigating the histories of families over a wide circuit what a series of unlike diseases may again be classed as a natural order: puriform eruptions upon the upper half of the body, especially the scalp and face, chronic inflammation of certain glands, certain low forms of disease of bones and joints, certain visceral diseases, and, finally, epilepsy and kinds of insanity. Take, again, the gouty families, and in various members of such families we shall again find a natural group of diseases: a peculiar dyspepsia, rheumatism, acute and chronic, and gout properly so called; inflammatory affections of sero-fibrous tissues; scaly diseases of the skin, which are dry, unsymmetrical, and do not tend to spread, but to recur in the same place or places; bronchitis; granular contracting kidneys; arterial degeneration; and here, again, a different kind of mental habit appearing in lesser and greater degrees of anger, spleen, and melancholy. If we discover that all these various manifestations belong to one series or family we have a key to their treatment of an invaluable mastery. Let me now describe to you another family constitution, one which, I think, I can frequently detect, which is common enough, and which will, I think, be readily recognised by experienced family practitioners. For want of any better name, I would call it the neurotic habit or constitution. Persons belonging to this class present, for the most part, the following characters:—They are acute in perception, and quick and energetic, their quickness tending to degenerate into asperity and their activity into restlessness or the fidgets. They are seldom imaginative or sentimental, but rather shrewd and practical—the women being rarely hysterical, which the women of gouty families often are. They are generally small and spare in form, though not always so; they are pale and sallow in complexion; their hair falls off early, so that early baldness is a common misfortune in the women; and their tongues are commonly red at the tip and edges, and often lightly coated with silvery white on the surface. Their tongues, too, are generally long and thin; the gouty tongue being usually broad and flabby. The women are not very fruitful in childbearing, and they often suffer from irregular and painful menstruation. They may be of either light or dark complexion; and their features, though usually small, are various. They are very uniformly temperate as regards alcohol, but intemperate as regards tea; they are small eaters, and bad sleepers. Their friends accuse them of wearing themselves out; and they are often working under the pressure of chronic discomfort, though they usually live long.* These persons are especially liable to diseases of the skin of a spreading kind. Eczema, for example, breaks out in them, not, as in the gouty, repeatedly on well-worn patches, but in a pink, irritable, sweating form, which rapidly spreads all over face, head, and ears, or swiftly invades the whole length of both forearms. If they have psoriasis, it is silvery white, and rapidly covers both legs and arms. These two forms of skin mischief they chiefly present, though they are often sadly tormented with acute attacks of herpes and with urticaria, especially in childhood. Passing from the skin, we find their mucous membranes are not relaxed as in the gouty, but are irritable, and show a great susceptibility to catarrh; they sneeze on the slightest provocation; they have stuffy colds in the head, which last perhaps but a few hours, and pass off; or they have capricious attacks of bronchitis or diarrhœa. Usually, however, they are constipated in the bowels. Asthmatic persons generally belong to this class of patients, and you will often have an asthmatic mother bringing to you her child covered with herpes, or outbreaks of eczema will alternate in the same person with attacks of asthma. On turning to the nervous system more especially, we find neuralgias very frequent, and perhaps even more common in the viscera than in the head or limbs. They will complain of obstinate and unintelligible muscular or visceral pains, and of nothing more often than of those dyspepsias which are really neuralgias of the stomach. Now, the sovereign remedy for persons of this habit is arsenic, and arsenic has from this fact gained the reputation of being a specific in diseases of skin. But the arsenic which clears up the psoriasis of my neurotic friends like a charm will scarcely touch the psoriasis of a gouty subject; but, on the other hand, it not only cures the psoriasis of the neurotic subject, but it likewise cures his diarrhœa, his coryza, his gastralgia, and does much even for his asthma; so you see arsenic is only a specific for skin disease in a very narrow and superficial sense. It supplies something which lacks in a whole class of patients, and, when supplied, improves their nutrition as surely as cod-liver oil improves the scrofulous, as iron improves the chlorotic, or as quinine improves the bogtrotter. And now, gentlemen, I shall not pursue this technical part of my address any further, I have chosen two very different kinds of morbid action to bring before you, as we see them clinically—the one kind, the infectious diseases, which owe their propagation to the continuous ingrafting of inoculating particles; the other kind, which owe their propagation to the not very dissimilar process of generation and hereditary transmission. I have chosen these very different kinds of disease with especial reference to the humoral pathology; and if I have succeeded in my task, I have proved to you that the humoral pathology is a narrow and inadequate explanation of the facts before us, and one which, being weighed and found wanting, must give place to some theory of far wider meaning, and of greater use to us as a guide in therapeutics.

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* Vide Practitioner, vol. i., 1871.

Thus far, gentlemen, I have hoped to carry forward the younger of my hearers with me while I more especially addressed the older; let me hope, as I turn the rest of my discourse more especially to those who now enter this School for the first time, that I may not fail still to carry my older hearers with me. I think, without any undue self-congratulation, I, in the name of my colleagues and myself, may welcome you into a School where there is a great deal of vigorous work going on, and where, on the whole, there prevails a healthy tone of feeling and discipline. The need of working hard for our subsistence and our dear lives is impressed year by year with more terrible force upon the young men of our own station in the world. Competition increases with alarming rapidity; and you have to fear, not only the competition of those of your own middle class, but the eager advance of many able young men from the class of life below you, so great are the advantages of education now held out to all men. So, gentlemen, work you must or starve; this, even at your young age, is beginning to stare you distinctly in the face. This pressure has, however, one great advantage at present, which is, that among students it has brought brains into fashion. When some of my older colleagues were young rowdyism was in fashion; when I was a student it was rather the thing to be a fine gentleman; but now every man must put his shoulder to the wheel; and the prevailing feeling is, that the man who daren't think is a slave, that the man who wont' think is a bigot, and that the man who can't think is a fool. When I find a man in my class who doesn't use his brains, I therefore mentally set him down as belonging to one of these three classes. But, on the other hand, I do not call you to a feast of dry bones; I do not care to urge you to become glib with words, or to learn a string of doctrines by rote; I want you to bring the same healthy, fresh, joyous hearts to your pursuits here that you bring to other pursuits elsewhere. Come here with free, open, hopeful, and exploring tempers, and your teachers on their side will not endeavor to instruct you in dry formulas, but to carry you with us and associate you in the energies of our own minds. You have learnt from what I have said before that you are not coming to learn a science or an art the laws of which are already discovered and laid down. You are coming partly as learners, no doubt, but also in great measure as searchers and pioneers. Slender threads may sew sure stitches, and there is not a man of you here that may not, if he will, add something fresh and original to our storehouse. It has been well said that the neglect of plain and palpable truths is the source of most of the errors with which the world is infested; and nothing tends more to such neglect and to such a closure of eyes to living facts as the spirit of dogmatism and the repetition of written formulas. One great fault that both students and practitioners have to avoid is this tendency to rest comfortable with cut-and-dried opinions. The Empress Catherine the Second said, "How I pity these poor philosophers; they never dare to pronounce those four words, 'I do not know,' which are so useful to us ignorant people." Here, gentlemen, we wish to lay before you the growth of our own minds—to show you the *momenta rationis*, the springs of our knowledge, and the way we make it up and arrive at our conclusions. We wish you to join us, and aid at this continual development, and so to infuse your young minds into our older ones that, while we try to give you promptitude and maturity of judgment, you in your turn will try to stimulate us, and to keep alive in us that flexibility of mind which is open to new thoughts and to new influences. As we get older we are in sad danger of becoming entangled in routine, and every day I am conscious, to my own distress, of the tendency to crystallise, to satisfy myself with such opinions as I have, and to talk as if fixed and positive opinions were good things, whereas, as Arthur Hallam has well pointed out, they are rather the product of an exhausted than of an energetic and growing mind. For my part, then, in the wards, when I do not know a thing, or when I doubt a thing, you shall see my ignorance and share my doubts, so that together we may strive after certainty. This flexibility of mind and openness to new ideas, with the tact which comes with them, are of the highest possible importance to a medical practitioner for the very reason that he treads on such uncertain ground. Perhaps not in one-half of his work has he anything like certainty to go upon, and for the rest, therefore, he must trust to such penetration, such habits of varied and cultivated perception, such finesse in drawing rapid and subtle deductions from small things, as he may possess; and these precious faculties will never be obtained by men who rest contentedly in respectable traditions. There is said to be a plant which the Norman peasants call *Herbe maudite*, which, if a man tread upon it, he is spellbound, and turns round and round in the same place, believing that he is still going forward. A great deal of *herbe maudite* must grow in England, too, gentlemen, I think, from the complacent way in which many persons mistake wandering round and round in one rut for active progress. Do not think, however, that I am opposed to your reading books and to your storing your minds with such knowledge as has been harvested by those who have gone before you. On the contrary, read; but read without subservience to the writer, and read always when you can of such cases as you have at that time under your eye. Practice writing also constantly. To write readily is as hard to learn as to play the fiddle, and to form a clear style requires incessant attention and cultivation; but it has its ample reward in teaching you accuracy. Write from the life as you would paint from nature; thus your eye will be quickened and your memory much strengthened. To read only, and to read of cases you have never seen, is of little worth, for the green grass of forgetfulness will soon grow over the decay of such short-lived knowledge. Help us, then, to send you forth hence, not as Bartle Massey said of college, that "it mostly makes people like bladders—just good for nothing but to hold the stuff as is poured into them,"—but to send you forth able to think and observe for yourselves; not merely well-informed men, but acute, original, and able to put your own characters into all that you do. Yet, gentlemen, all learning, yea all thought, will avail you nothing unless you learn not only to give to your patients your intellects but your hearts also. In the face of the dark mysteries and awful responsibilities amid which you must work, you might well grow faint-hearted were you not conscious that the mounting fiery spirit of man, by its very dealing with these mysteries, makes itself equal with them, and makes good its claim to so high a mission. To quote one of the finest of modern poets:—

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* Mrs. Poyser, in "Adam Bede," is an admirable example of this constitution.

> "PAUSANIAS.—But I will also say, that when the gods
> Visit us, as they do, with sign and plague,
> To know those spells of time that stay their hand
> Were to live free from terror."
> "EMPEDOCLES.—Spells? Mistrust them.
> Mind is the spell which governs earth and heaven.
> Man has a mind with which to plan his safety:
> Know that, and help thyself."
>
> ARNOLD'S *Empedocles.*

To be thus called is the glory of man, but likewise will it return upon him as his curse if he give not his heart in like measure. One of the most bitter reproaches ever hurled at a physician was this: "You have not healed me, you have not comforted me, nor have you brought me consolation." Doctrines have changed, and will change onward, but the claim of man for man's sympathy has always remained the same; and may we ever remember with pride that, more than two thousand years ago, on the shores of the Ægean Sea, there flourished a glorious people, and that the bright Mediterranean sun shone on the face of one of the greatest of them, on the noble face of Hippocrates, the venerable father of our art. And is it not our truest pride in this great ancestor of ours, that, with an intellect as vast and as brilliant as any of those who lived in that glorious age, in him was found not only this, but also a great heart and a deep human compassion which lifted him immeasurably above them all. The founder of Western medicine was not only a profound thinker in the days when Plato and Aristotle taught the people, but he had a lofty sense of the true dignity of his art. Firm and energetic in action, no one had a keener sense of the duties and awful responsibilities of his profession; no one had a more bitter aversion for those unworthy persons who would taint or degrade it by quackery or self-conceit, and most of all no one has spoken more touchingly and with so pious a care for those afflicted fellow-creatures whom he was called to heal or to console. He knew well that the keenest eye may fail when not guided by human sympathy; that no diagnosis, however acute, and no system of treatment, however elaborate, can be perfect unless the heart goes with it. You will be called, gentlemen—many of you are called already,—to minister to the sick and weary; and remember that it is not, for the most part at least, that you will be called to help in cases of romantic trials or of picturesque misery. It is to the bedsides of the common people that you must go—go up dim entries to hear the sullen complaints of worn and listless women, to set the broken leg of a drunken brawler, to soothe the wearing cough of querulous age, or to endeavor to stay the fatal flux of sallow and shrivelling infants. These will be among your duties, and to the beds of these and of plain everyday, prosaic, unlovely people, high or low, rich or poor—to the beds of those who, having never known goodness, have never learned gratitude—must you bear with you, not only knowledge and dexterity, but also that gentle insight into the wounded spirit, that quick recognition of your human kinship with all sufferers, and those words, few and slight perhaps, but spoken without a grain of self in them, which will help the needy, comfort the sorrowful, and bring peace, if not healing, to the afflicted. Believe me, that he who lacks compassion, long-suffering and love for each one of his fellow-creatures, simply because they are each of them members of that human family to which we all belong,—though he have the intellect of a god, it shall be as foolishness; and though he have all knowledge, it shall turn within him to bitterness. And in your own lives, gentlemen, let simplicity and moderation be your guides to happiness. Happiness does not depend upon wordly success, but he is happy who can gather flowers in every pasture, and who does not trample on those simple pleasures which lie at his feet in his feverish yearning for things which are far off. Remember that the envious man waxeth lean, and we are, nowadays, too ambitious for place and money, so that we foolishly throw back all blessings but gold in the face of God. I have a thousand good wishes for you, and a hope for every one of them; but there is no one reward I would so gladly win for you when your work is done as the blessing of cheerfulness and a contented spirit.

Clinical Lecture

ON A

CASE OF FIBRO-PLASTIC TUMOUR

SPRINGING FROM THE SHEATH OF THE POSTERIOR TIBIAL NERVE; AMPUTATION AT THE KNEE-JOINT.

Delivered at University College Hospital.

By Professor Erichsen.

Gentlemen,—I wish to direct your attention to a case of tumour in the leg, in which amputation was performed a few days ago. The case is interesting both as regards the nature of the tumour and the principles of practice involved in the treatment.

G. G——, aged thirty-two, gardener, was admitted on December 28th. He stated that his general health was good. Sixteen years ago he sustained a fall; he says there was no fracture. Since then a small, sharp, hard tumour came on the inner side of the head of the right tibia. Four or five years ago the lower part of his right leg and the sole of his foot became painful and tender, with shooting pains. About two and a half or three years ago the limb began to enlarge very slowly. Two years ago, at St. George's Hospital, a grooved needle was inserted. His general health has not suffered, but he has felt nervous. His work has not been laborious. The limb has enlarged a little more rapidly of late, he thinks, and has become very tender. The circumference of the limb over the tumour is eleven inches, that of the other leg being ten inches. It is very tender, but there is no redness or œdema. The tumour seems continuous with the inner edge of the tibia; it is of firm consistence. The swelling of the calf ceases abruptly, and just below it is the tumour. The patient limps in walking. At night there is pain, especially after walking. About an inch and a quarter from the inner edge of the tibia a large artery is felt pulsating; it runs in the axis of the limb nearly parallel to the tibia, but at the lower part of the tumour, with a slight inclination towards that bone. On compressing this artery strongly pulsation in the posterior tibial at the ankle is arrested. There are no large veins over the tumour; it is elastic, almost deeply fluctuating, and very sensitive when touched. On pressing behind the pulsating vessel the patient complains of very considerable pain.

Dec. 31st.—Occasionally he feels an increase of the shooting pain, followed and relieved by sweating of the foot. A liniment consisting of extract of belladonna (one drachm) and glycerine (two drachms) was ordered.

Jan. 4th.—The patient being placed under chloroform, an exploratory incision was made to the inner side of and parallel with the tibia. The tumour was found to be solid, and with such deep attachments that interference with it was impossible except by amputation. This was deferred, the patient not having been prepared for it. The wound was dressed antiseptically.

During the next few days he was feverish, with some vomiting. An ounce of the bismuth mixture used in this hospital was ordered to be taken when the sickness came on, and one minim of ipecacuanha wine in water every hour while the sickness lasted.

9th.—Yesterday his temperature was 103·2°. He felt very hot, with headache and no appetite. Epistaxis came on. Last night he slept, following a morphia injection. His tongue is covered with a brown fur, and is very dry, Temperature 102·2°; pulse 104. The leg and foot are very œdematous. There is no discharge from the wound, which shows some granulations. A purgative and saline ordered.

11th.—No more sickness. Tongue brown and rough, except at the edges and tip. Temperature 103°; pulse 100. Ordered tincture of aconite, one minim every half-hour for three hours, and then every hour for three hours.

12th.—There is less feverishness; tongue less brown. Temperature 100·6°; pulse 104.

14th.—Patient feels much better. Temperature 99·5°. The tongue is less coated.

16th.—There is some suppuration from the wound, not apparently deep; also a little œdema of foot.

17th.—Still a small amount of suppuration. The antiseptic dressing discontinued.

19th.—There is a continuation of the discharge, coming from the deeper parts. There is also some swelling and fluctuation on the outer side of the leg near the ankle. Mr. Bell, the house-surgeon, made a transverse incision just behind the ankle, and let out a good deal of pus.

The suppuration continued for the next few days, diminishing on the 23rd. The patient was told that nothing could be done for his leg except amputation.

25th.—The patient being under chloroform, I amputated at the knee-joint by a long anterior and short posterior flap. The patella was preserved in the anterior flap. The hæmorrhage was not great. About twenty vessels were ligatured. The edges of the flaps were brought together by nine silver-wire sutures, but the lowest part of the wound was left open for draining. The wound was well syringed out with carbolised water, and then lint soaked in it applied over the whole. Pieces of strapping were placed transversely above the patella to keep it down. Before operating, I found a small abscess on the outer side of the leg at the flexure of the knee; it was opened and found to be quite local.

27th.—Tongue dry; thirst; nausea; redness and tenderness over surface of anterior flap. Water dressing was ordered, and a mixture composed of five grains of citrate of iron, twenty grains of bicarbonate of potash, five grains of carbonate of ammonia, and ten minims of spirit of chloroform, in an ounce and a half of water, to be taken every six hours, with fifteen grains of citric acid.

28th.—The redness and tenderness slightly diminished. There was slight suppuration. He vomited last night. He has less nausea this morning, and much less thirst. The bowels have not been open for four days.

30th.—At 9·30 A.M. there was hæmorrhage from the stump; the application of a tourniquet stopped it, but it recurred; the tourniquet was tightened, but the flow of blood persisted, and altogether about eight ounces were lost. At 10.30 A.M., the patient being under chloroform, the sutures were removed, the flaps cleansed of clots, and two bleeding points tied, one being apparently the popliteal artery. A quantity of pus was evacuated from the front of the femur, and it was found to burrow up the thigh. A probe was passed up, and a large counter-opening made down to it. The flaps were sponged with carbolised water, and the sides brought together by wire sutures, the most dependent part being left open. He sweated profusely afterwards, and his pulse was almost imperceptible. His bowels have not been opened yet.

From this time the progress of the patient towards recovery was satisfactory, although slow. I need not trouble you with the details, but suffice it to say that he left the hospital well in about two months.

The history of this case, then was briefly this:—About four years ago the man began to suffer pain in the leg. There was not much inconvenience till two years and a half ago, when there was evidence of a tumour. When admitted there was a tumour occupying the posterior, and lower half of the leg below the calf.

The important point was to effect the diagnosis of the tumour. And this had to be done in two directions—first, as regards its anatomical relations; and, secondly, as to its nature, in order to determine the course to be pursued. It was obviously obscure in both these respects. A grooved needle had very properly been introduced some two years previously, but this had revealed nothing. The tumour, therefore, was probably solid. It felt exceedingly elastic, so as almost, but not quite, to give a sensation of fluctuation. It was like a collection of very thick fluid enclosed in a dense cyst, or a loosely structured and infiltrated tumour, elastic from this infiltration, and almost giving a sensation of undulation. It was evidently simple, not malignant. It had grown slowly for two or three years, and had not yet attained any very con siderable bulk. The tumour, then, was probably either a soft solid or a thick fluid. It was certainly of a simple character.

The next point was to determine its relations. On examining the tumour attentively, a large artery was seen passing over it, one inch to the posterior side, and parallel to the inner edge of the tibia. This artery, from its size, force of pulsations, and from the fact that pressure on it arrested pulsation at the back of the ankle, was the posterior tibial. Behind this, much pain was elicited on pressure, running down the limb and into the foot, but chiefly at the point compressed. This was evidently due to pressure on the posterior tibial nerve. It was evident that both artery and nerve had been displaced from the middle to the inner side of the limb, being thrust inwards and rendered subcutaneous by the tumour's growth in that direction. These circumstances throw much light on the deep relations of the tumour. It was evident that it must have grown, not only deeply beneath these vessels, so as to have carried them towards the surface, but also that it grew from the outer side of the limb. It is plain, therefore, that the tumour was more connected with the fibular than the tibial aspect of the limb. On examining the limb on the outside, no enlargement could be perceived there, and the outline of the fibula was clean and smooth. It was obvious, therefore, that if the tumour grew from bone, it grew rather from the tibial side of the fibula than from the other side of it. But it was, to say the least, doubtful if it grew from bone at all. I said in the ward that it felt to me much like a tumour I had sometimes felt in connexion with the deep muscles or tendons; originating, perhaps, as a ganglion, but eventually becoming malignant. It was more like one of those than anything else. I supposed that it had grown from one of the deep muscles.

To clear up the nature of the tumour, I determined to make an exploratory incision. A little more than three weeks ago I cut down upon the back part of the tumour, behind the structure that I conceived to be the posterior tibial nerve, parallel to the inner edge of the gastrocnemius. The finger could be passed for some considerable extent behind the tumour, but the growth could not be enucleated. I could not make out its exact relations, which were very close to, and apparently inseparable from, the nerve and artery; so I left it, closing the wound with carbolised dressings. An abscess formed on the outer side of the limb, in consequence of some subcutaneous infiltration. It was evident that we had to deal with a slowly growing tumour in the lower third of the leg; but it was doubtful if it was connected with bone. As the patient's sufferings were severe and continuous, as he was utterly incapacitated for work and had been so for many months, and as the tumour could not be separated from the artery and nerve so as to be enucleated, I thought it wiser to amputate.

In discussing the kind of amputation I preferred amputation through the knee, rather than through the tibia and fibula. And why? For two reasons. First, although I thought it very doubtful that the tumour was connected with the fibula, I could not be certain that it was not. I was anxious not to leave a portion of a bone connected with a morbid growth, for I consider it to be unwise to amputate through a bone from which a tumour springs; there is apt to be infiltration of the medullary structure; and, if so, the disease will speedily recur in the sawn end of the bone in the stump. Secondly, supposing that the tumour did not grow from the bone, but from muscle, it has been shown by Mr. Teevan that it is of importance to remove the whole of the muscle from which the tumour springs. If not, there will be infiltration of morbid matter in the belly of the muscle, and there will be speedy recurrence. In fact, I was anxious to be sure of going clear above the bones and the deep flexor muscles of the leg. One must look to the future as well as to the present, and, above all, avoid the chance of early recurrence. Amputation was accordingly practised through the knee, in the manner to be presently described.

After the operation the limb was carefully dissected by Mr. Beck. The tumour lay imbedded in the long flexor of the great toe. This was expanded over it like a capsule. The tumour was about the size of a large goose's egg (three to four inches by two and a half). On prosecuting the dissection, it was found to spring from the sheath of the posterior tibial nerve, and that this sheath was completely involved in it for about three inches on the fibular aspect of the nerve. The posterior tibial artery was tightly stretched over the tumour, and the sheath of the vessels was so closely incorporated with the tumour that the artery was unavoidably torn across in the dissection. The nerve was enlarged and rounded all the way up the limb. Here, then, was a tumour springing from the posterior tibial nerve, with the flexor longus pollicis expanded over it so as to encapsule it. It could not have been removed. An attempt at extirpation would have led to division of the artery and removal of several inches of the nerve, so that the the limb would have been rendered paralytic and useless. Amputation, therefore, was the proper course to pursue.

The tumour was quite smooth, and scarcely lobed on the surface. On cutting into it, it was seen to be of a bluish-grey color, smooth, uniform, semitransparent, œdematous, and infiltrated to a certain extent. Under the microscope it had all the characters of a rapidly growing fibro-plastic tumour. It was carefully examined by Mr. Beck. The cellular predominated over the fibrous elements, showing that it was rapidly growing at the time of removal. It was much the same kind as that very large one I removed from a woman's axilla some time ago. The growth is an extremely interesting specimen of a large fibro-plastic tumour growing from the sheath of a large nerve. Separation of the tumour from the nerve itself after removal of the limb was impossible, though no fibrils of the nerve were actually involved.

And now for a very few words about the operation itself.

Amputation through the knee-joint is one of those operations which has seen much fluctuation in surgical estimation. It is one of those operations that, although there may be many *a priori* arguments in its favor, has some practical difficulties attendant upon it, which render the propriety of its employment sometimes doubtful. In this instance I chose it so that I might remove all the bones possibly implicated.

In amputating through the knee-joint you have a choice of three methods: 1st, by a long anterior and a short posterior flap; 2nd, by a long posterior and a short anterior flap; 3rd, by lateral flaps. As a general rule, the operation by the long posterior and short anterior flaps is not a good one. You have a large mass of fleshy substance which has to be brought up over the condyles, and kept there contrary to gravity, tending all the while to fall away. The operation by lateral flaps has been introduced of late years by an American surgeon of the name of Smith. I have had no experience of this method, which is spoken well of by the author. There is much tissue available for the purpose at the sides of the joint. I discarded both these, for, independently of the reasons just given, I wanted to remove all the affected muscles possible.

In performing this amputation, proceed in the following way:—Enter the point of a short broad-bladed amputating knife to one side of the joint: the fibular, if it be a left leg; the tibial, if it be a right. Carry the knife downwards for about four or four and a half inches by the side of the fibula or tibia. Then bring it straight across the limb, just rounding the angles, and carry it to the corresponding part on the opposite side. Dissect up the flap, turning up the patella with it. Open the anterior part of the joint. Bend the joint forcibly. Divide the lateral and then the crucial ligaments. Put the knife behind the limb. With a determined sweep of the knife cut inwards to the bone, making a short posterior flap. You then detach the limb readily at the knee-joint. The only large vessel to tie is the popliteal. You have several articular branches, which may be enlarged, but are readily tied. Then lay down the flap over the exposed surface of the condyles, and attach the flaps together.

There is one question of practical interest in this operation. It is this: Should you leave the patella in the anterior flap or not? I have performed the operation both ways, and I think that there is an inconvenience in both. The method to be adopted in any case must be a compromise of these inconveniences. If you dissect out the patella, you thin the flap so much in the middle that it is apt to slough. I have seen this happen more than once when the patella has been dissected out with all care. If you leave the patella, what will happen? The circumstance that has occurred in this case. You are apt to get an accumulation of inflammatory and synovial fluid above the patella, from the large synovial surface there, which is apt to run into pus, forming a large abscess above the patella, under the extensor muscles of the thigh, and which, as in this case, will require an incision for its evacuation. Of the two, the inconvenience of the latter is the least; and I therefore prefer leaving the patella. It might, perhaps, be prevented by what I should adopt in another case of this sort—viz., by introducing a drainage-tube into the synovial sac above the patella, and bringing it out between the flaps. What is the advantage of leaving the patella? Is there any positive advantage in doing so?—anything beyond the negative advantage of not thinning the flap? I think there is, and it is this: you get after the operation a more solid stump. The retention of the patella gives the patient a natural knee to rest upon. You get for this purpose a portion of the body intended by nature to bear pressure, and on this the pressure is more conveniently carried.

There is yet another question in these amputations at the knee to which I will briefly allude. Should you leave the cartilaginous surfaces of the bones or remove them? Some surgeons practice one, some the other plan. I think it does not signify much which you adopt. I have sawn off the articular surfaces both of patella and condyles, and thought that some advantage was gained in readiness of union. But whatever advantage is thus gained by bringing cut osseous surfaces together, there is also one disadvantage—viz., that you open the cancellous structure of the bone, and so there is risk of osteo-pyelitis. Upon the whole, therefore, I think it is more advantageous to leave the cartilaginous surfaces than to subject the patient to the increased risk of pyæmia by removing them.

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Clinical Lecture

ON THE

SYMPTOMS IN WOMEN WHICH POINT TO THE EXPEDIENCY OF INSTITUTING LOCAL EXAMINATION.

By Robert Barnes, M.D. Lond.,

Obstetric Physician to St. Thomas's Hospital, and Examiner in Midwifery and the Diseases of Women to the University of London and the Royal College of Surgeons.

Disorder of function—Distant and constitutional reactions—The subjective signs of local disease indicate appeal to objective signs—Comparison of study of disease of pelvic organs to that of skin and eye—Disturbance of functions of ovaries, uterus, and vagina—Amenorrhœa, real and occult; Menorrhagia; Dysmenorrhœa—Dyspareunia—Retention of urine—Sterility—Abortion—Discharges, sanguineous, mucous, purulent, albuminous, watery, fleshy, membranous—Pain, lumbodorsal, inguinal, pelvic.

There is nothing special in the mode of studying the diseases of women. Just as the ophthalmic surgeon is led to examine the eye because the patient complains of loss or disturbance of its function, or because he feels pain in it, or has some other subjective symptom referred to that organ, so by disturbances of function or some other subjective sign are we led to the discovery of disease of the sexual organs. When the function of an organ is disturbed, the *primâ facie* inference is that the organ itself which constitutes the mechanism by which that function is performed is out of gear. This is not indeed always absolutely true; because an impaired state of the blood, or disordered innervation, or derangement of a different organ, may entail the functional disorder which arrests our attention. The genital organs are no exception to this proposition. The functions of the ovaries, uterus, or vagina may be seriously deranged by a state of anæmia or blood-poisoning, by disease of the nervous centres, by disease of the heart, lungs, or liver. These functions may be even more seriously affected by mechanical pressure in contiguous parts. Still the fact remains that we can hardly appreciate rightly or successfully treat these primary or correlated diseases if we do not take into careful consideration the state of the genital organs themselves. The general or distant affections require to be investigated and treated; but it is not safe to overlook the organs that may be secondarily involved.

It is needless to say that every woman who is ill and seeks advice does not suffer from disorder of the sexual system. She may labor under various constitutional disorders, and under disorders of parts of the body quite independent of the sexual system. On the other hand, general or local disorders may in their course react upon, and induce disorder in, the sexual system. And there are disorders of this special system, commencing in it, and in their turn reacting upon, and inducing disorder in, distant organs or in the general system.

These inter-reactions are exceedingly frequent. Indeed, it may be affirmed that no severe constitutional disorder can long continue in a woman during the predominance of the ovarian function without entailing disturbance in this function. And the converse is also true, that disorder of the sexual organs cannot long continue without entailing constitutional disorder, or injuriously affecting the condition of other organs.

These facts point to the necessity of guarding against the error of fixing our attention too specially upon one particular class of symptoms or upon one organ. Whilst searching out the part which is more especially the seat of diseased action, we must be careful not to overlook possible disease elsewhere, and not neglect to observe the mutual reactions. The clinical physician, although led by the intuition of experience to seize quickly upon the offending part, will not omit to pass under review the state and working order of the other parts. In this manner most important complications are often brought most unexpectedly to light; and in every case some useful indication in treatment is discovered. The late Professor Chomel, a man of admirable skill, sagacity, and judgment, never failed, when a new case of disease came under his care, to interrogate successively every function. Thus, I have seen him in a case of pneumonia, the signs of which at once arrested attention, proceed nevertheless to explore the abdomen, the uterus, and the rectum. This may look like carrying out a principle to extremes. Yet who shall say that Chomel, as a clinical teacher or as a physician, was wrong?

It is not, indeed, necessary, in ordinary practice, to follow out in rigorous completeness the plan which to the clinical professor may seem desirable. It will therefore be useful to ascertain *what are the leading symptoms which, alone or grouped, indicate such disorder of the sexual organs as to call for direct exploration.*

This is the question we have set before us: When a woman presents herself, complaining of certain symptoms, chiefly subjective, some, or perhaps none, referred to the pelvis, how are we to act? Will these subjective symptoms enable us to refer them to their cause, to establish a diagnosis, to give satisfactory indications for treatment? Hardly. We must therefore call to our aid the objective signs; we must weigh and determine the significance of these before we can arrive at a conclusion at all precise or trustworthy as to the underlying pathological condition. The whole tendency of modern medicine is to subject every organ which manifests functional disorder to direct physical exploration, in order that it may solve the question presented obscurely by the subjective signs. The sound, the probe, the stethoscope, the laryngoscope, the otoscope, the ophthalmoscope, the various forms of speculum, are only so many contrivances for enabling us to project or extend the senses of touch, sight, and hearing into the internal structures. In the case of the skin, all is at once exposed to direct observation; and, as Alibert remarked, we should be glad to have the same advantage in investigating and treating the diseases of the heart, lungs, liver, kidneys, and nervous centres. Why is it that the study of the pathology of the skin and of the eye is invested with such facinating interest? Those who devote themselves with the greatest zeal and success to this study will tell you that it is because the skin and the eye reveal their condition directly to the senses, and thus furnish not only positive objective signs which the patient can neither suppress nor misrepresent, but also because in this direct observation of the skin and eye they can read and follow as on a map or on a telegraphic dial the working of distant organs and of many affections of the general system. Here, then, you see how the reputed special practitioner, turning to account his special experience, often acquires an insight into general pathology denied to those who neglect the lessons they might read upon the visible organs.

This advantage we possess to a great extent of perfection in the case of the pelvic organs. It is by the proper use of this advantage that so great a degree of precision in knowledge, and of success in treatment of diseases of women, has of late years been attained.

And there is one property in a high degree characteristic of the instruments employed in the investigation of the diseases of women of such singular value that it ought to completely silence the objections at one time so passionately urged against them. It is this: the instruments have a therapeutical as well as a diagnostic application; the speculum, for instance, revealing a lesion of the cervix uteri, enables the surgeon at once to apply his remedy. Thus treatment follows upon the track of diagnosis, one sitting and one operation serving for both.

Here then, as in medicine generally, our first indication of the direction in which we have to look for the disease which causes the patient to complain, lies in the disturbance of function. We have then to consider what these functions are. The first in importance, because it is continued with occasional interruptions throughout the period of active sexual life, is *menstruation.* The other functions are incidental to married life only; these are *the relation to the other sex, pregnancy* and *lactation.*

Most of the diseases which attack the ovaries and uterus, whether primary or secondary, entail some disturbance in the menstrual function. The flow is diminished or in excess, or its periodicity is deranged. It is attended with pain in the pelvic organs and other nervous phenomena.

We shall discuss the history of amenorrhœa, menorrhagia, and dysmenorrhœa hereafter. Our object now is simply to determine the conditions which suggest local examination. In the great majority of cases of *amenorrhœa* in single women, no local exploration is necessary; but in some cases it becomes imperative: for example, amenorrhœa is sometimes presumptive only—that is, the secretion takes place, but owing to some imperfection of structure it is retained in the cavity of the uterus or vagina. This may be called *occult menstruation.* The suffering becomes urgent in the highest degree, and nothing short of an operation which shall liberate the retained secretion will save the patient. Some cases again of suppressed menstruation, leading to effusion of blood behind the uterus, setting up circumscribed peritonitis, and displacing the uterus so as to press upon the bladder, may cause retention of urine. Here again local examination is imperative. This may be said of every case of retention of urine. In almost every case of retention of urine in women the cause is external to the bladder, and in the great majority it is due to some disease or displacement of the uterus.

Menorrhagia is a relative term; that is, some women lose much more than the average without suffering in health; but whenever the loss continues profuse, obviously entails anæmia and general debility, and persists in spite of internal remedies, local examination is clearly necessary. We shall often find a sufficient local cause in polypus, tumour, inflammation, congestion, hypertrophy, displacement, or malignant disease, all of which conditions require local treatment.

When we come to study the history of *dysmenorrhœa* we shall find abundant proof of the almost constant association of this disorder with a mechanical condition of the uterus impeding the easy performance of the function. So long, however, as the distress does not clearly affect the general system, so long as it does not exceed endurable bounds, and if it appears to be moderated by general remedies, it is not necessary to examine; but, in the contrary event, examination should not be long postponed. To postpone examination is to postpone discovery of the cause and effective treatment. This is more especially imperative in the case of a married woman in whom dysmenorrhœa is complicated with dyspareunia and sterility. Abortion, if not primarily depending upon some local disease or displacement of the uterus, is so very likely to be followed by some such condition that an examination should be instituted. If a sanguineous discharge, even periodical, resembling menstruation, goes on during lactation, especially if it be excessive in quantity and attended by leucorrhœal discharge, it may be almost confidently predicated that there exists some uterine disorder requiring local treatment.

I have used a new word, "*Dyspareunia.*" It is incumbent upon everyone who coins a new word to explain its meaning and to justify the innovation. Just as the word "dysmenorrhœa" has been coined in order to express compendiously the condition of difficult or painful menstruation; just as "dyspepsia" is used to signify difficult or painful digestion, —we want a word to express the condition of difficult or painful performance of the sexual function. Such a word would be convenient in many ways. It would enable us to avoid the longer and coarser forms in use, by substituting a single word at once euphonious, expressive, and in harmony with medical language. After consulting with my colleague, Dr. W. H. Stone, whose high classical attainments give authority to his advice, I have determined to adopt the word "dyspareunia." It is derived from δυσπάρευνος, a word used in this sense by Sophocles. However disagreeable the topic may be, it is impossible to escape reference to a function so important. Dyspareunia in the female is perhaps the most absolute of all the indications of local malformation or disease. It calls the most imperatively for local examination as to its cause. In its milder forms it may make the sufferer's life a course of physical and mental wretchedness; in its severe forms it virtually unsexes her; and in any form it may lead to the most disastrous social calamities.

Taking this condition, dyspareunia, as a symptom of disordered function, we shall be astonished, when we proceed to direct examination of the organs concerned, at finding how many those causes may be, and what a wide field of pathological inquiry is associated with it. For example, there may be original defect or malformation; there may be obstructing tumours or growths, inflammation, dislocation or altered form, disordered innervation,—in short, almost every disease to which the sexual organs are liable may entail dyspareunia for one of its consequences; and in not a few of these diseases disregard of this symptom may entail positive danger.

The existence of *certain discharges*, such as blood, under conditions of quantity and times of occurrence which distinguish it from normal menstruation, mucus, pus, albuminous, aqueous, fleshy, or membranous, if at all protracted, point clearly to some local disorder as their origin which requires direct exploration.

Then there are some subjective signs, as pain, lumbo-dorsal, iliac, pelvic, or crural, and a sense of bearing down or pressure upon the rectum or bladder, entailing disorder in the function of these organs. These, especially if connected with abnormal discharges and other symptoms, call distinctly for local investigation.

Then we must observe the constitutional or remote effects of the foregoing conditions. Disorder of the pelvic organs seldom goes on long without entailing anæmia, disordered digestion, hyperæsthesia, neuralgia, or other manifestations of nervous derangement or prostration. Where these conditions are observed in association with marked signs of derangement of function of the pelvic organs, the necessity for exploring the physical state of these is as clear as is that of examining the state of the heart or lungs when these organs perform their function with distress, and the whole system suffers.

Such, then, is a summary view of the conditions, chiefly subjective, which point out to us the desirableness of instituting direct observation of the pelvic organs. This direct observation commonly enables us to analyse the groups of subjective symptoms; to determine the cause and significance of each, separately and collectively. It always brings to our assistance the discovery of other symptoms, entirely objective; and almost always puts it in our power to apply the proper treatment.

The study of the significance of the several symptoms of pelvic disease, subjective and objective, we shall enter upon on a future occasion.

Original Papers.

ON EXCISION OF THE KNEE-JOINT AND THE CONDITION OF REST NECESSARY TO BE MAINTAINED.

By Wm. Knight Treves, F.R.C.S. Exam.,

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There is, perhaps, scarcely any operation whose merits have been so differently estimated by various surgeons, or which has been so lauded on the one hand and so depreciated on the other, as excision of the knee-joint. Whilst some surgeons have the highest opinion of the operation, and almost invariably resort to it in cases of incurable knee-joint disease, others look on excision with coldness and mistrust, and, if they do not discard it altogether, resort to it only at the last extremity, and, as it were, under protest.

Nor is this wide difference in the estimation in which the operation is held difficult to account for; on the contrary, it is probable that these opposite views are justified by experience, and that the opinion of any individual surgeon is drawn from observation on the result of his own cases or those of his friends. Now, this is what I wish to indicate—namely, that in actual fact the results of the operation are, in different hands and under different circumstances, as totally opposite as are the opinions which are held of its value. One operator has good results and an almost uniform success; another, of perhaps equal skill and reputation, is just as unfortunate in his cases. This statement can be confirmed by anyone who will investigate the literature of the subject.

The operation itself being easy of execution and presenting no difficulties, it may readily be allowed that for all practical purposes it is as well performed by one surgeon as another. Also that, although the selection of suitable cases is of the utmost importance, it may fairly be presumed that the class of surgeons who usually excise joints are not likely to select improper cases, and that by far the greater majority of those excised are fit and proper ones for the operation.

What, then, is the cause of this difference in results in the hands of different operators? The answer is, that, although each surgeon may operate in much the same way, the same scrupulous care is not shown by all in preparing and applying the apparatus for ensuring rest, nor are equal trouble and pains taken by every surgeon in personally attending to the minutiæ of the after-treatment. I think I may assume as an axiom that the best after-treatment is that which ensures the most perfect rest, and that if an apparatus could be applied which would effectually prevent the sawn surfaces from receiving *even the slightest jar* from the time of the operation until union had taken place, this would be a perfect plan of treatment. Also I may fairly take for granted the converse of this—namely, that any plan of treatment which does not ensure absolute and perfect rest, with freedom from even the slightest occasional jar, during the entire period in which the bones are uniting, is not perfect; and that the further the treatment fails to produce this effect the more imperfect is that treatment. And still more, that any treatment which entails necessarily daily jarring of the sawn surfaces, and perhaps the removal and reapplication of the apparatus during the period of union, must be very imperfect indeed, and must militate much against the prospect of recovery with a useful limb.

I trust I am keeping within bounds when I express my belief that this perfect rest is not maintained in the great majority of cases operated on, that it is kept up by some surgeons more than by others, and that in proportion to the extent to which this principle of perfect rest is observed so is the success.

It is my belief that any movement or jar which causes even slight pain is injurious to the successful progress of the case, and that occasional jars, producing some amount of concussion of the bony surfaces, are in many instances the cause of unfavorable termination; and that, although some cases may and do come to a successful issue while subject to and in spite of these drawbacks, yet that this want of perfect rest is the main cause which operates against recovery. Thus too great care cannot be taken to prevent such movement. I have remarked cases very fairly put up, and which have afterwards done well, shrink on an approach to their bed or a heavy step across the ward; so that the pain and injury which must be caused if the splint has to be moved, or the limb otherwise jarred, should lead us to adopt the strictest precautions.

The dangers which imperil the perfect rest which it is desirable to secure are twofold: thus, they arise, firstly, during the performance of the operation and the application of the splint; and, secondly, after the patient's removal to bed and during the after-treatment. I will, as briefly as possible, enumerate what are some of the chief of these dangers, trusting that the points I may mention will not be considered trifling. Success in this operation depends more perhaps than in most others on the strict observance of those little precautions and cares which may appear to some as beneath their notice, and rather belong to the province of the dresser or nurse.

Causes which arise during and immediatelg after the operation tending to produce injury of the surfaces.

1st. Injudicious sponging—excessive or too forcible sponging, which is likely to irritate and bruise the bony structure.

2nd. Too powerful use of the saw, or employment of the gouge to remove bone which may appear diseased after the surfaces have been sawn.

3rd. Closing of the wound before all hæmorrhage has ceased, and omission to secure all the small vessels.

4th. Want of care in the dresser or other person holding the limb after the excision while the splint is being applied or hæmorrhage arrested, whereby, from proper extension not being maintained, or other cause, the sawn bones are allowed to press or knock slightly against each other and the cancellous structure to be bruised.

5th. In cases where there has been some dislocation and consequent contraction of the flexors of the thigh, insufficient removal of bone, so that the sawn surfaces are drawn and pressed against each other by the shortened and contracted muscles.

6th. Incision not sufficiently dependent at the sides, so that bagging of matter may possibly take place, requiring subsequent opening and exposure to the risk of escape of matter into the splint.

7th. The flowing of blood into the splint at the time of its application, causing at a later period putrefaction and smell, and entailing the changing or purifying of the splint.

8th. In cases where a metal or other splint is applied, want of foresight and care in the padding and application of the splint, so that subsequent alteration or shifting is required, entailing a certain amount of movement. Thus, want of regard in making the splint comfortable to the patiant and securing him against undue pressure on any particular spot, or in providing carefully against the tendency of the bones to become displaced in certain directions.

9th. In cases where sinuses exist either towards the posterior aspect of the limb or some little distance up the thigh or down the leg, the neglect of proper precautions to ensure that the discharge from the sinus or sinuses is not blocked or interfered with by the apparatus applied; also that there is no possibility of the matter finding its way into the splint, and thus causing foulness of the splint and necessitating its removal.

The above are some of the chief little mishaps and drawbacks which may occur during or immediately after the operation. They for the most part suggest their own remedy. The matter of keeping the splint clean is very simple, and only requires a little care. I have usually kept a piece of lint thickly spread with zinc ointment applied to each side of the leg below the incision. This dressing sticks closely to the limb as far as the narrow part of the splint, and is then turned outwards to the porringer, forming a sort of gutter; the blood or discharge will run over the zinc dressing, and is entirely prevented from getting into the splint.

With regard to the second point—namely, the dangers which imperil the patient's perfect rest after his removal to bed and during the after-treatment,—I think it is more than probable that many hospital surgeons are quite unaware of the interruptions their cases receive after leaving the table and remaining from visit to visit under the care of the dresser or house-surgeon. The dresser likes his case to look clean and trim for his surgeon, and reapplies bandages; the nurses change sheets; and between the two the patient may get rather more attention than is good for him. Take the case of a female patient whose leg has been put up in an ordinary excision splint (Fergusson's): the extension piece is not long enough to be of much service; and there is only room between the excised joint and as near the vulva as one can conveniently apply dressings for a few turns of bandage, which must not be too tight, on account of some little swelling which is likely to take place. Thus secured, the patient is removed by the porters, and she will be fortunate if the leg does not receive a fair amount of shaking in getting her to bed. We will suppose, as entailing the least amount of jarring on movement, that the leg is swung. Now whenever this patient uses the bed-pan, or has draw-sheets or other sheets changed—and of course these are daily occurrences,—the bones are jarred. And necessarily so, for the following among other reasons:—Every time the patient's buttocks are raised, the weight of the iron excision-splint falls to a great extent on the upper surface of the thigh by means of the bandage confining the thigh to the splint; the remaining weight of the splint resting on the swing at the foot or leg. Of course a careful nurse may raise the thigh-piece of the splint at the same time as the buttocks by the tape connecting it to the cradle; but nurses are scarcely likely to take this precaution, and if they do they will probably effect more harm than good from the difficulty of following the movement of the patient; besides, the raising of the splint and patient would be scarcely likely to be simultaneous. There is, unless guarded against, a drawback in the use of these splints when swung—which is, that, the foot being raised, the weight of the splint acts in pressing towards the hip, and as this is a fixed point, the weight is exercised as a matter of course in pressing the sawn surfaces together.

I hope I have been able to point out some of the disadvantages under which cases of excision are often placed.

In published reports of cases it should particularly be mentioned at what date the splint was first removed or changed. From observation and inquiry I am inclined to believe that, in a considerable proportion of cases of excision, the splint is changed within or about three weeks from the operation, on account of soiling of the splint. If this be true, we cannot be surprised if excisions often do not turn out satisfactorily. At the end of three weeks repair is taking place, but union can scarcely be said to have commenced; yet often at this time, if not earlier, the bones are moved, and a certain amount of inflammatory action is not unfrequently set up.

These dangers, then, have to be guarded against by—

1st. Taking the necessary precautions at the time of operation to ensure perfect rest, and to provide against any soiling of the apparatus or shifting of the sawn surfaces, either of which would entail moving the limb before the cure is complete, and danger to the limb.

2nd. Supposing every precaution to be taken at the time of operation, it will be necessary afterwards to guard against any movement which might be caused by changing of bandages, dressings, or splint by the dresser; and, what is still more essential, nurses must not be allowed to shift sheets, or in any way disturb the patient, except under the personal supervision of the dresser or house-surgeon.

Important among the precautionary measures to be taken to prevent movement is careful padding of the splint. This duty is usually left to nurses, and very often indifferently executed by them. The splint must afford firm, even support; therefore nothing soft, of the nature of cotton-wool or tow, should be used. The best plan is to take an old blanket and cut it into shapes the size of the thigh and leg pieces of the splint. Then take as many pieces as may be necessary to afford the desired thickness, have them quilted firmly together and covered with linen, and then strained tightly to the splint so as to fit its hollows. In padding the splint it is well to bear in mind what we have to guard against. Thus the pad should be thicker under the head of the tibia than under the thigh or further down the leg, and this is better provided for by quilting in extra layers of blanket in this part than by pushing in a pad at the time of operation or other makeshift contrivance. We must also provide in the padding for a full drop of the heel, which will not bear any permanent pressure. By putting additional padding under the head of the tibia, and allowing the heel to drop, we entirely avoid what is the most common displacement after the operation—namely, slipping of the tibia backwards behind the femur, or, what is in effect much the same thing, displacement of the femur forwards over the tibia. Over the narrow part of the splint it is best to use a movable pad of the same character as the fixed portion and covered with zinc dressing, so that, if unfortunately it should become soiled, another could be sewn to it and drawn under without disturbing the limb. In bandaging the limb to the splint it is well to bear in mind that it is not a matter of indifference whether the bandage be applied in one uniform manner—viz., from left to right or from right to left, as the dresser may be disposed; on the contrary, a bandage applied, say, from right to left presses more particularly in that direction, especially at the moment of application, so that it is possible the bones may be slightly disturbed in their relation to each other during the bandaging. Recollecting the tendency of the femur to rotate outwards in excision, we shall do well to bandage over this bone from without inwards; while over the tibia it is better to bandage from within outwards.

The apparatus to be applied depends of course on the individual preference of each operator. I am inclined to think that, for adults, Fergusson's excision splint, with an extension piece longer than usual and reaching to the axilla,* answers very well. There are objections to this splint, such as its weight and the fixed position it entails on the patient; but it has this advantage—viz., that in case there should be any tendency to displacement, the surgeon can exercise a good deal of control over the relation of the two bones to each other without disturbing the limb. The measures I have adopted in using this splint are the following:—

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* I have lately seen, in one of the Half-yearly Retrospects, an abstract of a paper by Mr. Gant, in which he recommends that the extension-piece should be made to reach the axilla. The advantages accruing from this alteration are so obvious that the same idea will probably have occurred to more than one surgeon. I have used the long extension-piece since the beginning of 1866, and there are now in the hospital here some splints with this adjunct, which I had made in that year whilst resident surgeon to the hospital.

1st. To fix it well at the axilla by a band made precisely like an ordinary perineal band. This is passed over the front of the opposite shoulder, then behind the back, and tied to the top of the extension piece, thus keeping the latter firmly fixed to the armpit.

2nd. To secure it by a wider band, which, being strapped firmly round the hips, steadies the apparatus at this point, and prevents jarring in the case of any attempted movement of the buttocks.

3rd. Having the limb swung sufficiently to clear the bed, I have affixed a weight working over a pulley to the foot-piece heavy enough to counteract the pressure of the splint towards the hip. Moreover, if the knee be painful from the bones being pressed together by contraction of the muscles, the weight may be increased and made to exercise as much extension as may be necessary to overcome such contraction and consequent pain; to do this, loosen the thigh bandage, and put on as much weight as may be required to afford ease.

4th. There is occasionally after the operation some starting and jumping of the muscles of the thigh. This is usually relieved by a nurse keeping her hand gently on the thigh; but what is more effectual for the purpose is an oblong bag of sand, —the length of the bag and the quantity of sand in it being so arranged that when the bag is applied over the thigh the material of the bag lies smoothly on the front of the limb, while the sand, distributed as required at both ends of the bag, forms a steady weight on each side, which, although allowed to rest almost entirely on the bed and keeping but slight pressure on the thigh, is yet ready to act if there should be any starting of the muscles in sleep or otherwise.

Lastly. What is more important than any other measure is to have a proper bed, so that the patient can use the bed-pan and have draw-sheets changed without the slightest movement. I can recommend a mattress which effects this object, and which is perfect in its action, inexpensive as an ordinary one, and of the simplest possible description. It consists of two half mattresses firmly stuffed with horsehair and of good thickness. These two halves have a definite space between them, as regulated by the piece of tick connecting them underneath. This space is exactly filled by a rectangular air-cushion, which is made to occupy the gap between the two half mattresses. This cushion, when the mattress is in use, is kept firmly distended with air, and of course is under the patient's buttocks. When the patient requires the bed-pan, it is only necessary to let the air out of the cushion. The patient lying on his back on two firm half mattresses, the buttocks do not sink when the air is let out, but form a bridge over the gap; into this the bed-pan is introduced without the slightest movement being required on the part of the patient. On removal of the pan, the cushion is blown out again in about a minute by the small bellows for the purpose. I have found this bed invaluable, not only for excision of the knee-joint, but also in cases of acute hip disease, excision of hip—in fact, whenever perfect rest is required and the use of the bed-pan causes pain. It is moreover just as easy to change draw-sheets with this mattress. I cannot myself lay claim to this simple invention: it was first thought of by a patient suffering from acute hip disease, whose wits, sharpened by his sufferings, led him to the idea. I have shown it to a few surgeons, but have not until now brought it generally before the profession.

Thus Fergusson's excision-splint, with some modification, affords a very good means of securing the limb after excision in the case of adults. But for children it certainly is not suitable; for these we require something lighter, more movable, and not necessitating that strict restraint from movements which Fergusson's splint entails.

I believe the following will be found the best apparatus for children. It is light, entirely prevents any movement between the sawn end, and admits of some little twisting about on the part of the child without injury to the limb. In preparing and applying this apparatus I have been much indebted to the ingenuity and skill of my friend Mr. Clouting, resident surgeon to the hospital.

The splint consists of leather strengthened at the back by a piece of iron about two inches wide, which is riveted to the leather. To prepare it, cut a piece of leather stout in proportion to the age and size of the patient, in length from just above the heel to within two inches of the fold of the knee-joint, wide enough to surround two-thirds of the leg, and properly shaped. Prepare another piece to extend from about two inches above the joint to the tuberosity of the ischium, and long enough to reach on the outside and front as high as the crest of the ilium. The two pieces of leather are connected by the piece of metal of the width I have stated, a space of about four inches (but varying according to the size of the patient) being left between them. The piece of metal passes the length of the splint to within about half an inch of either extremity; it is hollowed slightly to correspond to the curve of the leather, to which it is riveted along its whole length by two rows of rivets. Before the operation, the leather, with the metal attached, is soaked in water and thoroughly softened, chamois leather is pasted to its inner surface, and the whole made ready. Immediately after the operation the splint is applied to the limb and fixed by gum bandages, which commence at the foot and terminate by a figure of eight round the pelvis. Great care is taken that the blood does not get into the splint; and the limb is laid in a M'Intire for twenty-four hours, to keep the foot straight and in good position while the gum hardens. The limb is then swung; the apparatus, being light and firmly fixed to the trunk, readily follows any movement of the patient without jar. The patient requires no other support until union has taken place and he is able to walk. I have lately found this splint very useful in the case of a woman, and prefer it in most instances to any other, but it is for children that it is especially valuable. The chief care required is in applying the gum bandage so as to afford the proper amount of support without running the risk of causing subsequent pain to the patient, and necessitating change in the apparatus from its being too tight.

One point remains which influences the result of excision of the knee-joint, and that is the sanitary conditions under which the patient is placed. It appears tolerably certain that a patient about to undergo the operation of excision of the knee-joint, entailing at the best a somewhat lengthened convalescence, is not having the best thing done for him when he is advised to submit to the performance of the operation in a crowded city, or, worse still, in a large city hospital. The fact that the disease for which the operation is recommended often has its origin in constitutional causes, and that the successful progress of the union between the bones depends so much on the maintenance or recovery of the general health, renders it scarcely fair to the patient with joint disease to excise under such unfavorable circumstances. The patient should be removed out of town to the country, or, still better, to the seaside, before he undergoes any such operation. For the poor, the large hospitals ought to have branch establishments at the seaside, to which they might send cases of this kind. Failing this, I humbly submit that it is better to use an institution such as the hospital here for the purpose.

Margate, August, 1871.

ON THE

TREATMENT OF SMALL-POX.

A Lecture delivered at the Homerton Fever Hospital.

By Alexander Collie, M.D.,

Resident Medical Officer.

In the mild variety of small-pox treatment is not of importance. "*Il ne faut que demeurer en repos.*" The patient should be kept in bed until crustation is completed, should be fed on light and easily digestible food, and should have his linen changed daily and fresh cool air in abundance. No drugs will be required, except perhaps a purgative pill or a little castor oil. In the black small-pox all treatment is useless, and our efforts should be directed simply towards palliating the patient's sufferings. Cold water will probably be most grateful, and that ought to be given freely. An egg beaten up in a little whisky-and-water will relieve sensations of exhaustion, but beyond this our art is powerless. Large doses of stimulants are simply wasted. In the severe or confluent small-pox, however, treatment is of the highest importance. Here our management will sometimes determine the result. The patient should be placed in a large well-ventilated room, a room with opposite windows if possible, and these should be kept open by night and by day and in all seasons. If the weather is very cold, keep a good fire in the room, and let the patient have an extra blanket, but keep the windows open; and if the weather is mild, treat the patient absolutely in the open air. Nothing is of so much importance as pure air, and that in unlimited quantities. In this hospital we have kept our windows open constantly by night and by day throughout the months of February, March, April, &c.; and this has been attended with the very best results, for our mortality is the lowest of all the small-pox hospitals in London, and we were receiving our patients from the same sources, and some time before the epidemic reached its height. Including every death, our mortality is only 14·6 per cent.; and if we were to strike off those who died of sequelæ, and those who died six, twelve, and twenty-four hours after admission, it would be considerably less. Up to July 22nd the mortality in the different hospitals has been as follows:—Hampstead, 19·1 per cent.; Stockwell, 17·6; Homerton, 17·1; Mrs. Gladstone's Small-pox Hospital, 17·1; Stockwell Fever Hospital, 15·5; Homerton ditto, 14·6.

The patient's bed may consist of feathers, or a hair or flock mattress; and there ought to be two beds in the room, in order that the patient may be changed from one to the other, and the bed made—a matter of great comfort to a sick person. The sheets ought to be of the finest and softest material. The coverings should be light, and all curtains should be removed. The room ought to be absolutely cleared of rugs, carpets, cushioned chairs, wardrobes, pictures, and everything which might harbor a particle of dust or a small-pox scab. Secure two competent and efficient nurses, one to act by day and the other by night, and remember that, usually, relatives make bad nurses. Inquire first about the bowels, and, as a rule, give a gentle purgative; but do not purge.

Do not place your patient on low diet. That a patient about to undergo the exhausting labor involved in an attack of small-pox should be starved is, in my opinion, of all medical absurdities the most absurd. Your patient will not be able to take food in the same form as in health, but he must and should have the same in quantity, if not indeed more, although in a different form. Let your patient then have milk in abundance, as many raw eggs beaten up with a little whisky as can be stuffed into him, beef-tea, arrowroot, sago, tapioca, &c. Ordinary tea and coffee, of moderate strength, you will find pleasant stimulants, and these should be given, remembering, however, that if given in too large a quantity they have a tendency to prevent sleep. To relieve thirst, cold water is usually the best and the most pleasant to the patient, and he ought never to be refused this. You will find, however, amongst some nurses and patients' friends a decided objection to giving much cold water. Always combat this motion. There is nothing more pleasant to a parched mouth and a dry tongue than pure cold water. Effervescing drinks, such as lemonade soda and potash waters, are very refreshing.

For heat of skin the patient may be sponged with tepid water two or three times daily. Indeed, in all cases of small-pox the whole body should be sponged daily. If there be restlessness or sleeplessness, the following, repeated in half an hour, if needed, will be found of great service:—Tincture of opium, fifteen minims; spirit of ether, fifteen minims; camphor water, one ounce. Or, what we have often found to succeed when drugs proper failed, was two or three ounces of whiskey in warm water, and a little sugar. Of course the dose must be modified according to age and sex; but in restlessness, sleeplessness, and delirium we have found this latter most useful. In the early stages, with the last-mentioned exception, avoid stimulants as a rule; but remember them about the tenth day, when the patient's powers will be taxed to the utmost. There is no special drug to be given; and I would advise you not to bother your patients much with mixtures. Feed them. If salivation be present, you had better let it alone; and be careful of giving sedatives, as the mouth and air-passages become more or less choked up with the saliva—a condition which would be very serious if bronchitis were also present. Soreness of the throat is always more or less troublesome. Oleaginous and mucilaginous drinks, and black-currant jelly, may be given as an amusement; but the only remedy is time and patience. Try the following, which we have found beneficial in this condition:—Tincture of perchloride of iron and glycerine, of each thirty minims: three times a day.

Towards the eleventh day look out for laryngitis; and when it occurs wrap a large linseed poultice round the throat, and keep it there, changing it three or four times in the twenty-four hours. Raise the temperature of the room, and surround the patient with an atmosphere of steam. Avoid antimony, mercury, and such like; and do not exhaust your already exhausted patient with emetics. I suppose I need hardly caution you against bleeding. Warmth, steam, feeding and some stimulant are in my opinion the best and safest remedies. If the difficulty of breathing increases, and you perceive commencing exhaustion, perform tracheotomy at once, unless there be reason to believe that the patient will die of small-pox. If there be no reasonable chance of recovery from this a prolongation of a few hours is only a prolongation of human suffering. Still, one can readily imagine cases where prolongation of life for an hour, or even for a few minutes, might be of the greatest importance. Of three tracheotomies performed here one recovered.

Delirium is present in some stage of the disease in a considerable proportion of cases, and it requires very careful management. It is usually most violent and dangerous in the early stage of the illness. See that your patient is not left alone for a moment, otherwise you may find him through the window, and on the street below. Delirious patients are very cunning, and watch anxiously for the absence of attendants in order to carry out their wishes. They bear contradiction badly, therefore see that the nurse does not oppose them, or argue with them. If a patient will get up, let him do so, and walk him about the room or into the staircase, and after that you will probably find him glad to get to bed again. Humor him always if you can, and do not treat him on the supposition that he is a poor stupid fool, for whom any kind of story will do. Remember that such patients are very cunning, and that their *reason* always remains intact. The objective world, as it presents itself to their consciousness, is retorted, but the reasoning on it is always sound. Always agree with your patients, therefore, and try to convince them that you are taking the necessary steps to accomplish what they wish, or to prevent what they fear. Try to discover the way in which the objective world presents itself to their consciousness, remembering that in the highly irritable condition of the nervous system present in delirium objects are far more vividly conceived, and produce a much stronger impression than they do in health. If your patient thinks he is going to be murdered, and that his nurse is in league with someone for this purpose, do not try to convince him to the contrary. You will only irritate him, and produce a conviction in his mind that you are in league with his enemies. Send away the nurse; put your patient into another room; command absolute darkness and absolute silence, and in the majority of cases this will succeed. Beware of using force, or of tying your patient down. I have again and again seen slightly delirious patients driven frantic by this means. Patience, gentleness, and firmness will almost always succeed, but above all things gentleness. Remember you can never handle a sick person *too* gently, nor speak to one *too* softly. To a shattered or highly sensitive nervous system the slightest sound, the faintest light, the gentlest touch, are sometimes almost unbearable. If therefore, a patient will get out of bed and walk about, will persist in throwing about his arms and legs, let him do so. It is far better that he should exhaust himself than himself and one or more attendants. Of course, if suicide is attempted in any form, force must be used; but proper nursing will never give an opportunity for an attempt of this nature.

The prevention of pitting has exhausted the ingenuity of all who have thought about it. To assist in preventing this deplorable result, common olive oil should be used: but all means employed for this purpose fail where most wanted. In the severe cases nothing will prevent pitting; and I would not advise you to try any of the ordinary methods. To use nitrate of silver effectively in confluent small-pox would give rise to so much irritation as to endanger the patient's life. The famous emp. Vigo is in my opinion, humbug. Tincture of iodine, calamine, with olive oil, sulphur ointment, &c., have been employed with more or less success as the practitioner has argued *ergo propter* or *ergo post.* I believe you will only annoy your patient by using them.

About the eleventh day you may find diarrhœa troublesome; but I would not advise you to interfere with it unless it is excessive or clearly weakening the patient. A little castor oil may be necessary; and in most cases I should prefer to try this first, as patients are usually constipated in the earlier stages of small-pox, and diarrhœa is sometimes an indication of the presence of irritating substances in the intestine. If castor oil fails, try the following:—Dilute sulphuric acid, fifteen minims; tincture of opium, five minims; water, one ounce: four times daily. Along with this may be given a starch-and-laudanum enema, with fifteen, twenty, or thirty drops of the latter, according to the age. This will succeed in the majority of cases; but when it fails, recourse must be had to the more powerful astringents. See that your patient is not taking too much whisky, as that sometimes causes diarrhœa.

Just about this period bronchitis, by itself or with pneumonia, may arise. Wrap your patient's chest in a blanket or in cotton wool; and if you think he requires a drug, the following is a good one:—Carbonate of ammonia, five grains; spirit of chloroform, fifteen minims; tincture of squills, ten minims; infusion of senega, one ounce: every three, four, or six hours.

Stimulants may now be useful. We avoid rules on this subject. In short, we always think more of the person, his history and constitution, than of the disease. In other words, we treat a *person* in a diseased condition, and not merely a disease. I will not speak very dogmatically on the question of alcohol; but I am quite sure that, in large doses, it sometimes does well. In small doses, such as an ounce or two, to help a raw egg down, or with a little hot water to produce sleep, it is always useful; but I think that in some cases it actually determines recovery. Ordinary physiological rules, coupled with the age and habits of the patient, must be the chief guide. If he has been accustomed to his daily or nightly glass, by all means let him have some stimulant all through.

The most hopeless of complications or sequelæ is pleurisy. We have employed with some success the following treatment: tincture of iodine painted over the affected side; cotton-wool wrapped round the chest and throat; a mixture containing fifteen minims of tincture of muriate of iron and thirty minims of spirit of nitric ether, in an ounce of water, three times daily; with stimulants in moderation, and good feeding. Always feed, whatever may be the complication. Erysipelas requires simply feeding and tonics; as local applications we have invariably used fine flour and poultices. We have only had one fatal case.

With regard to abscesses, early opening, tonics, feeding, and stimulants should be the treatment pursued. The following is an admirable tonic: two grains of sulphate of quinine, twenty minims of tincture of muriate of iron, and fifteen minims of spirit of chloroform, in an ounce of water —thrice daily.

Crusts should not be allowed to remain about the nostrils and mouth, where they poison the air which the patient must breathe. They should be carefully removed as they form, and a stream of carbolic acid lotion kept running over the part which is being removed. Air cannot pass over decomposing fetid matter without being more or less contaminated, and I believe this source of contamination is not sufficiently recognised. Always remove scabs under which pus is forming; and at this period see that your patient is bathed, which, if properly managed, may be done without producing exhaustion. It greatly adds to a poor creature's comfort to have a quantity of stinking matter washed away. A little carbolic acid may be put in the bath.

When the patient begins to convalesce, and also for some time during convalescence, he is often irritable, and generally uncomfortable. In this condition we have found the following very useful: dilute sulphuric acid, fifteen minims; tincture of opium, five minims; water, one ounce—three or four times daily. It relieves irritation, soothes the mind, and helps on the appetite.

Be particular throughout that your patient's linen is frequently changed. In some cases this may be an hourly instead of a daily necessity. Also see that he does not always lie upon his back, but that he is frequently changed, so as to avoid too constant pressure on particular parts. In severe cases procure a water bed at once. By good nursing, let me repeat, almost all bedsores might be prevented, and to their prevention we ought to look most carefully. Do not believe your nurse that your patient is dry and all right; look for yourself and see. Receive no information from nurses or friends which you may acquire from your own observation. Accept nothing on credit which you can see for yourself.

For hæmorrhages from the mucous surfaces there is no special treatment. If it is severe, add to your stimulant, and take care that your patient does not get out of bed to stool. Indeed, in severe small-pox, you should never allow your patient out of bed to stool after the seventh day of the disease. Insist on the bed-pan.

Orchitis, gonorrhœa, and arthritis sometimes require warm poultices and mild astringent lotions, but these are usually sufficient.

Such is the treatment we have employed. To sum up: air, cleanliness, feeding, stimulants in moderation, sedatives, and tonics are, we believe, the best treatment for small-pox.

One important question may be here answered—viz., when a small-pox patient may be considered free of danger to his neighbors? This, in reference to the public, is a most important question, and one which requires an accurate answer. We have thought over this very carefully, and we believe that we have arrived at an unassailable conclusion. It is a truism to say that a healthy man cannot give to another a contagious disease; for the question at issue is, when and how a person may be certainly recognised to be in a state of health. Now, you know the ordinary signs of health: a certain temperature, or rather range of temperature, a quiet pulse, a clean tongue, a clear mind, &c. When you find these conditions in a small-pox patient he is in a state of health. But—and this "but" is very important—certain products of disease remain for an indefinite time attached to the body. These are the scabs, and the scales which follow them. When these are quite gone, your patient well washed, and clean clothing put on, you may send him anywhere without let or hindrance. The practice here has been that, as a patient is ordered out of bed, he has a bath, and this is repeated every second day until he leaves the hospital. It facilitates the removal of the scabs. No person has ever been sent out of this hospital with a small-pox scab or scale.

I have but a word to say about children's heads; they are most aggravating. Shave, poultice until removal of the scales, then use tar or oxide-of-zinc ointment, the former being preferable, and give the following:—Cod-liver oil, one drachm; iron wine, two drachms: three times daily.

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ON SOME PHYSIOLOGICAL ERRORS CONNECTED WITH CHOLERA.

By William Sedgwick, M.R.C.S., &c.

In one of Dr. Geo. Johnson's late publications on the subject, it is assumed that cholera is due to a material poison, which "is absorbed and enters the blood." Having entered the blood, the poison is then assumed to have a twofold effect: in the first place, it is said to produce an arrest of the pulmonary function; and, secondly, to produce a gastro-intestinal flux. All the phenomena of cholera are supposed to be due to this specific blood-poison, which, by its effect on the lungs, produces a state of collapse which has been considered similar to that "which accompanies a fit of spasmodic asthma," and, by its effect on the alimentary canal, produces a flux, which, as "the result of a conservative effort to expel the poison and its products from the system," is said to be "analogous to the eruption of small-pox." If, consequently, with the author of this theory, we admit that cholera is due to a blood-poison, we have then to encounter the physiological difficulty of the twofold action assigned to it, and be prepared to assume that this specific blood-poison can, by its effect on the muscular coat of the minute pulmonary arteries, cause them to contract, so as to arrest the pulmonary function; and that it can, at the same time, by its effect on the muscular coat of the minute arteries of the alimentary canal, cause them to dilate, so as to produce a flux. Now, as the local condition of the vascular system immediately preceding the occurrence of a flux is well known, it is obvious that the so-called "stop-cock action" of a blood-poison on the vessels of the lungs must be inconsistent with the simultaneously contrary effect of the same poison on the vessels of the alimentary canal; and, accordingly, it has been found that such is the case. For there is absolutely nothing in the condition of a patient suffering from cholera analogous to that of a patient suffering from "a fit of spasmodic asthma"; and the unimpeachable testimony of successive clinical observers, from the earliest period in the history of cholera down to the present date, sufficiently disproves this assumed "stop-cock action" of an assumed blood-poison on the pulmonary circulation. It has indeed for many years been satisfactorily established that where there is no previous lesion of the lungs, percussion and auscultation reveal no pulmonary disorder in cholera. "Percussion of the chest," as M. Desnos in his lately-written and valuable article on Cholera informs us, "furnishes a normal or even an exaggerated resonance; and the vesicular murmur, far from being weakened, is heard in all its purity." Whilst the effect of cholera on the respiration in cases of pulmonary disease is opposed, even still more completely, to this theory of "a stop-cock action," and to the consequent attempt of Dr. Johnson to establish an analogy between cholera and asthma; for, according to MM. Briquet and Mignot (whose evidence, essentially agreeing with that of other observers, may be selected for the occasion), the dyspnœa was observed to be invariably diminished in all their phthisical patients attacked with cholera, and at the same time the expectoration almost, if not altogether, ceased. The chemistry of respiration in cholera, which was very carefully studied during the epidemics of 1849, 1854, and 1866, is alike opposed to this theory of the "stop-cock action" of a blood-poison on the pulmonary circulation; for the diminution of the numerical relation which subsists between the amount of carbonic acid and of oxygen in cholera respiration, and which leads to pulmonary collapse, is, as I have long since stated in my work "On the Nature of Cholera as a Guide to Treatment," simply an indication of the extent to which the organic functions of other parts are affected: *the arrested formation of carbonic acid in the system having the same relation to pulmonary collapse as the arrested formation of urea has to suppression of urine.*

The assumed antagonism in the action of an assumed blood-poison on analogous structures, leading to defective oxygenation of the blood in the lungs and to increased secretion in the alimentary canal, is not only unsupported by any satisfactory evidence in its favor, derived either from clinical observation or from chemical research, but it is, moreover, opposed to the observations of experimental physiologists. There is, it is true, an antagonism of action manifested in the effect produced by sympathetic or vaso-motor nerve-section, which may simulate the effect produced by poison or disease; and it will be useful to refer to this fact in connection with some of the occasional phenomena of cholera. But whenever such antagonism is displayed in the results of the same morbid agency, the structures involved are essentially unlike; and a very brief consideration of one of these phenomena in cholera will serve still further to show the fallacy of the physiological assumptions on which the theory proposed by Dr. Johnson rests.

Among the phenomena which are thus occasionally associated with cholera is the continuance of the mammary secretion during collapse, on which great stress has been laid, and which has been very confidently cited both by Dr. Johnson and by Sir Thomas Watson as capable of affording evidence in support of the theory that the disease is essentially due to defective oxygenation of the blood, consequent on contraction of the minute pulmonary arteries from the presence of a specific blood-poison. Dr. Johnson informs us that "in a nursing mother who is the subject of cholera and collapse the secretion of milk continues, because the milk constituents—casein, sugar, oil, and water—are not oxidised products." And, in like manner, Sir Thomas Watson, in the newly revised edition of his lecture on the disease, having urged in favor of this theory of defective oxygenation of the blood in cholera that the suppression of bile and urine during collapse is due to their chief constituents being all results of oxidation, proceeds to assure us that this "is rendered all the more probable by the curious fact that, when a nursing mother becomes the subject of cholera and falls into collapse, the secretion of milk continues unchecked. Now the chief constituents of milk—casein, sugar, oil, and water—may be derived from the blood without the addition of oxygen. They are *not* the products of oxygenation." The fallacy of this argument was fully exposed in the *British Medical Journal* of Sept. 19th, 1868, where it was shown that "the occurrence during cholera of the mammary secretion is in no respect exceptional; for, as regards the reproductive functions generally, there is satisfactory evidence to prove that menstruation may, like the mammary secretion, occur during the algide stage of the disease, without being either delayed, diminished, or checked; that the liability to abortion is, for the most part, limited to the early periods of pregnancy, and that the blood lost on such occasions is not unfrequently florid; and that towards the full term of pregnancy, not only does the liability to abortion usually disappear, but the life of the fœtus may be fully maintained during a prolonged algide stage of cholera, and even after the life of the mother has apparently ceased." As the literature which has been for some years steadily accumulating on this subject seems, however, to have been in some quarters quietly ignored, it may be useful to give a short summary of what has been suggested and observed respecting the continuance of the mammary secretion during collapse.

Three theories, which may be described as the anatomical, the chemical, and the physiological, have been on different occasions advanced to account for this phenomenon. The first of these theories, which was suggested by the celebrated Magendie, refers it to the nearness of the mammary arteries to the centre of the circulation. The second theory, which has been proposed by Dr. Johnson, and accepted by Sir Thomas Watson, refers it to the chemical composition of the mammary secretion, which is said to differ essentially from other secretions (bile and urine) which are suppressed, in the fact that oxygen is needed for the latter, but not for the former, of these secretions. Whilst in the third theory, which has been suggested by myself, it is proposed to account for it by the antagonism which naturally prevails between nutrition and reproduction, and which would allow of the continuance of functions associated with reproduction even when, as in the collapse of cholera, there is a central arrest of nutrition. The evidence in support of this theory, which is contained in my paper "On the Continuance of the Mammary Secretion when the Urine is suppressed in Choleraic Collapse," and which has never been refuted, proves that the mammary secretion is simply one in common with other phenomena, either primarily or secondarily associated with the reproductive organs, which are unaffected by cholera. In addition to the evidence, chiefly clinical, which was adduced on that occasion in support of this physiological theory of the continuance of the mammary secretion during collapse, there is, as might be expected by anyone whose attention has been directed to the progress made in such inquiries during the last few years, evidence of another kind, derived from experimental physiology, which satisfactorily confirms the explanation which I have given of this phenomenon in cholera. For, as I have lately had occasion to state in my paper "On Choleraic Glycosuria," it has been experimentally demonstrated by M. Legros (*Gazette Médicale de Paris*, 1866, p. 104) that paralysis of the branches of the sympathetic nerve distributed to erectile tissues, not only of the reproductive organs themselves, but also of the secondary sexual structures, instead of causing turgescence, produces a less active determination of blood to the part, so that immediately after extirpation of the superior cervical ganglion, for example, in cocks and turkeys, the erectile tissue of the head on the operated side becomes pale and collapsed.

It may be interesting to state that an unsuccessful attempt, analogous to that of Dr. Johnson, to account for the occurrence of one of these occasional phenomena in cholera by a chemical theory, was made some years since by M. Becquerel, who proposed to explain the fact that encysted dropsy of the ovary suffered no diminution during an attack of cholera which was attended by abundant evacuations by referring it to the presence of insoluble cholesterine in encysted dropsies, from which the liquid in true dropsies is free.

The history of cholera furnishes many examples of admitted mistakes in theory and in practice. Even Sir Thos. Watson himself has thought it advisable, in the later editions of his lecture on this disease, to give up the theory of vicarious secretion; although, for many years previously, he had proposed to explain the absence of uræmic poisoning in cases of prolonged suppression of urine in cholera, by assuming that the urea "is drained off from the blood in the enormous and unnatural flux from the stomach and bowels" (vol. ii. 1848, 3rd edit., p. 595). For this theory of vicarious secretion had necessarily become untenable when, in addition to the important chemical fact that there was as a rule no urea present in the rice-water flux, it was moreover satisfactorily demonstrated that suppression of urine usually occurred, without being followed by uræmic poisoning, in cases of excessive gastro-intestinal irritation. And as the well-directed investigations of many scientific inquirers have now succeeded in dispersing much of the mist of unfounded assumption which has hitherto obscured the disease, it cannot be considered unreasonable for us to hope that Dr. Johnson will soon be induced to follow the good example of Sir Thomas Watson, and, in like manner, give up a theory which has always been regarded as physiologically weak, and which has lately been deprived of the highly valued though indirect support which it was erroneously supposed to receive from the continuance of the mammary secretion in suckling women during collapse.

Park-place, Upper Baker-street, Sept. 1871.

ON AN EPIDEMIC OF ROTHELN.

By Andrew Dunlop, M.D.,

Hon. Med. Officer, Jersey General Dispensary.

For the last six months, or thereabouts, an apparently infectious exanthematous disease has been prevailing in St. Helier's, which several of my *confrères* and myself at present consider to be rubeola or rötheln, though in many respects it does not correspond with the descriptions of that disease which have been published. The first cases occurred at the end of last or the beginning of the present year. The epidemic reached its height about two months ago, so far as I can judge; and it seems now to be declining rapidly. I first saw the disease about the beginning of the year, when I was frequently requested to see children on account of an eruption which had made its appearance, and which the parents suspected to be that of scarlet fever or measles. In these cases there was an eruption consisting of minute red dots or puncta, about the size of pin-heads, on the face, neck, trunk, limbs, and hands and feet. It was copious at the root of the neck, abundant on the chest and back, and more thinly scattered over the extremities and dorsal aspect of the hands and feet. Over the gluteal regions, and sometimes on the cheeks, and at the flexures of the joints, there was generally a red flush. As there was usually sore-throat, the disease at first sight had all the appearance of scarlet fever; but there was scarcely any constitutional disturbance, and the temperature was only slightly, if at all, raised above the normal standard. The eruption disappeared in three or four days, without the occurrence of any other symptom. Early in the spring a great many patients, children and adults, applied to me on account of an eruption which had suddenly appeared on the face, trunk, and extremities. They had no other symptoms of any kind, and said that they felt perfectly well. This eruption was the same in appearance as that I had seen on the children, a month or so before, only I noticed that it varied in color, being in some cases of a darker and in others of a lighter red. I also observed that in many places, especially on the face and extremities, three or four of the puncta had coalesced, forming an irregularly shaped patch.

I soon saw cases in which the eruption was accompanied by mild feverish symptoms, with slight elevation of temperature, or sometimes derangement of the digestive system. Some of these patients complained of sore-throat, and I then found that in the great majority of cases the velum, uvula, and fauces generally, were covered with a deep-red punctate eruption. Later, cases came under my notice where, in addition to these symptoms, there was injection of the conjunctivæ, lachrymation, coryza, and slight cough; sometimes, too, the pyrexia was well marked. The following cases will give an idea of the nature of the disease. The first is an example of the most common class of cases, where the eruption is unaccompanied by any marked constitutional disturbance; the other two were perhaps the most severe cases that came under my care.

CASE 1.—Miss F——, aged thirty, went to bed quite well on June 14th, 1871, but on waking on the morning of the 15th she found that her face and neck were covered with an eruption. I saw her the same forenoon, and found that on the face, neck, trunk, and upper part of the chest, and on the dorsal aspect of the hands and feet, there was a pale-red, punctiform eruption. It was most plentiful at the root of the neck, and was very faint on the hands and feet. The puncta were about the size of pin-heads, and in several places on the face and neck three or four of them were aggregated together, forming irregularly shaped patches. She did not complain of sore-throat, but on examination a dark-red punctate eruption was seen on the soft palate, uvula, and fauces. Tongue brownish, but moist; appetite fair; bowels regular; slight injection of the conjunctivæ and a little lachrymation, but no running at the nose; slight cough; pulse 80; temperature normal.

June 16th.—Pulse 84; temperature normal; eruption rather faded on neck and chest, perhaps more distinct on hands, and a little to be seen on limbs; lachrymation and injection of conjunctivæ now considerable, with swelling of upper eyelids. On the extensor aspect of the arms there is a slightly red papular eruption, apparently consisting of enlarged papillæ. Throat still the same. Feels well.

17th.—Eruption now very faint everywhere; a very little desquamation, in the form of minute scales, on the side of the nose; eyes less suffused.

18th.—Eruption entirely gone. Throat better.

19th.—Eyes now clear, and coryza gone; no further desquamation.

CASE 2.—H. O——, aged eleven, felt sick and ill on the 14th of June, 1871, and in the evening he came home from school feeling chilly and complaining of sore-throat. I saw him on the morning of the 15th, and found that there was a rather dark red, punctiform eruption on the neck, chest, back, and hands and feet. It was most abundant at the root of the neck, over the anterior folds of the axillæ, and in the groins. It was copious and distinct on the chest and back, but scarce and faint on the limbs and the hands and feet. On the cheeks and over the nates there was a deep-red flush. Tongue covered with patches of white fur. There was a deep-red punctate eruption on the soft palate and fauces; appetite bad; bowels opened twice on the 14th; has had running at the nose for the last fortnight; conjunctivæ injected; slight cough; pulse 112; temperature 100·2°.

June 16th.—Pulse 116; temperature 103·5°. Passed a good night. Eruption rather faded on trunk, but more distinct on hands, feet, and limbs; tongue denuded of epithelium, with prominent papillæ.

17th.—Eruption everywhere faint. Pulse 100; temperature 99·6°. Profuse perspiration yesterday afternoon.

18th.—Eyes much injected; upper eyelids swollen; considerable discharge from the nostrils. Eruption almost imperceptible. A little desquamation to be seen on the sides of the nose. Pulse 84; temperature normal.

19th.—Pulse 80; temperature normal. Passed a good night. Eruption now only to be seen faintly on the anterior folds of the axillæ. Papular eruption on the extensor surfaces of the arms. Eyes less injected; throat better.

20th.—Pulse and temperature normal; eyes natural; slight cough. On the anterior folds of the axillæ there is a number of dark-red spots, which do not disappear on pressure. Except these spots, no eruption visible anywhere. No further desquamation.

23rd.—Has been doing well since the 20th. The spots on the axillary folds have now disappeared. No desquamation visible; but on rubbing the chest the cuticle peels off in small rolls.

25th.—Much better. Slight desquamation here and there on the neck and chest.

CASE 3.—A. O——, aged eight, a brother of the last patient, complained of sore-throat on the 16th of June, 1871. I saw him on the 17th, and found the uvula swollen and a dark-red eruption on the soft palate and fauces. There was a slight red flush, with commencing spots, just perceptible at the root of the neck. Tongue slightly furred; appetite fair; bowels regular; eyes rather glistening and watery; no coryza. Pulse 80, irregular; temperature 99·4°.

June 18th.—Passed a restless night. There is now a bright-red punctate eruption on the neck, chest, back, upper arms, and face. On the cheeks there is a red flush. In many places on the chest a few of the puncta are run together, forming irregular patches. The eruption is scarce and faint on the forearms. Throat rather better. Pulse 88, regular; temperature 100°.

19th.—The eruption is now abundant on the upper arms, and rather less so on the thighs. It is also more plentiful and more distinct than yesterday on the forearms and hands, where it is of a pale-red color. There is none to be seen on the legs or feet. There are red flushes over the nates and at the joints. On the arms there are many aggregations of a few of the puncta into irregular patches. Eyes the same—glistening, but no injection of the conjunctivæ. Pulse 108; temperature 100·8°.

20th.—Eruption now well marked on the legs and feet, and growing faint on the trunk and upper arms. On the legs and feet many patches of aggregated puncta. Conjunctivæ injected, with lachrymation and running at the nose. Under lip swollen. Pulse 104; temperature 102·2°; appetite fair; bowels regular.

21st.—Eruption now very faint on the trunk, except on the anterior folds of the axillæ. It is still distinct on the hands. Eyes less injected; throat better. Slight desquamation at the sides of the nose. Pulse 88; temperature 99·8°.

22nd.—All traces of eruption gone except a few dark-red dots, which do not disappear on pressure, on the anterior axillary folds, and here and there on the chest. Itching all over the body. Pulse 72; temperature 99·2°.

23rd.—Conjunctivæ still a little injected, and throat red. No further signs of desquamation. Pulse and temperature normal.

26th.—Doing well. There are a few traces of desquamation on the neck and upper part of the chest. Eyes natural and throat well.

I regret much that the urine was never examined in these cases.

When the epidemic was at its height, it prevailed most extensively at St. Helier's, passing through a great many families, and breaking out in most of the schools. The principal of one of the schools told me that while the disease was prevalent in his establishment one of his ushers left for London in perfect health, and that five or six days afterwards he found himself covered with the eruption.

Jersey, June, 1871.

TOXICAL EFFECTS OF CHLORAL HYDRATE WHEN PERSISTENTLY USED AS A HYPNOTIC, AND FATAL RESULTS OF LARGE DOSES.

By N. R. Smith, M.D.,

Late Professor of Surgery in the University of Maryland.

In February last, a medical friend, long retired from practice, called on me for advice in regard to a singular affection of the fingers of both hands, attended with desquamation of the cuticle and superficial ulceration, especially about the borders of the nails. It was associated with pain and much morbid sensibility to touch. There was also some acceleration of pulse, and general *malaise*. He visited me daily for some ten days, when, by the use of astringent lotions and of ointment the local affection was overcome. He informed me that he had been taking chloral in liberal doses, as a hypnotic, for four months (I am not perfectly sure that it may not have been four weeks). He expressed to me his conviction that the disease of his fingers had resulted from the use of that medicine. Having never observed the agent to produce such a result, I was reluctant to believe that such was the case.

Some three weeks after the cure of the local affection, I was called to attend my friend, in consultation with his family physician. We found him laboring under acute bronchitis in severe degree. His respiration was exceedingly embarrassed, and there was a high degree of hoarse mucous râle. The bronchial tubes were filling. The pulse was about 140, and the action of the heart extremely feeble. In the treatment adopted our object was to sustain the powers of life, which were rapidly failing, and to relieve the bronchial tubes of mucus. Our efforts, however, were unavailing. He died on the third day after I first saw him.

I scarcely, at the moment, entertained a suspicion that the use of the chloral was concerned in producing the fatal malady of my friend, it being not at all uncommon for persons of his age (seventy) to succumb suddenly to such malady from ordinary causes. Some two months later I accidentally met a medical friend, who expressed pleasure at the meeting, as he wished to consult me in relation to a singular affection under which his daughter, a young lady twenty-two years of age, was suffering. He described precisely the affection of the integuments of the fingers which had occurred in the case described above, erythematous inflammation, desquamation, and ulceration around the borders of the nails. Struck with the resemblance which her malady bore to that of my friend Dr. C., I inquired if she had been taking chloral. He replied that she had taken it, as a hypnotic, every night for a month, and that he had suspected this article to be the cause of her disease. The young lady was not suffering constitutionally at that time, but about ten days after I was called to see her. I found her extremely ill. There was universal anasarca, the action of the heart was exceedingly feeble, the pulse 140 and extremely weak. Her respiration was much embarrassed, and the recumbent posture was impossible. Procuring some of the urinary secretion, I tested it with nitric acid, and discovered a notable quantity of albumen. I was very apprehensive of a fatal result, but immediately prescribed stimulants and diuretics, digitalis being the constituent most relied upon.

On visiting the patient, after an interval of a day, I was much surprised and gratified to find her greatly improved. Her pulse had been reduced to 90, and was much improved in tone. The kidneys had acted freely, and the anasarca had much abated. Having been myself confined by sickness, I did not again see her. On meeting her father some three weeks later, I was gratified to learn that she had entirely recovered. I have knowledge of two other cases in which the same affection of the fingers resulted from the use of chloral.

Within the last ten days two deaths have occurred in Baltimore, manifestly from the toxæmia caused by an overdose of chloral. The subject of one of these accidents was a gentleman who had been under the care of an irregular physician, and by his advice had taken chloral in ordinary doses for the relief of a painful neuralgic affection of the neck. After the medical attendant had discontinued his visits, the patient persisted in the use of the hydrate, taking it, as I was informed by his brothers, in doses of not less than half a drachm. On the day of his death he was known to have purchased three drachms of the article. How much he took during the day is unknown. In the evening he retired to his chamber, and in about twenty minutes afterwards was found dead beside his bed. He was undressed, the bed-clothes were turned down, but the bed was undisturbed, and it was manifest that death had arrested him at the moment that he was prepared to step into bed. The coal-oil lamp which he used was extinguished, but the glass chimney was still hot. The glass from which he had taken the chloral stood on a small table near the head of the bed, and in it were a few drops of the medicine, recognised by his brother by taste and smell. There can be no doubt, therefore, that he fell almost instantly dead from the effects of the poison.

Another instance of almost equally sudden death has occurred in this community. The fact is well known, but I am not authorised to name the individual. He had been laboring under a painful affection of the head, and was attended by a homœopathic physician. On the evening of the night of his death, he had a hypodermic injection of morphine practised upon him, probably in ordinary quantity. This not relieving his pain, chloral was administered. He went to bed, soon became quiet, and for some hours was left undisturbed. His perfect stillness at length attracting attention, he was found to be dead, and probably had died soon after the administration of the chloral. I have no reason to believe that the medicine was given in larger dose than has been recommended as safe by high authority, nor do I know whether he had taken it for any length of time.

Another case of which I have knowledge was that of a lady who had undergone a severe surgical operation. As she suffered pain, and was restless, it was determined in consultation to give chloral by injection, so as to avoid irritating the stomach. A drachm and a half was thrown into the rectum. She at once sank into a state of insensibility, and died in some three hours. An eminent physician of Washington, who was in immediate attendance on the case (Dr. N. S. Lincoln) gave it as his opinion that she died from the effects of the chloral.

These cases are, it appears to me, amply sufficient to establish the toxical effects of this powerful agent. It is probable that its poisonous effects are exerted in two ways—first, when given in a large dose, and especially where the system may have been charged with it by its previous administration, it at once overwhelms the powers of life, and causes immediate death. Upon what organ or organs does it exert its deadly effects? It must be upon either the heart or the brain; perhaps on both. It is believed that chloral, entering into the blood, develops chloroform in that fluid, the amount developed being determined not merely by the quantity taken, but by the condition of the blood. Chloroform, we know, when inspired, exerts its influence upon both brain and heart. In the numerous cases in which it has caused death by inspiration, this result has been produced by its interrupting the circulation. Secondly, it appears, when given in small doses, and continuously for some time, to induce a form of toxæmia similar to that caused by the continued administration of ergot. Its effects on the fingers of both hands, in the two cases related above, would justify such a belief. It is well known that animals fed on spurred rye suffer gangrene of the extremities. In one case in which I tested the urine, albumen in notable quantity was detected. This case alone, however, establishes nothing.

Another very interesting and important inquiry is certainly suggested by the foregoing observations, crude as they are. If chloroform, developed in the blood from chloral, is productive of such disastrous effects, primary and secondary, can the direct inspiration of chloroform be as innocuous as it is thought to be? The profession are sufficiently aware of the fatal primary effects of chloroform in numerous instances. It has undoubtedly caused death in many cases in which it has been given with every caution in regard to quantity and mode of administration; in cases, too, where there existed no malady of brain or heart to forbid its use. In some cases it has been administered fatally in which it had been previously breathed with good result. But I would more especially call the attention of the profession to the chronic poisoning of the blood which I believe results from its free and repeated use. I have administered chloroform perhaps as often as any other surgeon in America, both in hospital and private practice, commencing its use from the time of its discovery and its first application as an anæsthetic. Indeed, I have been constrained to use it in many cases in which my judgment was adverse to its employment, for such is the overweening confidence in its effects that many patients refuse operations except under its influence. But the more I have used chloroform the less has my confidence become in its innocuousness. When I compare the results of my operation performed before anæsthetics were employed, with those performed during the last twenty-two years by the aid of chloroform, I am satisfied that unpleasant secondary results were less frequent during the first period than they have been under the use of that agent. I allude to secondary hæmorrhage, pyæmia, erysipelas, and hospital gangrene. Whoever will take the trouble to look over the medical journals and retrospects of the last ten years will discover that pyæmia or septicæmia occupies far more space in surgical records than it did before anæsthetics were so generally employed. When chloroform is administered during the period of an hour or more, as it frequently is, it undoubtedly enters copiously into the circulation, not only powerfully impressing the brain and heart, but modifying the constitution of the blood and the functions of the capillaries.

If the effect of chloroform developed from chloral in the blood be such as I have shown on the functions of the minute vessels, causing erythema and ulceration in the extreme parts, may we not suppose that the introduction of chloroform more directly into the circulation may promote the occurrence of those results not uncommon before its use?

These suggestions I trust will not be regarded as impertinent from one who has practised surgery for more than half a century, without and with the anæsthetic agents. I doubt not that, if these remarks are deemed worthy of any notice at all, they will be rejected by the majority of the profession, but I have an abiding confidence that their truth will ultimately be acknowledged.

Baltimore, June, 1871.

CASE OF ULCER OF THE STOMACH.

EFFECTS OF ANIMAL CHARCOAL; RECOVERY.

By J. Farrar, L.R.C.P. & L.R.C.S. Ed.

J. A——, a farm laborer, aged twenty-six years, unmarried, presented himself about a month ago with the following history and symptoms:—About ten weeks previously he began gradually to feel sick, as if he were going to vomit; which feeling was accompanied with a sensation of heat at the pit of the stomach, heart-burn, and a falling-off of his appetite. The heat in the epigastric region continued to get worse, till, as he said, it felt "burning him"; and he now had a dull, heavy, continuous throbbing pain at this place. These sensations of heat and pain were always worse just after meals. The patient was almost constantly affected with dizziness, but especially after food; and with a severe headache. On placing his hand on the seat of pain, he felt a tumour there, which, he thinks, was much larger than an apple, and which he could very clearly define, as he could grasp it in his hand as a "hard, firm lump." On his exerting pressure at this place he experienced a severe pricking, cutting sensation, amounting to intense pain on the pressure being increased. At times he had shooting pains from the tumour through to his back and up to the left shoulder, but more particularly to his back, and which, I may incidentally mention, I found to be at a point a few inches below that at the pit of the stomach. These symptoms becoming worse, he was compelled to cease work; and he then applied for relief to a medical man in the district, who gave him a "bitter, sour medicine," and advised a day or two's rest, treating him for "liver complaint." In a few days he thought he felt better, and began his work; but was again in a short time compelled to take rest. He then put himself under another medical man for three weeks, and finally came home to be treated by the family doctor, by whom he was attended for a period of four weeks, and up to the time at which he applied to me.

On examination I could detect no tumour in the epigastric region, but there was extreme tenderness there. He still felt pains darting through to his back and up to the left shoulder, the right shoulder also being now implicated. His tongue was dry, parched, and coated with a dirty, thick, yellowish-white fur; bowels costive, the fæces described as hard and dry—"crumbly." There was great thirst, and the patient had no appetite; he had dizziness and headache. Skin moist; head cool; pulse 110, somewhat jerking and wiry. He always rested well at night. There was nothing in his general appearance to indicate that there was malignant disease, though he was somewhat thin from his inability to eat. He could recollect no blow having been received at the seat of pain, nor could he give any reason to account for his disease. He is not addicted to strong drinks, and has always been steady and regular in his habits. His greatest discomfort was the presence in his mouth, at night when in bed, of a fetid and highly disagreeable fluid, which came from the stomach. He never awoke without finding his mouth filled with this abominable matter, and running out of the angles of the mouth on the pillow; by day he was never thus troubled unless he lay down, and he therefore avoided the horizontal position as much as possible.

I gave him no medicine, but told him to continue with that which he was taking from the family doctor till it was exhausted, and then again present himself, and, if possible, to bring with him, in a thoroughly cleansed bottle, a little of the fluid which found its way into his mouth; he was to well wash out his mouth before going to bed, and on awaking to immediately allow the fluid to run into the bottle from his mouth, and bring it to me the same morning for examination. The matter thus collected was found to be a dirty greenish-yellow fluid, which emitted a most abominably fetid stench; under the microscope it was found to consist of pus-cells, a few blood-corpuscles, epithelial scales, &c., suspended in a thin but apparently viscid fluid.

From the foregoing history and symptoms, together with the patient's general appearance, I diagnosed the case to be one of non-malignant ulcer of the stomach. He was ordered to keep his bowels open, so as to have one motion a day, with castor oil; to abstain entirely from every kind of stimulant and all hot irritating fluids and condiments—no pepper, cayenne, mustard, &c.; his food to be of the simplest and lightest character—no animal food to be taken except in the fluid state, as broths, beef-tea, milk, &c.; and nothing to be taken at a higher temperature than that of new milk. On every alternate night he was to apply a mustard cataplasm over the seat of pain; also to take perfect rest.

This treatment continued for a week, gave great relief to the burning and throbbing pain in his stomach, but had no apparent effect whatever in restraining the disagreeable rise of the fetid matter into his mouth. He had hitherto been given nothing in the way of medicine. I now gave half-teaspoonful doses of animal charcoal three times a day, to be taken, if possible, mixed with a little cold water—just enough to form it into a bolus, and to be taken half an hour or more before food; his diet as before. On the second night after beginning the charcoal he expressed himself so greatly relieved of the presence in his mouth of the fetid fluid that I at once doubled the dose of the charcoal; and with the effect, during the first night, of complete immunity from this the patient's most distressing symptom. From the day on which he commenced taking this remedy in the larger dose, up to the present time, he has only on one occasion been troubled with the symptom, and this was one night when he had run short of the charcoal.

In a few days after the disappearance of this symptom the pain and heat in the stomach also began to diminish; so, too, the headache, the dizziness, &c. The tongue became moist and clean, and the thirst subsided. In short, the patient rapidly improved in every particular from a few days after the time the charcoal treatment was adopted. The mustard cataplasms were, however, still continued, one now being applied every third day, and the same precautions carried out with regard to the food, &c., as at first, the only alteration being an allowance of milk and egg—two eggs beaten up with milk, and two or three tablespoonfuls of this taken at a time every hour or two.

In parenthesis, I will notice here an appearance I observed in the epigastric region, produced by the action of the cataplasms. This was an increase in the redness of the part in the situation of the ulcer, showing in a manner the size and extent of the ulcer. The spot thus defined in the space covered by the mustard was not circular, but irregular in outline, having projections from its centre like the rays of a star-fish; it was, in fact, in appearance something like a crab, and might therefore, if the *shape* of the mass only were considered, be called *cancer*. I mention this appearance because I have not yet seen it noticed by anyone, or heard it spoken of; and because in such diseases, with more obscure symptoms, the mustard cataplasm might thus perhaps be made use of in assisting us to determine as to the presence or absence of an ulcer or vascular tumour in this situation, though this appearance may, as far as my knowledge goes, be purely of an accidental character, and have nothing whatever to do with the existence of such diseases. If the appearance *was* a result of the presence of an ulcer in the stomach, there must have been a limited amount of peritonitis between the anterior walls of the stomach—where I believe the ulcer was situated—and the walls of the epigastric region, resulting in the union by lymph of these parts, and forming one continuous solid wall from the exterior to the internal ulcer—in a similar manner to what is sometimes seen in abscess of the liver.

Aug. 28th.—Everything is as could be desired, the patient being to all intents and purposes cured of the disease; there is now no tenderness on pressure in the epigastric region, no shooting pains, no disagreeable taste in the mouth; the appetite has returned; and the only thing which prevents the patient from resuming his duties is his general weakness, consequent on the forced abstinence from a sufficient quantity of proper food during the two months he has been ill.

Remarks.—I am firmly of opinion that the most important part of the treatment in this case was the administration of charcoal. The patient, from the day on which he took the first dose, felt greatly relieved, not only of the presence in his mouth of the discharge from the sore, but in every other respect. The feeling of nausea immediately left him; also the headache, the dizziness, the thirst, &c. Whether the remedy acted by preventing the formation of pus, or it absorbed, altered, or destroyed some of the more obnoxious and acrid parts of the discharge, I cannot say, but certain it is that it effected from the first a most striking change for the better in the patient's condition; and its power was all the more clearly and certainly recognised when at one time the patient had run short of it, and was again, on that particular occasion only, troubled with the most distressing of his symptoms.

Boroughbridge, Aug. 30th, 1871.

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ON POISONING FROM MORBID FLUIDS IN CONTACT WITH THE SKIN.

By JAMES HENRY BENNETT, M.D.,

Late Obstetric Physician to the Royal Free Hospital.

ON reading Mr. Paget's interesting communication on "Dissection Poisons," it has occurred to me that my own experience may be worth recording, more especially as regards the sequelæ of such cases.

I can fully substantiate by my own personal history Mr. Paget's assertion that poisonous fluids often exercise their influence on the human economy by imbibition through the skin, without any solution or continuity. I have six times been thus poisoned—twice in post-mortem examinations, and four times in making uterine examinations, without having at the time the slightest visible wound or abrasion. On each occasion the poison seemed to pass down a hair into the hair-bulb; at least, a hair was in the very centre of the painful spot in which diseased action commenced. On the other hand, I dissected and taught anatomy in Paris for seven years, from twenty to twenty-seven, and during that time had cuts and wounds innumerable, without once being poisoned.

My first attack was at the age of twenty-four, when house-physician at La Pitié, and was the result of the post-mortem examination of a case of peritonitis. An excessively painful spot appeared on the medius finger of the left hand, which soon began to swell. Ribbon-like inflamed lymphatics ran up the arm, soon followed by erysipelatous inflammation of the skin, the arm swelling to the size of the leg. The axillary gland also became swollen and tender. The erysipelatous inflammation of the arm passed on to the walls of the chest; and matter formed in the finger, which had to be freely laid open. There was considerable constitutional disturbance, fever, and prostration. I was treated by my teacher and friend Lisfranc, who applied numerous leeches, cold lotions, and kept me on a spare diet. About the tenth day a remission came, and all the symptoms gradually subsided.

All subsequent attacks have been exactly of the same character, only each successive poisoning has been more severe. On each poisoning the lymphatics have become inflamed the second or third day, except on the last occasion, twelve years ago, when I destroyed the painful spot with potassa fusa within twenty-four hours of its appearance. I did not thereby prevent inflammation of the whole hand, nor suppuration of the subcutaneous cellular tissue in the finger; but I apparently prevented the extension of disease to the lymphatics of the arm. In this case the poisoning followed a post-mortem examination for uterine cancer. The disease had given rise to some interesting abdominal conditions, and my hands were long imbued in the morbid fluids.

The second attack, which occurred whilst I was still in the Paris hospitals, was the result of a mere vaginal examination, and was treated in the same manner, on antiphlogistic principles. The third and fourth were produced in the same way, by the imbibition through the sound skin of poisonous secretions in the course of active duties as a London obstetrician and a gynæcologist, without my being able to fix the poisoning on any particular case, owing to the number seen. On the fourth occasion, in 1857, I very nearly died. I was attended by my friend Mr. Erichsen. Another friend, Mr. Samuel Hey, the senior surgeon to the Leeds Infirmary, who saw me, says that he never recollects seeing any other person recover who was as near death from poisoning as I then was.

After this attack, however, I did not recover my health. I became subject to chronic laryngitis, and had symptoms of chronic low gout, to which I am constitutionally liable. Two years afterwards I had the fifth poisoning, which followed the cancer post-mortem. A fortnight later I was seized with subacute bronchitis, which lasted all summer; and in the following February I had a severe attack of hæmopytysis, which led to the discovery by my esteemed friend Dr. Quain of extensive tubercular deposit in the right lung. According to Dr. Williams, one-quarter of the lung was infiltrated; according to Dr. Quain and Professor Bennett, one-sixth. In the ensuing summer the disease progressed; extensive softening and the formation of cavities followed; and this state of things led to my giving up practice entirely for two years. At the end of that time I recovered sufficiently (to my great astonishment) to resume practice in a modified manner.

The chest disease was only arrested by adopting a hygienic country life, and by living, winter and summer, in a good climate. The summer months of that year, 1859, I spent on the coast of Scotland, boating and fishing; the winter at Mentone, with marked improvement in the general health. Still, during that winter I had another poisoning, my last. I was casually asked to give a consulting opinion in a case of subacute vaginitis in a young lady with purulent discharge, and made a vaginal examination. The next day I had the old stinging burning red papule at the base of a hair, on the index finger. The papule ulcerated, and gave rise to a small, painful, burning, excavated ulceration, which lasted three weeks, only healing after repeated applications of the solid nitrate of silver. This time the mischief remained local; I had no subcutaneous cellular inflammation; no angioleucitis of the arm, or swelling of the axillary glands. My general health was no doubt better, so I resisted the local poisoning. Since then I have done a good deal of uterine surgery, and have exposed my fingers and hands to all kinds of morbid secretions, but I have never been poisoned. This immunity in one so susceptible is no doubt owing to the strictly hygienic life that I have since led, and to the fluids and solids of the economy having been constantly kept in a healthy condition.

It appears to me that my case is interesting and instructive in various ways. As an anatomist, notwithstanding a special susceptibility, I attained and preserved for seven years that immunity mentioned by Mr. Paget as attainable by the constant habit of dissection; but it did not preserve me from poisoning by morbid fluids produced by diseased processes in the living.

The liability to such poisoning, however, seems to have depended on the state of the general health. It was only after five years' hospital work, and after actually living in a hospital for a couple of years, that I succumbed to the danger to which I had previously been daily exposed—that is, when the general health had become lowered by "hospitalism." The same fact repeated itself in London. I was some eight years without being poisoned, although constantly exposed. But these were years of leisure and of hygienic life compared to those that followed once I became thoroughly immersed in active obstetric practice. No doubt the general health gave way first, without my perceiving it. Then the economy became a fit nidus for disease, and the poison being applied, it was received and germinated. Such, no doubt, is the origin of most zymotic disease. For an animal poison to act, to germinate as it were, whether imbibed through the skin or through an internal mucous membrane, the general vitality of the system must be modified so that the organisation constitutes a suitable nidus. What the conditions are which make the animal organisation such a nidus I will not discuss. Still I think, with the great majority of the profession, that a state of lowered organic vitality from defective hygienic conditions is one of the most frequent and most important of these conditions. This fact is illustrated by the sequelæ of my own case.

After four months' exposure to full hygienic conditions I had not lost the liability to poisoning by the skin; but I was sufficiently improved in general health to throw off the poison locally. Since then for twelve years I have kept the economy in a thorough state of hygienic vitality, and have satisfactorily resisted the morbid influence to which I had six times succumbed before. It is partly the conviction that a seventh poisoning would probably be the last that has kept me true to the resolution taken twelve years ago—viz., to abandon the active, excelsior path in my profession, and to remain merely an outsider; working it is true, but making the care of my own health the first and paramount consideration.

During the course of my professional life several friends have died from a first poisoning; others have died from the second and third; others have died later from other diseases, probably induced by the poisoning. From their cases and from my own I think we may draw the practical deduction that any medical man thus poisoned should, if possible, give up work for a year. I would advise him, judging from my own personal experience, to spend a summer boating and fishing in Scotland or Ireland, and a winter on the shores of the Mediterranean. If the poisoning occur a second time he should (still if possible) withdraw either entirely or partially from the active duties of the profession. In the latter case he should confine himself to such practice as will allow him to live or sleep in the country *evermore*. Otherwise he may expect a repetition of the poisoning, death, or disease and death; nor can he reasonably complain if it is so, for he has had his warning.

Grosvenor-street, July, 1871.

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CONTRIBUTION TO OBSTETRIC MEDICINE.

By Robert Bell, M.D., L.R.C.S. Ed.

CASE OF PUERPERAL CONVULSIONS, ILLUSTRATING IN A MARKED DEGREE THE EXPEDIENCY OF EMPTYING THE WOMB AS EARLY AS POSSIBLE.

Mrs. W——, a strong, healthy woman, eight months advanced in her second pregnancy, was up to March 2nd feeling quite well, with the exception of having experienced a considerable amount of pain in the lower part of the back. The gravid uterus was unusually high up in the abdomen, quite filling up that cavity and pressing considerably on the thoracic viscera. There was no œdema in any part of the body, neither was the patient drowsy or uncollected. Indeed, up to the forenoon of the above date her pregnancy was progressing most satisfactorily, when she began to feel slightly out of sorts, and in the afternoon was seized with a most marked epileptic fit. This was followed by another towards evening, and again another occurred, the interval each time becoming shorter, till between 11.30 P.M. and 12.30 A.M. she had no less than four very severe epileptic seizures. Matters wore a very alarming aspect, the more so as no appearance of approaching labour was present; and I felt certain that my patient's strength could not hold out much longer if immediate relief was not given. By this time the pupils were quite insensible to light, and deep stertor had set in, which was becoming more and more profound.

I immediately sent for Dr. J. G. Wilson, and in consultation we deemed it advisable to induce premature labour, so as to effect delivery as speedily as possible. Dr. Wilson then proceeded to rupture the membranes; and in a most remarkable manner, from the moment that the liquor amnii escaped the stertor began to subside, but not altogether, nor to such an extent as to cause us to be less anxious as to the final result. The os was fortunately very flaccid, and could be dilated to a considerable extent by the fingers without much difficulty. As soon as it could be accomplished with safety, Dr. Wilson applied the long forceps, and within thirty minutes of the rupturing of the membranes the child, a fine healthy boy, was born, and although in a semi-asphyxiated state, this soon passed off on applying the usual restorative measures. The placenta speedily came away, and the uterus contracted in a most satisfactory manner.

No sooner was the womb thoroughly empty than the stertor ceased, and all immediate danger seemed at an end. The sudden relief which the whole system appeared to receive on the birth of the child was most instructive, and indicated the propriety of the treatment pursued; but this was still more manifest in the next few hours, for there was no recurrence of the convulsions till 9.30 A.M., fully eight hours after delivery, when a very slight one occurred. This was the last unfavorable symptom which the mother had, and from that time her recovery was rapid and uninterrupted. The babe also is well and healthy.

CASE OF DIFFICULT LABOUR, WITH COMPLETE INVERSION OF THE UTERUS OCCURRING WITHOUT SHOCK OR HÆMORRHAGE.

Mrs. S——, a primipara, aged about thirty, full period. Soon after labour commenced a brow presentation was detected, with the face towards the pubes. For upwards of twelve hours the pains were strong and regular, but ultimately became feeble and inefficient, until at length several hours passed without progress being made, and the patient was becoming nervous and exhausted. It was then resolved in consultation with Dr. J. G. Wilson to effect delivery by means of the forceps. The patient was accordingly put under the influence of chloroform, and the infant (a strong and healthy female, considerably above the average size) was extracted with considerable difficulty. After waiting the usual time, slight traction was being made on the cord, when, to the surprise of Dr. Wilson and myself, the uterus suddenly became completely inverted, with the placenta firmly adherent to the fundus. Without any delay, Dr. Wilson separated the placenta, and effected reposition of the uterus in the usual manner. This grave accident occurred when the effects of the chloroform had almost passed off, and yet no perceptible shock to the system was apparent; and what is as worthy of record is the fact that very little above the usual amount of hæmorrhage took place, notwithstanding that the placenta was forcibly detached during the time that the uterus was inverted.

In case secondary hæmorrhage might occur, I remained in the house all night; but the discharge was no more than in an ordinary case, and my patient spent a very comfortable night, expressing herself as feeling "very well" in the morning. From that time she made an uninterrupted recovery, and was feeling so well on the 30th of March (four days after her confinement) that she began to talk of getting up.

Remarks.—Such cases cannot fail to be very instructive, possessing as they do so many features of more than ordinary interest. The first perhaps affords more instruction than the second, and it stands foremost in importance on account of its more frequent occurrence, and also from the fact that two lives are jeopardised—that of the mother and the babe; whereas in the other the mother alone is in danger. Here, then, we have convulsions coming on at the eighth month, without any apparent cause, in a woman who up to the day of her sudden seizure had been enjoying the best of health. No external evidence could be found to show a faulty action of the kidneys, nor indeed of any of the other organs; yet a succession of convulsions of such frequency and severity supervene, that for a time the lives of the mother and child are in imminent danger, and depend altogether for their safety on the skill of the accoucheur. To decide what was to be done in the circumstances was simple enough; but to act, and bring away a living child with safety to the parts of the mother in the short space of time which the operation took to perform, was quite a different thing. Another point worthy of note was the partial cessation of the stertor when the liquor amnii escaped, and the complete suspension of it almost immediately after the uterus was emptied; and, what most important of all, the discontinuance of the convulsions. But the case possesses interest apart from the mother altogether, and that is in the fact of the child, although premature and having been exposed to so much danger, being alive and doing well nearly six months afterwards.

The second case is also full of interest, and presents many most important points worthy of remark; and amongst these may be noted the almost spontaneous nature of the inversion, its completeness, the absence of shock and hæmorrhage, the facility with which the uterus was replaced without any recurrence of the displacement, no tendency to secondary hæmorrhage, and the completeness of the recovery without an unfavorable symptom.

Woodlands-road, Glasgow, Aug. 24th, 1871.

A CASE OF CONGENITAL MALFORMATION OF THE RECTUM.

By Isaac Mossop, L.R.C.P. & L.R.C.S. Edin.

Late Resident Physician at the Royal Hospital for Sick Children, Edinburgh.

Congenital occlusion of the rectum is not unfrequently met with; and whenever the continuity of the bowel is so interrupted, early discovery is of the greatest importance. The following malformation may be of interest, inasmuch as it differs, in some measure, from the cases already described.

On the 6th of June last year, E. A——, a female child, was brought to the surgery, thirty-six hours after birth, by a midwife. The child had not defecated, was very fretful, struggling, and crying constantly. Its abdomen was tense and full. On examination, the anus was found to be perfectly formed; but a membrane closed the lower end of the bowel, through which, with the peristaltic action of the intestines, the dark meconium could be seen now and again. I examined the vagina, but found no meconium there; and, satisfied that there was no way of exit for it, a crucial incision was made, and the child ordered a little castor oil, also to be brought back to the surgery in the evening, when the bowels had ceased discharging. The angular flaps were seized and tacked to the margin of the anus by four silver-wire sutures, and in the course of a few days adhesion had taken place. To obviate any tendency to contraction and narrowing of the orifice, an ivory clyster-pipe was orordered to be frequently used, anointed with oil; but from carelessness in its use the adherent flaps were again detached from the anal margin, and the bowel was again loose and movable within the anus. This, however, did not prove so great a detriment as might be expected; for the pipe could easily be inserted, and, with the exception of a few times that the bowel got twisted a little to one side, the child managed to relieve itself remarkably well. Injections were occasionally given. Several attempts at examination were made; but the peculiarity of the malformation was not decided upon further than that there was no recto-vaginal fistula, and that the bowel did not terminate at the anal orifice, nor yet at the anterior margin of the anus.

On the 24th May I was summoned to the child, in consequence of an attack of bronchitis having set in, and to which it succumbed on the following day. It is now a year within a few days since the operation, and the mother told me that the child had required very little attention of late, but that she had continued now and again the use of the cystic pipe, sometimes at intervals of three weeks.

Post-mortem examination.—I was allowed to examine the state of matters after death, when the following particulars were noted:—The anus was completely formed, the rectum terminating in a cul-de-sac, which turned upwards; its posterior wall was adherent to and incorporated with the recto-vaginal septum, from the anterior margin of the anus, extending upwards for a little more than two inches, and easily admitting the finger. This sac contained feculent matter with a very strong odor. Having cleared this away, I injected milk into the vagina, and was satisfied that no fistula existed. I now noticed the opening into the bowel; it was rounded, with a slightly thickened margin, and did not appear less than it was nine months ago. At the seat of the operation the bowel was of normal calibre, but the prolongation was somewhat smaller. No traces of inflammation were observed.

Remarks.—My object in placing this case on record is simply for the peculiarity of the malformation, since, after careful investigation, I have not met with a single instance of malformation of the rectum having this peculiarity. The occlusion was due entirely to the prolongation of the bowel, and its attachment to the septum. The cases of this kind generally met with are characterised by a deficiency of bowel, some few opening into other canals, as the vagina, bladder, or urethra; seldom have cases exhibiting a redundancy of bowel been observed. (See Ashton, Curling, &c.) I may here mention that an hypertrophied condition of the terminal end of the rectum has been known to occur, owing to excessive distension from superincumbent pressure of meconium, in cases that have remained for a long time unrelieved (Curling); but the discovery was soon made in this case, and no traces of inflammatory action remained. Moreover, the union was as perfect as in the case with webbed fingers or toes so commonly met with; but there was no history of congenital deformity in the family, so far as I could ascertain.

The operation was a failure. If the child had lived and matured, in all probability this condition of the bowel would have proved a serious inconvenience. It had thriven, and enjoyed perfect health for nearly a year; but I am inclined to think that such would not have continued long. A secondary operation might have been attempted—viz., to attach the bowel again to the anus, with a chance of its ultimate success, at the same time enlarging the opening in the direction of its anterior attachment to the anus. Time, however, was not given for the carrying out of this project; and so long as the child continued in health, and was getting on so favorably, there might have been some advantage in deferring it.

Prompt interference was necessary, and relief was soon obtained by the incision; but the subsequent treatment being the most important, great care and precision are necessary in inserting the dilator for some time after the operation.

Bradford, Yorks, July, 1871.

ON A

NEW AND SAFE METHOD OF PERFORMING ACUPRESSURE.

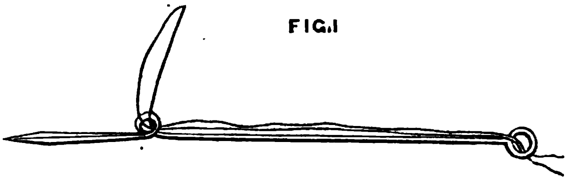
By R. Clement Lucas, F.R.C.S.,

Late House-Surgeon to Guy's Hospital.

It is now some time since acupressure was introduced at Guy's Hospital; but at the time it was there practised I had an opportunity of observing that it was not unfrequently followed by hæmorrhage, either from imperfect application in the first place, or from the wires and pins becoming loose from some cause, or from attempts made to remove them too soon after the operation. I then contrived the method of securing vessels that I am about to describe, which renders acupressure more simple and quite as safe as a ligature, and has the advantage of being equally certain in the hands of a novice as in those of the most expert professor of acupressure. The subsequent introduction of torsion, and the rapid conversion of all Guy's surgeons to that mode of securing vessels, prevented my drawing attention to it at the time; but finding that acupressure is still largely practised and advocated by surgeons of the greatest eminence, I have thought it right to lay this method before the profession.

An ordinary acupressure needle, having a ring at one end, is twisted so as to form a small loop about an inch from its pointed extremity, or at such a distance as the surgeon may think most convenient, varying with the size of the needle. A piece of fine wire is doubled in the middle as in ordinary acupressure, and the loop thus formed is passed first through the ring at the end of the pin, then through the loop on the staff. After sufficient has passed to reach easily over the point of the needle, the loop of wire should be bent up at right angles so as to be out of the way, as shown in Fig. 1.

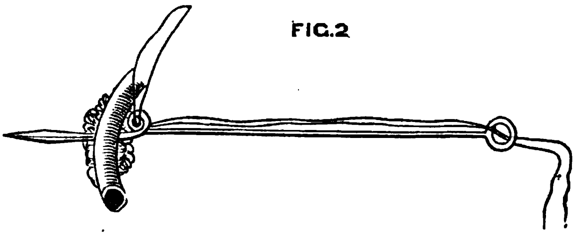
FIG. 1



In this way needles should be kept ready for use.

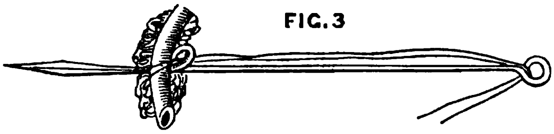
In performing the operation for securing the vessel there are three acts. First, the point of the needle is passed under the vessel, as shown in Fig. 2. Secondly, the loop of the wire is bent down over the vessel and slipped over the point of the needle. Thirdly, the vessel is compressed by

FIG. 2



pulling the free ends of the wire tightly through the ring at the extremity of the needle, and finally secured by bending back the wire at this point, as shown in Fig. 3; or it may even be made more safe by giving the ends of the wire a twist round the needle close to the ring.

FIG. 3



To remove the pin, all that is necessary is to straighten the wire bent round the ring at its outer extremity; then on gently pulling the pin the loop of wire glides along towards the point and releases the vessel. Should bleeding now take place, the vessel may again be compressed by pulling on the wire, provided that the pin has not been so far extracted as to have allowed the loop of wire to pass over its point. After the pin has been removed the wire follows, as in the mode of acupressure described by Professor Pirrie under the name of "circumclusion."

I venture to claim for the above method the following advantages: 1. That it secures the vessel against all chance of bleeding. 2. That it permits the pressure upon the vessel to be regulated. 3. That, when about to remove the wire, it allows the surgeon to ease the vessel and again tighten the wire upon it in case of bleeding without reopening the stump, which it is not possible to do by any of the methods now employed.

Littlecote Hall, Hungerford, July, 1871.

◆

ON A NEW FORM OF NEEDLEHOLDER.

By Samuel Theobald, M.D.,

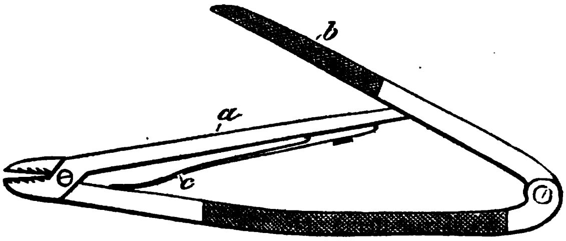
Of Baltimore, U.S.

The needleholder, of which the accompanying cut gives a correct idea, I have had made more especially for ophthalmic surgery, where delicacy of manipulation is particularly desirable.

The holder generally used by ophthalmic surgeons is, I believe, the invention of Dr. Sands, of New York. It is so arranged as to fasten, when closed, by a spring catch. This arrangement has some advantages, enabling an assistant to place the needles in the holder, and pass it when desired to the surgeon, or permitting the latter perhaps to put the instrument out of his hands, if he may so wish, without the needle becoming displaced. Its disadvantages, however, I think, more than counterbalance these, especially in delicate operations on the lids, in Critchett's abscission, or in cases where it is desirable to stitch the conjunctiva. A more or less rude effort is required to release the needle. If this be large, the effort must be increased, as it will be held more tightly in proportion to its size. If, on the contrary, the needle be very small, the blades may fail to grasp it firmly, because not held sufficiently close together by the catch. It is also often desirable to let go the hold of the needle, and take a second while the stitch is being made. Under such circumstances, it is very provoking to be compelled to fasten and unfasten the holder each time, and yet this is unavoidably necessary. As originally made, Dr. Sand's instrument had the further disadvantage of frequently breaking the eye of the needle by the sudden snapping-to which takes place as the catch fastens. This, however, has been remedied by coating the grasping surfaces with lead.

In the instrument which I have designed, I have endeavored, by doing away with the spring catch, to get rid of these faults. At the same time, I have so modified and increased the leverage that the needle may be held with great firmness without any effort (which might unsteady his hand) to the operator. With it a very slight motion of the thumb only is required to grasp or let go of the needle. The closure of the blades is regulated by the hand, so that whether the needle be large or small it is grasped with equal firmness. Again, as the snapping-to of the instrument is avoided, there is no danger of the eye of the needle being broken, and hence the lead coating is not required. It is, moreover, simpler in construction and less expensive than the old form, both of which are points in its favor.

Grooves should be filed in the grasping surfaces, so that the needle may be held firmly in different directions. The extremity of the lever (*a*) should be shaped so as to play in a narrow groove about half an inch long in the under-surface of the lever (*b*). This groove should end abruptly, and the extremity of the lever (*a*) playing in it should be so shaped as to catch firmly against the end of the groove, thus keeping the instrument from being opened too widely by the action of the sping (*c*). The construction of the instrument in other respects is shown by the cut. It should be about four and a half inches long, and of the proportions there shown. The exact size is somewhat a matter of individual fancy, but I think this will be found most convenient, as best suited to the grasp of the hand, although some might consider it unnecessarily large.



Mr. Soelberg Wells, to whom I presented one of the first instruments made, has tested it practically on several occasions, and expresses himself as very much pleased with its action. They may be obtained of Messrs. Weiss, Strand; of Schleifer in Vienna; and of Lüer in Paris.

Harley-street, Cavendish-square, Oct. 1871.

◆

A CASE OF SALIVARY CALCULI.

By J. Wyatt Pratt, M.R.C.S. Eng., &c.

L. P——, a retired medical gentleman, aged fifty-four; has been lately in good health, but formerly exhibited indications of strumous diathesis. Twenty years ago he suffered from symptoms of phthisis, and coughed up, during much bronchial irritation, several chalky concretions. Has passed, at varying intervals of a few years, without much discomfort, small single calculi from the left submaxillary duct. After three or four years' relief, on the 6th and 7th of July last he commenced to void many small calculi from the same situation, accompanied with painful mastication and uneasiness on the floor of the mouth.

July 8th.—Has had a bad night. At 10 A.M. he was found with both parotid, both submaxillary, and both sublingual glands much inflamed and enlarged; tonsils and uvula much increased in size, rendering deglutition very difficult, all food regurgitating into the posterior nares unless the nose was tightly held during attempts to swallow; tongue considerably enlarged, and covered with creamy fur; power of articulation almost lost; elevation at the termination of the left Whartonian duct prominent, and the duct itself distended by a small calculus, which was removed by Dr. Edwards (my senior partner), through the natural orifice, with a probe and pair of fine forceps, after which a quantity of saliva escaped and a little bead of pus. Opposite to the left lower first molar was a transparent vesication on the mucous membrane, over the sublingual gland, upon puncturing which a quantity of clear saliva-looking fluid escaped. Skin hot; pulse full and bounding, nearly 120; bowels confined; both cheeks puffy and much distended; breath very offensive, and saliva dribbling freely from the mouth. Ordered, two grains of calomel immediately; to inhale steam; brandy and milk diet; and twenty-five minims of chlorodyne at night.

9th.—Had a better night, and brought away two or three more calculi. A square block of white, smooth, firm lymph, as large as the crown of a molar tooth, had been secreted from the surface where the vesication had existed, and was now detached as it produced discomfort. Duct freely discharging saliva. Pyrexia diminished.

10th.—Restless night. Duct blocked up again; a blunt probe was introduced, and this had the effect of removing a little plug of sanious lymph, which was followed by a quantity of offensive pus. No calculi since yesterday. Block of lymph on the surface removed as before.

11th.—Continual discharge of healthy-looking pus from affected duct. Papilla at its entrance hard, and as large as a No. 1 shot. Block of lymph again requiring removal. Swelling reduced.

13th.—Swallows better. Left submaxillary gland still enlarged and tender; other glands diminished to natural size. Free discharge of pus. Ordered nitric acid and liberal diet.

Went on improving until the 22nd, when the largest calculus ulcerated its way through the duct, about half an inch from its mouth, and was easily removed with a pair of forceps.

From this time he progressed favorably. The fistulous opening caused by last calculus soon healed, and the discharge gradually ceased, though the floor of the mouth remained tender and irritable for three or four weeks.

The number of calculi passed during the fortnight was thirteen, varying in size from that of a good-sized pin's head to the largest, which is rather bigger than a full-sized grain of wheat, and was the only one that did not escape from the natural aperture of the gland duct. They are of a pale-fawn color, roundish, slightly rough, and somewhat porous, and of very light specific gravity.

Wiveliscombe, Aug. 31st, 1871.

A Mirror

OF THE PRACTICE OF

MEDICINE AND SURGERY

IN THE

HOSPITALS OF LONDON.

> Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

HOSPITAL FOR SICK CHILDREN.

CASES OF CHOREA TREATED WITH SULPHATE OF ZINC.

(*Communicated by* Mr. H. T. BUTLIN, Medical Registrar.)

THE following cases are selected only so far as they illustrate a particular mode of treatment. They comprise nearly all, if not the whole, of the cases of chorea which have been treated with gradually-increasing doses of sulphate of zinc at the Children's Hospital during the past twelve months. Although they do not show a complete success, they appear to merit consideration—in some instances, because the zinc salt was employed after other means had been fairly tried and had failed; in others, on account of the rapidity with which recovery followed its exhibition. In no single case, moreover, was its use unattended with some advantage; for those patients who did not entirely recover on the sulphate of zinc only, appeared to derive considerable benefit from it. The tolerance of the drug, which may be established in what would appear to be decidedly emetic doses, is well shown in every case. Some other points of interest in the history of chorea which these cases would well serve to illustrate have necessarily been scarcely touched, as time and space only admit of the report of such details as relate to the mode of administration and the effects of the drug in question. The temperature in most cases was found to range a little above the normal—generally between 99° and 100°; whilst in Case 11, for more than a fortnight it continued at or above 100°. The combination of hysteria and chorea, in one or two instances, made it somewhat difficult to decide which of the two was the primary disease; while the length of time through which the disease may extend is well shown in Minnie S——.

As regards the indication for giving the sulphate of zinc, it would appear that there does not exist any particular class of cases of which it can be definitely asserted that they will improve under zinc. In some it has been given because there was no direct indication for any other plan of treatment; in some because other treatment had been tried without success. One thing is certain—namely, that the rough, harsh skin, not unseldom observed in choreic patients, becomes soft and smooth when the administration of the drug is persevered in.

The mode of administration has been much as follows:—One or two grains of sulphate of zinc are given in half an ounce of water three or four times a day; occasionally to this is added, in anæmic patients, a grain or two grains of the sulphate of iron. The quantity of zinc is then increased by the addition of a grain every day, or every other day, until either the choreic movements have very decidedly diminished, or until the medicine has caused excessive sickness, when either the quantity of the medicine is gradually lessened or its use is at once discontinued.

The dose is usually given after a meal, and in one or two instances where sickness has followed a small dose, it has been discovered, on inquiry, that it was given shortly before the early morning meal. In several cases where a few grains had caused vomiting, tolerance has been established, and the ordinary mode of administration has been resumed after the omission, for a few days, of the progressive increase of the dose. Several times diarrhœa has occurred, but this appears to have been rather a coincidence than an effect of the medicine, for it was not arrested by the mere cessation from the zinc, and many of the other children, not taking the sulphate, were suffering from looseness of the bowels about the same time.

On referring to the latter cases it will be seen that the daily increase of zinc has been at the rate of three grains instead of one, as in the earlier cases. The improvement has certainly been more rapid during the larger increase, and it did not appear to cause any inconvenience to those few patients in whom it has been tried.

Lastly, as regards diet and confinement to bed, the children have been usually kept recumbent, and fed on pounded meat or beef-tea and milk, during the earlier stages of the treatment; but as the irregular movements decreased, and without reference to the quantity of zinc then taken, they have been put upon meat diet and allowed to get up. Stimulants, too, have always been allowed in those cases which appeared to require them. One fact has often been noticed—namely, that if no other treatment be adopted than mere confinement to bed, the choreic movements will often frequently diminish in a marked degree during the first week or ten days, after which no further improvement takes place.

Whether the sulphate of zinc act as a nervine tonic, or whether, as Dr. West has expressed his opinion in his late lectures at the College of Physicians, it may exert "a specific power over chorea," is a question which still awaits decision.

(Under the care of Dr. Dickinson.)

Case 1.—Emily C——, aged eleven years, was admitted with the following symptoms: a frequent, short, hysterical cough, very like a bark; a constant choreic jerking of the body and limbs, which affected sometimes one side of the body and sometimes the other, and, when most marked, was often accompanied by two or three short coughs. She was a fairly nourished girl, and remarkably intelligent. The heart-sounds were normal. Her history showed that thirteen months previously she had been an in-patient of the hospital with symptoms precisely similar; she had then improved slightly under treatment upon her discharge had relapsed, and continued in the same condition which she presented on admission.

Two days after admission she was put upon two grains of sulphate of zinc, with two of the sulphate of iron, in half an ounce of water, thrice daily, with meat diet and two ounces of wine. The dose of zinc was increased by one grain daily until it reached twelve grains, the quantity of iron remaining as at first. Her cough was then less frequent, and the choreic movements were not so violent. In eighteen days more the maximum dose of twenty-six grains, three times a day, was reached, and a very decided improvement had been attained both as regarded the cough and the movements. In two more days the medicine was stopped in consequence of continued vomiting. In three days it was resumed in twenty-grain doses, which were gradually diminished by one or two grains daily, until, fifty-nine days after the commencement of the treatment, she was placed on a grain of valerianate of zinc in half an ounce of infusion of valerian three times daily. On the following day she was discharged, but she still continued to have an occasional slight cough (no longer of a barking character), and the choreic movements were yet observable at times. On this account she was readmitted three weeks later, and placed on a grain of valerianate of zinc in half an ounce of water. In seventeen days she was discharged well.

Case 2.—Emily F——, aged nine years. Had always been a delicate child, and about one year previously had had rheumatic fever. This was followed by a first attack of chorea, which lasted for four months. Since then she had continued well until a month before admission, when she again became subject to choreic movements. When admitted she had well-marked chorea, affecting the body and limbs generally, but more especially the upper extremities. A faint systolic murmur, probably hæmic, was audible at the base of the heart.

She was at once put upon two grains of sulphate of zinc in half an ounce of water, three times a day; and, as in the former case, the quantity of the salt was increased by the addition of a grain either every day or every other day, until, on the twenty-fourth day, she was taking twenty-six grains of sulphate of zinc three times a day. From the first she steadily improved, and by the time that nineteen or twenty grains had been reached she was almost free from irregular movements.

From twenty-six grains the quantity was gradually reduced to ninteen on the thirty-first day. In four days more she was discharged quite recovered.

Case 3.—Ellen G——, aged ten. Had been ill for about eleven days before admission. She first made complaint of pains in the limbs. These were speedily followed by general choreic movements. She had always been a nervous, excitable child, but had never had an attack of chorea before. There was no history of rheumatism or of fright. The mother was said to be liable to suffer in a slight degree from rheumatism.

The patient was well nourished, with light hair and long eyelashes. Her face was very pale. She had general, but not very violent, choreic movements. A soft systolic murmur was audible at the base of the heart and along the course of the large vessels. It could also be heard at the heart's apex. The heart's action was irrgular. As the bowels were confined and her tongue was furred, a dose of calomel and jalap was given.

On the sixth day she was ordered a draught containing one grain of sulphate of zinc and one of sulphate of iron in half an ounce of water, to be taken three times a day. On the sixteenth day the quantity of sulphate had been gradually increased to seven grains, and the choreic movements had grown more violent than before.

The dose was still further increased until, on the thirty-eighth day, she took twenty grains. By this time the chorea was much diminished. She had not been sick, and the bowels had throughout been constipated, but were easily moved with the aid of aperients.

Twenty-two grains of zinc on the forty-sixth day made her sick, and on the following day the drug was discontinued, and ten grains of reduced iron, three times daily, was ordered instead. She continued to improve, and left the hospital at the end of three months, having spent three weeks at the Convalescent Hospital at Highgate. She had however, been free from choreic movements for several weeks.

A DRESS-HOOK LODGED IN THE LARYNX; REMOVAL BY BRONCHOTOMY; DEATH ON THE SEVENTH DAY.

(Under the care of Dr. Gee.)

For the notes of the following case we are indebted to Mr. H. T. Butlin, the medical registrar. Five day before admission, the patient, Lily A. A. ——, a healthy child of eighteen months, was pretending, in her play, to drink out of a jam-pot which her mother, a dress-maker, positively asserted to have been empty, Suddenly she uttered a scream, and appeared to be choking. From that time her breathing became difficult; she had occasional fits of choking, and her voice remained hoarse. She could lie down, had been sick the night before admission, but had not brought up any blood. On the day following the first appearance of these symptoms she was examined at one of the general hospitals; but she was not at the time suffering from dyspnœa, only a few bronchial râles were to be heard, and she was prescribed for accordingly.

On admission she had lost something of the healthy appearance said to be habitual to her; she sat up in the nurse's arms breathing loudly with ex- and in-spiratory dyspnœa, and with dilated nostrils. There was retraction of the episternal and epigastric regions. The face was much drawn, and dusky; the lips were of fair color. She had a cool skin, a clean tongue, and a throat of natural appearance. The two sides of the chest were symmetrically affected, while the percussion-note was universally good.

Notwithstanding the assurance of the mother that the jam-pot had been quite empty, the symptoms were so suggestive that Mr. Sankey, the house-surgeon, at the request of Dr. Gee, at once proceeded to perform tracheotomy. Having opened the trachea from the thyroid body upwards, the operator perceived a body which, after clearing away a quantity of thick, puriform matter, he was able to seize with forceps, but not to move on account of its firm attachment. He therefore prolonged his incision upwards through the cricoid cartilage, and found that an ordinary dress-hook was attached to one of the vocal cords. A hook of this description, as is well known, is provided on either side of the base with an almost circular loop formed by the curving outwards and upwards to the side of the stem of the respective ends of the reduplicated wire, and by means of which it is sewn on to the dress. So firmly were the tissues gripped between one of these recurved ends of the wire and the stem of the hook, that it was found necessary to divide them with the knife. The child was sick during the operation, and looked pale and ill for some time afterwards.

About three hours after the operation, the breathing being somewhat labored, a tube was passed into the tracheal opening with manifest advantage. In the evening the patient vomited several times, but afterwards passed a good night. On the following (second) day the tube was removed at mid-day. She breathed fairly well, inspiring through the wound, and expiring through the mouth and nose. With the exception of the edges of the wound being red and presenting a pouting appearance on the third day, and the discharge of a good deal of thick puriform matter, she continued to make favorable progress until the sixth day, when the tube had to be reinserted on account of difficulty of breathing. This measure gave relief for a time, but in a few hours the difficulty became steadily greater, and neither the introduction nor the removal of the tube, nor any other proceeding, afforded any relief. After suffering great dyspnœa, chiefly expiratory, she died on the seventh morning.

At the post-mortem examination the body was found to be well nourished. The wound gaped and looked unhealthy; it emitted thick, brown, frothy matter. The epiglottis was slightly injected; both vocal cords were much congested; in the centre of the right one was found the notch caused by the removal of the hook. The trachea contained a considerable quantity of thick semi-purulent fluid; its mucous membrane was congested. Both lungs were emphysematous anteriorly, and œdematous throughout. The bronchial tubes contained a great quantity of thick tenacious matter, and bore evidence of congestion. There was no collapse, and there were no traces of pneumonia.

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ST. GEORGE'S HOSPITAL.

CASES UNDER THE CARE OF DR. OGLE.

Case of Epilepsy and Hemiplegia, probably syphilitic.—Susan S——, a married woman, aged thirty-three, pale and thin, who had enjoyed general good health until three months ago, when she became subject to "lumps" on the right side of the scalp. After they went away, she became liable to "fits." These have generally come on once a week, and are preceeded by a "funny numb sensation" in the right hand, and last five or six minutes, during which time consciousness is completely lost. It appeared that the memory had not been affected; neither speech nor deglutition, nor the power of the sphincters, had been interfered with. At first the tongue used to be bitter, but it had not been so lately; it was protruded straight. On admission the right arm was in a sling, and the power of moving it was very defective, as also of moving the right leg; but no muscular rigidity existed. The power of the left limbs was entire. Everywhere sensibility of the skin was unimpaired, as also the sight, sense of smell, and hearing. The pupils and the movements of the eyes were natural. There had been no spasm or twitching of any of the muscles. The condition of the lungs and heart was natural. Although a syphilitic history could not be elicited, it would seem that probably this disease was at the root of the mischief. The patient is taking iodide of potassium.

Hemiplegia, probably from softening of embolic origin.—Mary R——, aged forty-five, a married woman, had suffered pain in the limbs for some days. She was anæmic, with a weak but regular pulse, and white and coated tongue. A systolic bruit was audible at the apex of the heart. The urine was acid and free from albumen. She had had rheumatic fever thirty years previously. A little Dover's powder at bedtime, and some senna for the next morning, were ordered. On the day after admission she was cheerful and natural at the early morning visit of the house-physician, but at one o'clock she was barely conscious. The left eye was partially closed, the mouth was drawn to the right, and the right arm and leg were paralysed. She has continued more or less in the same state ever since, showing little or no sign of pain or uneasiness, and but little consciousness at any time. There has been dysphagia and some stertor, at one time worse, at another better; and the urine, which has had to be drawn off from time to time, has become alkaline and slightly purulent. Bedsores also have made their appearance. A very interesting circumstance is that the temperature, which Dr. Ogle has regularly taken on both sides of the body ever since admission, has remained higher in the paralysed than the other side; on one or two occasions the difference was as great as four degrees.

Poisoning by Sulphuric Acid.—A woman took into her mouth a quantity of the acid by mistake, but appears to have spat it out immediately. She was at once treated with magnesia, and two hours afterwards she was brought to the hospital complaining of intense burning pain in the throat and stomach in the region of the œsophagus. The lips were greatly swollen and blistered, and the soft palate and uvula were observed to be congested and in places sloughing. The lining of the mouth otherwise was whitish, but not excoriated. Olive oil and bicarbonate of soda were given at intervals by the house-physician, Mr. Risdon. In the evening vomiting of an opaque milky substance commenced, and continued for a little time. Ice in fragments was then given. Two days after admission there was some difficulty in breathing, and Dr. Ogle feared that œdema of the glottis had set in. The pain in the throat was, however, less. Great relief was afforded by the sucking of ice and its application around the throat, and chlorate of potash frequently used as a wash. The patient's strength was maintained by suitable liquid food and small doses of brandy and soda-water. All the symptoms gradually subsided, and, after the use for a few days of quinine and iron, the patient was discharged well.

Poisoning by the Ammoniated Mercury.—The patient was a young woman who had taken a pennyworth (thirty or forty grains?) of the drug, under the impression that it was carbonate of soda. She was at once brought to the hospital, and Mr. Risdon gave an emetic of half a drachm of sulphate of zinc, which he followed up by an egg-and-milk mixture *ad libitum.* After the use of ice and effervescing salines, with a little opium, she soon became convalescent. The symptoms which she mainly complained of were pain at the epigastrium and a peculiar twitching of the muscles of the left arm and leg. For four-and-twenty hours the spasms were very manifest.

Hæmaturia.—The patient was a railway porter, aged sixty-one, and had previously been healthy. He had a yellowish tinge of face. About three months before admission he noticed a little blood in his urine; it gradually increased in quantity until, on admission, he appeared to pass almost pure bright-red blood. Of late scalding in passing the urine, and pains in the back, had existed. The hæmorrhage was worse at night, the urethra being often blocked up by coagula. He had been for a month an out-patient. On admission there was no pain in the groin or down the thighs, no retraction of or pain in the testicle; no gravel; no history of any calculus having been passed; and no tumour of the abdomen could be felt. A warm bath was ordered and aperients given, and strict rest enjoined. The next day the urine was much less bloody. The passage of blood and the pain in passing it daily diminished, and Dr. Ogle, wishing to try the effect of rest, gave no medicine except a senna aperient, and ordered a warm bath. Twelve days after admission the blood had entirely ceased, and not even could any albumen be detected in the urine. The tincture of perchloride of iron was given for some days afterwards, and the patient went out recovered. Dr. Ogle caused the patient to take one or two long walks in the park, but no hæmorrhage followed. He has, since his departure from the hospital, returned to show himself, but hæmorrhage had not recurred. At one time Dr. Ogle was about to have the bladder and prostate examined by the sound, &c., but Mr. Lee thought it better not to interfere mechanically.

Three Cases of Chorea.—The first was that of a healthy young woman, aged nineteen, who had been married lately, and was three months pregnant. Quickening had not been experienced. Without any apparent cause, such as fright, mental emotion, worms, or rheumatism, chorea, chiefly affecting the left side, came on about three weeks before admission. This was the first attack. She was treated by ether spray applied along the vertebræ at intervals. A second case was that of a girl, aged fourteen, in whom the left side was much affected. This was the third attack. No ostensible cause for the affection existed. She is taking the liquor arsenicalis. Both these cases are still under treatment. The third case was that of a youth, aged eighteen, who had been in perfectly good health until two months before admission, when twitchings of the left arm came on. The left side of the face then became affected, and these parts have remained so. He first had a calomel and jalap purge, and subsequently was treated by five-drop doses of solution of arsenite of potassium, followed by the cold shower-bath daily, with good diet. He left the hospital almost entirely well.

A case of *paraplegia*, in an old man, which had come on suddenly a few weeks before admission, apparently in connexion with constipation. The sensibility of the skin was not apparently impaired, nor were the sphincters affected. Under the use of aperients, followed by increasing doses of the solution of strychnia three times daily, he steadily improved.

A case of *œdema of the lungs* in heart disease much improved by the use of super-tartrate of potash given as a diuretic linctus: and a case of *gout*, with gouty deposits in the ears, relieved by the citrate of lithia, which acted strongly as a diuretic.

Diminution of *ascites* from disease of the liver under the use of iodide of potassium and soap liniment applied over the abdomen.

A case of *diseased heart* in which most intense dyspnœa existed, the only bearable posture being that of kneeling. Much relief followed hæmoptysis. After death very contracted mitral orifice was found, but during life no cardiac bruit had been detected.

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GUY'S HOSPITAL.

WARTY GROWTHS IN THE LARYNX; REMOVAL BY BRONCHOTOMY; RECOVERY.

(Under the care of Mr. Thomas Bryant.)

We are favored with the notes of the following two cases by Mr. R. S. Mutch.

A. T——, a healthy-looking boy three years of age, was admitted with complete loss of voice. On examination, some slight swelling, more appreciable to the touch than to the sight, was observed over the external surface of the larynx. Neither the skin nor the adjoining lymphatics were affected. The swelling was hard and immovable.

According to his mother's observation, he had first become hoarse about sixteen months previously, and for about fourteen months had been quite voiceless. His health had always been good. There was no family history as regarded tumours, but abundant evidence of phthisis. The loss of voice was attributed to his having kicked off the bedclothes and lain for some time as cold as a "piece of ice."

Having come to the opinion that the child's larynx was occupied by adventitious growths, Mr. Bryant made first an opening in the median line sufficient to admit of the introduction of a tube into the trachea, and then laid the larynx open by an incision from within and below. Considerable hæmorrhage occurred, but was arrested by torsion. Innumerable warty growths, springing from the epiglottis and rima, were then removed with dressing-forceps and the handle of a scalpel. The interior of the larynx was then sponged with tincture of perchloride of iron, the tube tied in position, and the incision above it closed with sutures. During the operation the complexion became livid and the respiration slow, but on its completion these symptoms rapidly subsided. On being removed to the ward, the patient was placed near a fire, a screen was placed round the bed, and a bucket of steaming water kept constantly at its side. On the fifth day the tube was removed, and a good deal of discharge issued from the opening; he was found able to express himself in a whisper, On the twenty-third day he was discharged; the wound had closed, the respiration was carried on without difficulty, and the voice had undergone a great improvement. He was seen again sixty days after the operation; the voice had become much more natural, and there existed not the slightest obstruction to the respiration.

REMOVAL OF CONGENITAL FIBRO-CELLULAR TUMOUR IN THE BUTTOCK.

(By Mr. Bryant.)

John B——, aged fifty, stated that from birth he had had a tumour on the right buttock, near the median line. Until within two or three years it had neither undergone increase nor caused inconvenience. About two months before admission it began to increase rapidly and became red, hot, and painful; shortly afterwards it broke and discharged matter with blood. Still it continued to grow rapidly larger; it continued also to exude blood and matter, but only gave pain when subjected to violence of some kind. There was found to be on the right buttock a tumour eleven inches and three quarters in circumference at the base, and eight inches diametrically over the summit. The skin was somewhat red and thickened. Its summit was flattened, and presented a dark, elevated patch about two inches and a half in diameter, with undetermined, but not everted, edges; it emitted fetid sanguineous matter. The whole was movable on the solid parts beneath; but an indurated condition of the subcutaneous tissue extended to a distance of about an inch from its base. It did not encroach beyond the median line; gentle manipulation produced no pain, and its bulk did not in the least degree interfere with the movements of the leg.

The tumour was excised by means of a circular incision; the bleeding was slight, and was easily controlled by torsion. The patient made an uninterrupted and complete recovery.

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ST. BARTHOLOMEW'S HOSPITAL.

SANGUINEOUS TUMOUR OF THE NECK; HÆMORRHAGE FROM RUPTURE OF THE EXTERNAL CAROTID ARTERY; DEATH.

(Under the care of Mr. Savory.)

We are indebted to Mr. P. Butler Stoney, house-surgeon, for the notes of this and the following case.

The patient was admitted with a tumour, of about the size of an orange, situated beneath the lower portion of the right sterno-mastoid muscle which it pushed forwards. He was forty years of age. He stated that he had first noticed the swelling two months previously, when it was about as large as a filbert. During the last fourteen days his voice had become husky; he experienced a great deal of pain in the back of the head, but had no difficulty in swallowing. The tumour yielded a sense of fluctuation to the touch, and a bruit on the application of the stethoscope. The introduction of a fine trocar resulted in the evacuation of a few ounces of thin sanguinolent fluid. On the cyst being laid freely open three days later, the common carotid artery could be plainly felt to beat against its inner wall—the bottom of the wound. For eight days the wound was daily washed with a weak solution of Condy's fluid, and plugged with lint in order that it might heal by granulation from below. The thermometer placed in the axilla never rose above 98°.

At about noon of the ninth day a stream of blood, nearly of the calibre of a little finger, rushed from the wound. It was almost immediately checked by digital pressure, but not before a loss of over three pints had been sustained. Mr. Savory forthwith proceeded to tie the carotid about an inch below its bifurcation. The wound was then plugged with lint and tightly bandaged. The patient rallied a little at first, but afterwards exhibited vomiting, jactitation, a cold skin, and feeble pulse, until he died on the following morning.

After death there was found softening of the external carotid artery, from its origin upwards to its superior thyroid branch, and between these points it exhibited a large irregular opening. The common carotid was healthy at the seat of ligature, but elsewhere much diseased. The whole of the thoracic and abdominal aorta was extensively diseased, and the arch of this vessel had undergone dilatation to about twice its normal calibre. Both the aortic and mitral valves were puckered sufficiently to allow of regurgitation.

A CASE OF TRAUMATIC DELIRIUM; RECOVERY.

(Under the care of Mr. Savory.)

In the following example the disease which has been variously designated as "traumatic delirium," and "prostration with excitement," supervened with remarkable celerity on the injury, and, by yielding with almost equal speed to a single dose of morphia, confirmed the established practice of treatment by opiates.

George D——, forty-two years of age, was admitted in a state of unconsciousness produced by the fall of a heavy beam of wood on his chest. Very shortly afterwards he passed into a state of delirium. The subcutaneous injection of half-a-grain of morphia was followed in a few minutes by quiet sleep. In a few hours he resumed consciousness, and complained of great pain in the chest. Though conscious, he remained dull and forgetful; when told to hold a thermometer in his mouth, he expressed compliance, but in a few seconds allowed it to fall. On the third day, the application of a linseed-meal poultice to the abdomen was followed by an improvement in his condition. Half an ounce of castor oil, administered on the fifth day, cleared his tongue of a thick brown fur, and effected a further general improvement. On the fourteenth day he was discharged, well.

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UNIVERSITY COLLEGE HOSPITAL.

REDUCTION OF OBESITY PREPARATORY TO OPERATION.

(Under the care of Mr. Berkeley Hill.)

The patient is the subject of a reducible inguinal hernia. He is a blacksmith by trade, and a man of active habits; though endowed with a well-developed muscular system, he suffers from extreme obesity, in consequence of which all attempts to control the hernia by means of trusses have failed. He is five feet nine inches in height, and weighed, on admission, eighteen stone six pounds. It is proposed to adopt Lister's antiseptic operation for the hernia, but, as a preparatory measure, an effort is being made to reduce the excessive deposit of adipose tissue. His diet consists of six ounces of meat with greens or turnips *ad libitum* for dinner; and tea without milk, though slightly sweetened, with bran biscuits, for breakfast and tea. In his desire to hasten the object in view, the patient usually takes no more than about two-thirds of his allowance of meat. In eighteen days he has lost seventeen pounds of weight. He professes always to have been a small eater, and says he has not suffered hunger since the first two or three days of the limited regimen.

CASES UNDER THE CARE OF MR. CHRISTOPHER HEATH.

Among others we noticed the following cases which had been under Mr. Heath's care during the vacation:

A Case of rapidly recurring Epithelial Cancer in a young subject.—The patient is no more than twenty-one years of age. In February last he was admitted with an ulcer of the lip of doubtful appearance, which improved considerably during a course of iodide of potassium. On his presenting himself again a short time later, the ulcer was found to have undergone a decided increase, and to present features typical of epithelioma. It was accordingly excised in May last. After the operation the patient was sent to the seaside, and, on his return, a plastic operation was undertaken with a view to improving the condition of his mouth. At the present time he is again an in-patient, with a distinct epitheliomatous growth and ulceration at that part of the upper lip which abjoins the old scar, and with affection of the lower jaw, extending along the alveolar process backwards from the canine tooth, which is ready to fall from its socket.

A Case of Rodent Ulcer.—The ulcer first declared itself thirty-two years ago, but did not begin to make rapid progress till about six years back. It now involves the whole of the left half of the forehead. Towards the centre of its surface the frontal bone appears to have been invaded, and movements communicated to the dura mater by the pulsation of the cerebral vessels can be distinctly observed. The patient is sixty-one years of age.

A Case of Ununited Fracture of the Tibia treated on Bickersteth's plan.—The patient suffered fracture of the tibia in January last, and it is surmised that the failure to secure primary union may be attributed to a severe "fever" from which the patient suffered five years ago in Malta, and which was followed in two years by extensive necrosis of the inferior maxilla. No history of syphilis has been elicited. The fracture is situated in the lower third. A month ago a steel drill was driven obliquely through the two fragments to a distance of five inches, thus pinning them together. On its removal, at the end ot three weeks, sufficient union had been effected for the limb to be encased in plaster-of-Paris with every prospect of complete success.

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ST. MARY'S HOSPITAL.

ANEURISM OF THE UPPER PART OF THE RIGHT COMMON CAROTID ARTERY; LIGATURE OF THE VESSEL; RECOVERY.

(Under the care of Mr. James Lane.)

Edward H——, aged thirty-nine, bootcloser, was admitted June 26th, 1871, with a large pulsating tumour on the right side of the neck, corresponding in position to the upper part of the common carotid artery. He had first noticed a small swelling about nine months previously, which had gradually increased in size until it measured, on his admission, about two inches in the vertical, and three inches in the transverse direction, pushing the os hyoides and larynx over towards the left side. The tumour had a strong and distinctly *expanding* pulsation; its size was diminished by steady compression, and also when pressure was applied to the trunk of the carotid artery below it. The patient complained of some difficulty in swallowing, and of uneasy sensations in the right side of the head and right ear, but his principal annoyance was the strong bounding pulsation in the tumour.

Operation (June 28th.)—A vertical incision was made three inches and a half in length, from the lower part of the tumour to the sterno-clavicular articulation. After division of the deep fascia and ligature of a vein, the inner edge of the sterno-mastoid muscle was exposed. This being drawn outwards, the omo-hyoid, sterno-hyoid, and sterno-thyroid muscles were seen. The first being pushed upwards, and the last two drawn inwards, the thyroid gland was freely exposed, and the artery seen close to its posterior border. A silk ligature was applied to it about two inches above the sterno-clavicular joint, and one inch and a half below the tumour. Neither jugular vein nor par-vagum nerve was exposed.

The pulsation in the tumour was arrested when the ligature was tied, but returned to a slight extent the same evening. On the following day, however, it had entirely ceased, and never recurred either in the tumour or in the branches of the external carotid artery. No symptoms indicating disturbance of the cerebral circulation were at any time observed, but a record of the temperature of the two sides of the head, taken in the auditory meatus, showed an increase to about one degree on the ligatured side for the first two days, after which it soon became level with, and then sank somewhat below, that of the sound side.

There was some suppuration in the wound, but on the whole it healed favorably. The ligature did not separate till the twenty-third day. Two days after the operation the tumour had a firm, solid feel, and was sensibly diminished in bulk. At the end of ten days it was not more than one third of its size before the operation. Its absorption proceeded steadily and uninterruptedly, though his recovery was somewhat delayed by the formation of an abscess at the lower part of the neck, which prolonged his stay in the hospital till August 16th. At that date the tumour had almost disappeared; and on his presenting himself for inspection on Sept. 18th, eleven weeks after the operation, scarcely any trace of it could be discovered.

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MIDDLESEX HOSPITAL.

TWO CASES OF CLEFT PALATE.

(Under the care of Mr. Hulke.)

We are indebted to the house-surgeon, Mr. Davidson, for the notes of these cases.

Cleft of the Soft Palate; Staphyloraphy, under chloroform, with Smith's gag.—A little girl, two years and a half old, was operated on, under the influence of chloroform, by Mr. Hulke, with the aid of Smith's gag. The levatores palati were divided after Fergusson's method. There was but little bleeding, and no sickness. For about a week the case did well; and then, in a fit of coughing, the sutures were torn out, and the cleft was re-opened. Thirteen months after the first operation Mr. Hulke operated again; using, on this occasion, silkworm-gut sutures instead of the ordinary ones of spun silk. On the fourteenth day two of the stitches, and on the nineteenth the remaining ones, were removed. There was perfect union throughout the entire length of the cleft.

Cleft in the Soft and Hard Palates; Staphyloraphy after Sir W. Fergusson's method, and uranoplasty after Langenbeck's.—A girl, seventeen years of age, was admitted with a cleft dividing the velum and also the hard palate as far forwards as the position of the incisive foramen. The maximum breadth of the cleft in the hard palate was half an inch, and the sides rose steeply from the alveolar border. For a few days the patient tickled the roof of her mouth with a feather, with the result of greatly diminishing its sensitiveness. Mr. Hulke united the halves of the velum by staphyloraphy. The great self-control of the patient enabled him to complete the operation in much less time than it usually occupies. The hinder part united immediately; but the front stitches were dragged out, and at this part a slight ulceration arose, which soon healed without doing much damage. Four months later Mr. Hulks closed the cleft in the hard palate, detaching the periosteum with the mucous membrane from the bones, as recommended by Langenbeck, by means of elevators, and separating the upper border of the velum from the posterior margin of the palatine process of the palatine bone. The flaps so made came readily together, and perfect union resulted, excepting at one point where a minute capillary aperture remained. When recently seen (two months after the second separation), the voice had greatly improved.

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ROYAL FREE HOSPITAL.

CASE OF APOPLEXY TREATED BY VENESECTION; RECOVERY.

(Under the care of Mr. Gant.)

For the report of this case we are indebted to Mr. E. R. Morgan, house-surgeon.

Wm. W——, aged fifty, was the driver of a night cab. He had never drunk much spirits, but had taken a good deal of beer, and smoked hard; he was very plethoric, and presented a marked arcus senilus, but no tortuosity of arteries. It appeared that he had been in bed all day, and, having risen at 7 o'clock, went to the stables, where he suddenly fell, and lost all consciousness. He had felt well previously.

On admission he was quite unconscious, and could not be roused; his pupils were dilated and insensible to light and touch; his breathing stertorous; the pulse hard and quick; he was bathed in perspiration, and the countenance was livid. Having carefully considered these symptoms, Mr. Morgan came to the decision to perform phlebotomy without delay, and proceeded to take from the left arm eighteen ounces of blood. As it was flowing there occurred a marked improvement in the symptoms: the breathing ceased to be stertorous; the countenance became less flushed; the pulse weaker. The patient, however, did not recover consciousness; but, as matters progressed so well, he was sent into the wards, where he was ordered to take a minim of croton oil, also five grains of bromide of potassium every four hours, and to have ice applied to the head. His bowels did not act until the fourth day, on which he became conscious, and could answer questions rationally. He has since gradually improved, and at the present time (the thirty-first day) complains of nothing more than occasional, but not severe, fits of vertigo. The patient lost no power or sensibility in any of his limbs.

Editorial.

THE ABSORPTION OF INSOLUBLE SUBSTANCES.

That the old statement "corpora non agunt nisi sint soluta" is not absolutely correct, has long been known. Nearly thirty years ago Oesterlen administered unguentum hydrargyri to cats, and found molecules of the metal in their blood. Herbst believed he had satisfactorily demonstrated the absorption of milk and starch corpuscles into the chyle and bloodvessels; and his views were supported by the experiments of Bruch. Donders and Mensonides mingled charcoal with the food of rabbits, and found particles in the blood drawn from every region of the body. Similar researches were undertaken by Marfels and Moleschott, who gave to frogs and dogs blood-corpuscles of sheep and pigment-corpuscles from the choroid, and saw these corpuscles circulating in the web of the frog's foot. Oesterlen, Eberhard, Landerer, and especially Voit, made a series of careful experiments, all showing that if blue ointment were vigorously rubbed into the shaven skin of cats and dogs, minute particles might be found in other and remote organs of the body—as the liver, spleen, pancreas, kidneys, &c. There have been, indeed, some experimenters—as v. Barensprung, v. Becklinghausen, and a few others—who have obtained only negative results; but, upon the whole, the evidence has been of late years strongly in favor of the possibility of the absorption of solid substances through the uninjured membranes and skin. Quite recently M. Heinrich Auspitz has given in the *Wiener Medizinischen Jahrbüchern* the results of his experiments on this interesting subject. He has employed a substance which, from its small specific weight and its strongly-defined outline and stability, is quite as serviceable as metallic silver and coloring particles. This is rice-starch meal, which is furthermore easily recognisable by its reaction with iodine and the action of polarised light. The largest rice-starch grains are about twenty times larger than the red corpuscles of the rabbit, whilst the smallest are rather smaller than the red corpuscles. He injected white starch meal as well as starch meal colored blue with iodide. His first series of experiments consisted of injecting the starch powder into the external jugular vein, the vena cava inferior, and the pulmonary artery, with a view of testing how far it could be recognised when diffused through the tissues of the various organs of the body; and he obtained the general result that the presence of the granules could be almost everywhere easily demonstrated by the appropriate tests mentioned above—as in the lungs, right heart, liver, spleen, kidneys, and brain, and in hæmorrhagic exudates. In subsequent experiments he injected starch-molecules suspended in water into the abdominal cavity, and into the subcutaneous connective tissue; in other cases the starch was suspended in oil, and similarly injected; and finally, in another series of cases, the contents of the ductus thoracicus were carefully investigated after injection of the starch into the peritoneal cavity and into the subcutaneous connective tissue. The general results of all these experiments may be thus briefly summed up:—Insoluble formed materials are undoubtedly capable of being absorbed both from the abdominal cavity and from the subcutaneous connective tissue into the circulation; they thus reach the lungs, and, passing through these organs, may enter the systemic circulation. In order to gain entrance into the veins they traverse the lymphatic vascular system; but it is not yet accurately ascertained whether they enter the circulation exclusively by this route. The epidermis forms a serious but not insurmountable obstacle to absorption of starch-grains from the surface of the skin. All these processes of absorption are materially assisted by the presence of fat, which is absorbed still more readily than starch-grains, and enters the circulation by the same path.

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THE PROPORTION OF WATER, FAT, AND NITROGEN IN MUSCLE.

In the last part of the *Zeitschrift für Biologie* (Band vii., Heft ii.) a paper appears by Dr. P. Petersen, giving the results of his very careful analyses of the flesh of various animals, with a view of showing whether any, and, if any, what differences exist in the proportion of their several constituents, and especially of their nitrogen. Up to a recent period the analyses of Voit had been made the basis of all calculations on the metamorphosis of the tissues; and in all researches on the balance of the economy Voit maintained that it might be assumed that the amount of nitrogen contained in muscle was in all instances 3·4 per cent. More recently, however, Seegen, resting on the analyses of Toldt and Nowak performed in Schneider's laboratory, has called this statement in question, and declares the percentage proportion of nitrogen to be constantly higher; and Schenck has also cast some doubts upon the general applicability of Voit's estimate. In view of these differences of opinion, Petersen has lately undertaken a series of analyses of the flesh of the horse, ox, calf, sheep, and pig. The part analysed was in each instance taken from both the fore and hind leg immediately after death, and the analysis at once commenced, to avoid loss by evaporation. The tissue was carefully freed from fat and tendon, cut into small pieces, and carefully dried. By this means the amount of water was ascertained. In twenty analyses of the flesh of the above named animals the proportion of water varied about 7·36 per cent., that of the calf giving the largest percentage, amounting to 79·29, and that of the pig the smallest, amounting to 71·93. The mean of all the experiments gave 76·20 as the percentage of water in muscle, which agrees well with Voit's estimate (75·9—75·67). M. Petersen next proceeded to determine the amount of nitrogen, and ascertained from forty analyses that its percentage in the dried muscular tissue varied from 11·88 to 15·07 per cent., which, calculated for the fresh tissue, gives a percentage proportion varying from 3·03 to 3·64, or, if certain other analyses made on the flesh of the goat and horse be included, to 3·27 per cent. of fresh muscle and 13·79 of dry—a result that, again, is almost identical with the estimate made by Voit. The percentage amounts of nitrogen in the flesh of the different animals were as follows: beef, 3·29; pork, 3·25; mutton, 3·15; veal, 3·18; and horse-flesh, 3·48. M. Petersen's analyses further show that a definite relation exists between the proportion of water and of nitrogen in muscle. The more watery flesh, as a rule, contained more nitrogen in the dry tissue than the less watery flesh, which was apparently due to the fact that the latter contained more fat, or at least more material capable of being extracted by ether. The proportion varied within wide limits, being sometimes as low as 0·76 per cent. in veal, and in other instances as high as 6·55, as in pork, which cannot be freed from the tissue by the knife or scissors. The mean amount of fat for all muscle he estimated at 2·34 per cent. M. Petersen concludes his paper by a reference to the proportion of nitrogen contained in the connective and elastic tissues usually intermingled with muscle; and shows that, although the amount of nitrogen in the ligamentum nuchæ and tendo Achillis is greater than in muscle, this is only due to the density and dryness of these tissues, and that no alteration need be made in the above estimates to allow for the loose and watery connective tissues of muscle. He concludes with the practical remark that, whilst the number 3·4 may be broadly taken as the proportion of nitrogen in dry muscles deprived of fat, it would be advisable, where extreme exactitude is required, to obtain an average from a large number of analyses made of the flesh of the particular animal consumed.

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TACTILE ORGANS IN THE MOLE AND THE MOUSE.

In the last part of Schultze's *Archiv für Mikroskopische Anatomie* (Band vii., Heft iii.) are two papers, one by Th. Eimer, of Würzburg, on the Structure of the Snout of the Mole, the other by Jos. Schöbl on the External Ear of the Mouse, both containing interesting points in microscopical anatomy.

The muzzle of the mole in part supplies the place of its extremely defective organ of vision. The sensory area is situated at its anterior extremity, and presents even to the naked eye a papillated surface. When examined with the microscope, the papillæ are found to be low elevations varying from a tenth to a fifth of a millimetre in height. The papillæ are composed of epithelium, which not only projects from the surface, but dips into the corium; the cells occupying the little fossa in the corium are ribbed cells. Each papilla is traversed from base to apex by an hourglass-shaped canal, the periphery of which is of course bounded by the epithelial cells. The canal is filled by a structureless homogeneous mass, probably of the nature of mucous tissue. The nerves traverse this tissue, and end by free extremities in the outer division of the hourglass-like cavity, which Eimer terms the tactile cone. The disposition of the nerves is very peculiar. If the skin of the snout be treated with chloride of gold, the corium is seen to be richly supplied with a plexus of medullated nerve-fibres. From these, small fasiculi containing twenty or more are given off, which run to the base of the papillæ and enter the inferior cone, at the same time losing their medullary layer. They then appear to arrange themselves in the form of a circle, presenting in transverse sections very much the appearance of the copper wires in the electric cables. As the fibres ascend the tactile cone towards the surface of the skin, they are connected by a slight nodal enlargement or varicosity with each successive tier of cells, terminating as near the surface as the third or fourth epithelial cell-layer. Here and there a nerve-fibre (cylinder axis) runs amongst the epithelial cells of the papilla external to the cone. As the number of papillæ is about five thousand, and as about twenty nerves enter each tactile nerve-cone, the total number of fibres must amount to about one hundred thousand, and their very superficial termination is a matter of great interest.

Schöbl's account of the nerve-distribution in the external ear of the mouse is almost equally interesting. He states that he knows no cutaneous structure throughout the entire mammalian series that is so richly supplied with nerves—not even the wing of the bat. And he goes on to say that he has discovered peculiar tactile bodies in it, and a remarkable pale terminal plexus of sensory nerves. If the cartilage of the ear be horizontally divided so that the ear is split into two lamellæ, no less than four nerve-plexuses may be seen in each. Lying on the cartilage are the larger trunks, which usually divide dichotomously, and intercommunicate in seven different fashions. The second layer, like the first, is composed of medullated fasciculi, of smaller size, freely intercommunicating with each other, and lying immediately beneath the capillaries. The third layer is composed of still finer medullated fasciculi, and is on a level with the capillaries. Small fasciculi of this layer, composed of from two to four fibres, run to hair follicles, and, having encircled each with one or several turns, terminate in a little nervous coil, knot, or glomerulus at its base. The nerve-knots are almost perfectly spherical, with a diameter of about 0·015 of a millimeter; and they occasionally include a few ganglion-cells in their interior. Schöbl estimates the number of nerve-knots at about 12,000 for each ear. Finally, from the last-named nerve-plexus a fourth plexus arises, which is composed of pale fibres, and which lies immediately beneath the Malpighian layer. At their points of junction are well-marked nodal enlargements.

Both of the above papers are accompanied by well-drawn illustrations.

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THE EARLY STAGES OF INFLAMMATION IN BONE.

The early stages of inflammation in bone have recently been made the subject of investigation by Edward Lang, of Vienna, who gives the results of his inquiry in the first part of Stricker's *Medizinische Jahrbucher*. The difficulties in the way of this line of research are considerable, since it is requisite to decalcify the bone in order to make sections; and it is by no means easy to effect the decalcification without injuring the cellular structures the relations of which it is desirable to investigate. M. Lang believes he has overcome these difficulties, however, by employing a weak solution of chromic acid to which a few drops of hydrochloric acid have been added. He finds that the solution should be frequently renewed, and that after the lapse of from seven to twenty-one days, according to its size, the macerated bone can be easily divided, whilst the characters of the tissue are well preserved. The experiments were undertaken on adult rabbits; and although these animals reacted but slowly to irritation of their bones, yet this proved an advantage, since the successive stages of the inflammatory process were separated by a considerable interval of time, and could be easily followed. He first amputated one fore-leg and the opposite hind-leg of a rabbit, and examined the stumps—the latter after two, the former after three days. The two-days-old stump presented no perceptible alterations, except that here and there the corpuscles of the bone were better defined, larger, and more succulent than natural. In the three-days-old stump the corpuscles were very much enlarged and irregular in shape, and they had lost to a considerable extent their regular concentric arrangement; they were more or less strongly granular, and sometimes contained two nuclei; all these changes being most marked near the medulla. The lamellæ and vessels presented no abnormal appearances. In another rabbit he set up much more severe inflammation by making a comminuted fracture and frequently rubbing the fragments together. After six days the animal was killed and the bones macerated. The bone-corpuscles were found to be still further enlarged, greatly altered in form, and replaced by one, two, three, four, or even five nucleated cells. He saw two, three, or four, and in the neighborhood of the periosteum six or seven cells in one capsule; and in many places nests of young cells or microscopic abscesses were present; whilst extensive tracts of the bony tissue were, so to speak, populated or infiltrated with young cells. The lamellæ in many parts were still normal, but occasionally they appeared swollen, with their lines of demarcation indistinct. Thus the direct result of the inflammatory process in bony tissue is the tumultuous development of vast numbers of cells, the multiplication of which augments with the vascularity of the parts; and it is interesting to inquire what may be their origin. Three possibilities may be admitted: they perhaps represent white blood-corpuscles which have escaped from the blood-vessels as vagrant or migratory cells; or they may proceed from the connective-tissue corpuscles, excited to proliferate by the inflammation itself; or, lastly, they may be the result of the multiplication of the bone-corpuscles. M. Lang, after reviewing each of these probabilities, pronounced decidedly in favor of the last. He has instituted another series of experiments, after a method suggested to him by Stricker, in which he has passed needles into the long bones of frogs and rabbits, and, after allowing them to remain for three days, has taken thin shavings from the injured part; these he placed in a trough and supplied with a constant current of oxygenated serum, the table being heated on Stricker's principle. This plan, however, has not hitherto proved very instructive, and the results are not of sufficient importance to require notice at our hands.

Medical Annotations.

"Ne quid nimis."

EXPERIMENTS ON ANIMAL HEAT.

A PAPER on this subject, by Dr. Winternitz, appears in Prof. Stricker's *Medizinische Jahrbucher* (Heft 2, 1871). It appears that Liebermeister, Kernig, Wahl, and others have observed that both in health and disease the abstraction of a moderate amount of heat from the body does not produce depression, but a not inconsiderable elevation, of its temperature. Now, the temperature of the living body is dependent on two factors: the generation of heat on the one hand, and the loss of heat on the other. Changes in its temperature are occasioned by variations of one of these two factors. Both must be considered as subject to the influence of the nervous system. Liebermeister's experiments were conducted on the principle that "if any body retains its temperature unaltered for a certain length of time whilst it is exposed to a colder medium, it must have produced just as much heat during this period as it has given off. If, therefore, the amount of heat abstracted can be ascertained, it will represent the amount produced." Prof. Winternitz's researches have been undertaken with a view of determining whether this principle is applicable to the living body.

The application of cold to the exterior of the body was made in various methods, as by simple exposure to cold air, by immersion in a bath of cold water, and by the application of ice and snow-bags, careful thermometric readings being taken before, during, and after the experiments. He shows that such exposure to cold may at one time produce an elevation, and at another a depression of temperature in the axilla, without its being possible to deduce the absolute amount of heat generated, such variations of temperature in the axilla being, in fact, due to local conditions; and he appears to have been led to the conclusion that the withdrawal of heat from the body is not followed by any marked increase in its generation. Some of his experiments show clearly that, even in normal conditions, very considerable differences occur in the distribution of heat through the body, not only on the surface, but in the interior, at the same distance from the surface, and the distribution of heat in the body is remarkably affected by its partial abstraction. Liebermeister's experiments were not extended over a sufficiently long period; for after exposure to cold it takes a long time for the peripherically cooled fluids to become thoroughly mingled with the warm fluids of the interior—i. e., for the loss of temperature to be uniformly distributed through the body. The chief generators of heat, Dr. Winternitz maintains, are the large actively secreting or functioning organs of the body—the liver, spleen, &c.

INFLUENCE OF WOORARA ON THE TEMPERATURE OF THE BODY

AN essay on this subject appears in the *Central-blatt* (No. 26) by Dr. Franz Reigel, in which he states that, having found in a large number of experiments on the effects of various agents on the temperature of the body and the generation of heat, where the effects of muscular exertion had been set aside by the injection of woorara, there was a constant and nearly equable fall of temperature beginning from the period of narcosis after antecedent convulsive movements, he determined to follow out by careful experiment the action of the woorara alone. The results obtained from his numerous experiments on dogs and cats essentially corroborate those of Röhrig and Zuntz; and show that, without exception, the temperature of the body falls from the very commencement of the muscular paralysis. Tscheschichin's more recent experiments on the same subject, he points out, were almost valueless, as no arrangements were made for the maintenance of artificial respiration, and consequently the animals all rapidly died. M. Riegel's researches, some of which extended over a whole day, readings of the thermometer being taken every five minutes, showed a fall of temperature amounting to two, three, or even more degrees centigrade. A fall of temperature, though to a less extent, occurred even when the animals were wrapped in wool. Another series of experiments were made on animals (principally dogs) in which fever was excited by the injection of pus into the veins; and it was found that even in this state the temperature was lowered by the action of woorara to the normal average, or even below it.

Correspondence.

"Audi alteram partem."

"ON SOME PHYSIOLOGICAL ERRORS CONNECTED WITH CHOLERA."

To the Editor of THE LANCET.

SIR,—I beg permission to offer a brief comment upon what Mr. Sedgwick calls his "physiological theory of the continuance of the mammary secretion during collapse." He says that this is a result of "the antagonism which naturally prevails between nutrition and reproduction, and which would allow of the continuance of functions associated with reproduction even when, as in the collapse of cholera, there is a central arrest of nutrition."

What is meant by this assumed "antagonism between nutrition and reproduction"? If it were a fact that the functions associated with reproduction were unaffected by choleraic disease, the mere statement of the fact would not explain it. I suppose that even Mr. Sedgwick would not attempt to explain it by supernatural agency. There must be a physical cause for this immunity, and until the cause were made known the phenomenon would not be considered to have received a physiological explanation.

This alleged immunity of the reproductive functions, however, is a figment of Mr. Sedgwick's vivid imagination. It is probable enough that the menstrual discharge may continue unchecked during the algide stage of cholera. I know of no physiological reason why it should not, but Mr. Sedgwick admits the liability to abortion in the early stages of pregnancy, and I commend to his consideration the following abstract from the Ninth Report of the Medical Officer of the Privy Council (p. 483). Mr. Simon says:—"From our earliest knowledge of the disease it has been on record that when pregnant women have cholera the intra-uterine offspring almost invariably dies; and more recently, in proportion as the anatomy of the disease has got to be better studied, cases have accumulated, giving detailed evidence in support of an opinion which had from the first been entertained, that the infant in such cases dies of true choleraic infection."

Here, then, is a statement of facts entirely opposed to the gratuitous assumption that the reproductive functions are untouched by cholera; and, moreover, the infection of the fœtus in utero affords conclusive evidence of the existence of a blood poison in cholera, for it is manifest that before the cholera poison can reach the intestines of the fœtus it must infect not only the maternal but the fœtal blood. It appears, then, that the facts here referred to are inconsistent with Mr. Sedgwick's so-callen physiological theory and with his persistent denial of the existence of a blood poison in cholera.

I am, Sir, your obedient servant,
George Johnson.

Savile-row, October 9th, 1871.

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ON THE LOCAL APPLICATION OF SULPHURIC ACID IN THE TREATMENT OF CARIOUS AND NECROSED BONE AS A MEANS OF HASTENING EXFOLIATION.

To the Editor of The Lancet.

Sir,—I have lately used the above treatment in the Chesterfield hospital, as recommended by Mr. Pollock, in two cases with success. One of the patients was a railway guard, whose leg was amputated below the knee. I need not enter into details; but after the operation there was slight sloughing of the stump, and some exposure of the tibia, to about the size of a florin, took place. I applied a lotion of equal parts of sulphuric acid and water, and in a few days a very gentle pull with the forceps brought the piece away, and the man is now recovering rapidly, with a good stump. The other was a waggoner, who was admitted with a very severe compound comminuted fracture of the tibia and fibula, and much contusion of the soft parts. Notwithstanding every care, exposure of the tibia took place in this case also, and I used the sulphuric-acid lotion for three weeks, when the bone came away in a similar manner.

Conservative surgery has been again triumphant, thanks to the carbolic-acid treatment in the first instance, and a good comstitution; while latterly the sulphuric acid has done good service. There is nothing novel in this application of a strong mineral acid to dead or dying bone; but perhaps its useful effects are not sufficiently known or appreciated, and this is my reason for sending this note. I believe the application of the acid in the above cases, and the slight operative interference by the forceps, saved the surgeon and patients many weeks, and perhaps months, of weary waiting for the exfoliation to take place. Mr. Pollock, in a paper of great practical interest and value says (The Lancet, 1870): "the antiseptic qualities of sulphuric acid are no small recommendation to its use. The foul and offensive discharges so constantly accompanying diseased bone become at once altered in character by it, and in a short time all disagreeable smell usually ceases."

I am, Sir, yours faithfully,
John Rose, M.D., R.N., &c.

Chesterfield, Sept. 23rd, 1871.

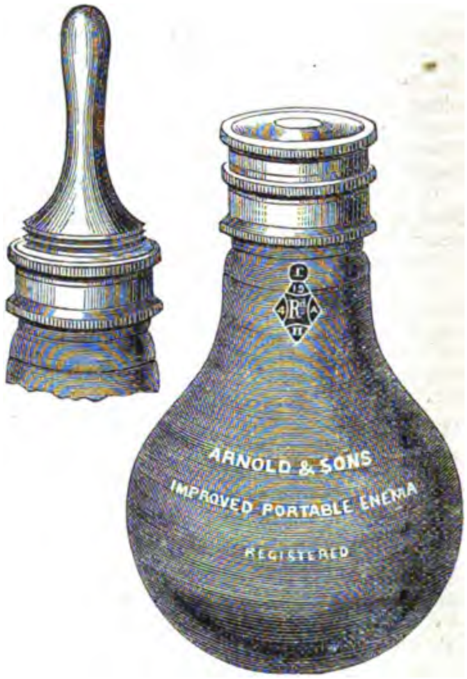
New Inventions

IN AID OF THE

PRACTICE OF MEDICINE AND SURGERY.

IMPROVED PORTABLE ENEMA SYRINGE.

This instrument consists of an india-rubber bottle, which is specially prepared to stand any climate, and is fitted with a reversible pipe and mount, to which is arranged a cap; so that a patient can with safety carry the fluid with him, and, by simply taking off the cap and reversing the rectum-pipe, it is then ready for use. There are certain advantages in such an instrument. There is little liability of its getting out of repair. The mounts are made of brass, and every part which comes in contact with the fluid is carefully thinned on the inner side, so as to prevent the possibility of corrosion. The instruments are fitted in various ways, and answer equally well as an enema or a vaginal syringe. A smaller size is made for injecting the urethra, so that the patient can readily, and without fear of breakage, carry with him the injection and syringe ready for immediate use. The enema syringe is manufactured by Arnold and Sons, 35 and 36, West Smithfield, London.



Obituary.

DR. GEORGE C. BLACKMAN,

Professor of Surgery in the Medical College of Ohio, U.S.

Dr. George C. Blackman died on Wednesday, July 19th, aged fifty-two. He was the second son of Judge Thomas Blackman, and was born at Newtown, Connecticut, in 1819. He graduated in medicine at the College of Physicians and Surgeons, New York, in 1840. For a time he was engaged in practice in New York City, and in Newbury, and then as surgeon to a packet-ship, in which capacity he crossed the Atlantic sixteen times. He also visited London and Paris in order to pursue his professional studies. We are told that upon his visit to London he possessed but seventy-five dollars; and on this sum he managed to live for some months. How patiently and heroically he struggled against the many privations he had to endure was known but to few; but his enthusiastic love of surgery made every other consideration of secondary importance. He was most constant in his attendance at many of the metropolitan hospitals, but especially at King's College and St. George's, where his intelligence, eager desire for information, and courteous manners, gained him a general welcome and secured him many friends. In 1855 Dr. Blackman was elected to the Chair of Surgery in the Medical College of Ohio. Prior to this he had edited the last edition of "Mott's Velpeau," and translated, with annotations, "Vidal on Venereal Diseases." Almost from his entrance into the profession he was a contributor to medical journals.

In 1847 he was elected a fellow of the Royal Medical and Chirurgical Society—"an honor," it is stated in the *American Practitioner*, "which, at that time, and for some years subsequently, we believe, no other American had received." Professor Gross was so highly impressed with Blackman's powers and abilities that, in urging him to settle in the West, he wrote, telling him that "an empire in surgery was awaiting him"; and a friend kindly adds, "he went, took possession of the empire, and proved his imperial power."

Dr. Blackman is said to have excelled in surgical diagnosis; and, as an operator, to have ranked among the greatest of his countrymen; and, from what we know of American surgeons, it may truly be said no higher rank, in this respect, could be coveted by any. He is reported, as a student, to have evinced his love for operating by successfully practising on some cases of talipes with a tendon knife fashioned from a scalpel by the "village blacksmith."

As a teacher, Blackman was accurate, clear, and impressive, essentially practical from his great experience, and full of information from his extensive reading. His private surgical library is reported to have been one of the finest in the United States, and will probably be secured by the profession of Cincinnati for their city library. A not long illness terminated in dropsy, from disease of the liver, and this promising surgeon was cut off prematurely at the age of fifty-two, when his experience and success were commencing to be equally and abundantly useful to his fellows and to the institution to which he was attached.

We regret to learn that one who had so struggled with the caprices of fortune in his early life, and had apparently successfully battled against them, as far as his professional reputation and position were concerned, has died poor, and left a widow and children without adequate provision for their support. It is proposed by his friends in the United States to raise a subscription in their behalf, and we most cordially wish the proposal every success, in respect for the memory of one so generous and genial as a friend, so thorough and honest as a man, so practical and expert as a surgeon.

THE

THIRTY-NINTH ANNUAL MEETING OF THE BRITISH MEDICAL ASSOCIATION.

ABSTRACTS OF PAPERS READ IN THE SEVERAL SECTIONS.

Dr. E. J. Tilt read a paper "On Hysteria and its various Interpretations." He began by remarking that writers on hysteria might be divided into two groups: a comparatively small group of writers, who owned their ignorance of diseases of women, and who nevertheless affirmed that uterine affections had nothing to do with hysteria; the contrary doctrine being held by the great majority of writers, those who were admitted to be thoroughly acquainted with diseases of women. The author quoted from the lectures of Dr. King Chambers, Dr. Russell Reynolds, Dr. Handfield Jones, and Dr. Hyde Salter, to show what was taught respecting hysteria by those little conversant with diseases of women. He then showed how this ignorance on the part of the leaders of the profession, and the teachers in public schools, injuriously affected their own interests, the welfare of their patients, and the advance of medicine. Dr. Tilt objected to identify hysteria with lust, and also to the too common practice of calling hysterical any inexplicable symptom that may occur in women. Lastly, the author explained hysteria as the product of an indispensable predisposition to emotion on the part of the brain, and some determining cause. This determining cause was said to be sometimes one of those debilitating influences that aggravate all nervous affections. Sometimes the cause is the disturbed action of one of the abdominal viscera, the reflex centres of our emotions, diseases of menstruation, in which slight structural diseases of the ovary find expression, and the mildest forms of uterine disease being given as the most frequent determining causes of hysteria. Dr. Tilt's conclusions were —(1) that to be efficient leaders of the profession and teachers in our public schools, hospital men should be thoroughly acquainted with diseases of women; (2) that whenever it is a question of hysteria, the state of menstruation should be carefully ascertained, and the sexual organs accurately examined when they present signs of disease; (3) that the best way for neurologists to disperse the cloud that still obscures our knowledge of hysteria, catalepsy, and epilepsy, is to study the pathology of the ganglionic nervous system.

Dr. Cornelius B. Fox read a paper "On the Estimation of Atmospheric Ozone by means of Aspirators and Acids." The author, having pointed out the great importance of estimating correctly the amount of ozone present in the air, if we would ascertain with certainty whether or not an excess or deficiency of this allotropic modification of oxygen is in any way connected with disease, proceeded to comment on the chaotic and inexplicable condition in which all ozone records are involved. The mode of estimating ozone which has been hitherto generally adopted appears to be liable to the following sources of error:—I. Impurity of chemicals employed. II. Impurity of paper employed. III. Ozonometers faults in construction. IV. Formation of the iodate of potash. V. Bleaching and fading of the colored tests. (1) From formation of the iodate of potash; (2) from presence of *true* antozone in the air; (3) from volatilisation of the iodine set free, in consequence of (*a*) a rapid current of air, (*b*) an excess of moisture in the air, (*c*) a high temperature. VI. Changes in the force of the wind. Brodie and others consider Schönbein's antozone to be a myth, whilst some German savants have recently proved that it is simply the binoxide of hydrogen. Dr. C. Fox believes in the existence of an antithetical state of the air, and describes both the atmospheric conditions under which it occurs and its effect. This principle he names *true* antozone, to distinguish it from Schonbein's antozone, with which it has hitherto been erroneously identified. The various errors above enumerated, of which the formation of the iodate of potash is one of the greatest, are then shown to be easily obviated. This colorless salt, into which much of the iodine set free by the ozone is often converted, he decomposed by the application to the tests of tartaric acid in the form of spray, so that the whole of the metalloid may be estimated. The error arising from the changes in the force of the wind is also avoided by the use of aspirators, by means of which a certain amount of air is made to pass over the tests at a certain velocity. The two forms of aspirators which have been employed for this purpose having been adverted to, a third kind of an improved construction, which has been devised and employed by Dr. C. Fox, was then described. As the author considered that *truthful* answers to such questions as the following—Have an excess or deficiency of atmospheric ozone or true antozone any effect on the public health? If so, what is the nature of this influence? What is the effect of epidemics on their amount?—are of immense interest to the whole medical world, he proposed that a committee be appointed by the British Medical Association to inquire into the merits and feasibility of the new scheme.

"On the different therapeutic indications of Rheumatism and Neuralgia, together with some remarks on Rheumatism as a sequela to Diphtheria." This was the title of a paper by Mr. D. De Berdt Hovell. He said that both rheumatism and neuralgia are conditions of ill-health attendant on low or depressed nerve-power; both are highly susceptible of pain. In rheumatism the first object is to eliminate the lactic and other allied acids from the blood, and to reduce the excess of fibrine; in neuralgia, on the other hand, to supply the deficiencies of the blood, adopting the opinion of Dr. Bence Jones, that the absence of quinoidine is the cause of malarious neuralgia. A similar plan of treatment is called for in the neuralgia of exhausted nerve-power, in that of old age, and from organic disease. Both diseases are liable to aggravation from intestinal irritation, and neuralgia from carious teeth and other forms of diseased bone. In both diseases the susceptible condition of the nervous system calls for relief by some form of narcotic, &c. Acute rheumatism has frequently been observed to follow diphtheria, in which case it is important to ascertain that the urine is free from albumen before adopting the blistering treatment of Dr. Herbert Davies. Assuming that there is excess of fibrine in the blood in diphtheria as well as in rheumatism, cantharides has been found to check elimination by the kidneys, and so to aggravate the symptoms, especially the cardiac complications. In this class of case iodine and the iodide of potassium are specially advocated.

Mr. Christopher Heath read a paper "On the Treatment of Stone in the Female Bladder," illustrated by three cases which had occurred in his own practice. The first case was in a patient aged thirty-two, who was subjected to lithotrity, a stone weighing four drachms, composed of phosphates, with a nucleus of oxalate of lime, being removed in five sittings with complete success. The second case was in a married woman aged forty-nine, in whom a large stone was readily felt per vaginam. Vaginal lithotomy was performed, and a stone of an ounce and a half and measuring two inches by an inch and a half was readily removed, the wound being closed immediately with six silver sutures passed through the entire thickness of both vaginal and vesical wall. The patient made a rapid and complete recovery without the formation of any fistulous opening. The third case was that of a child aged eleven, in whom the urethra was rapidly dilated and a small stone extracted, when there was found to be a much larger mass fixed to the bladder, which was removed with difficulty after being partly broken up, the whole mass weighing nine drachms. The child had incontinence for a few weeks after the operation, but eventually recovered complete control over the bladder. Mr. Heath briefly contrasted the three proceedings, maintaining that rapid dilatation of the urethra within certain limits was a perfectly harmless and most useful practice. He advocated lithotrity for moderate-sized stones in the adult, but for large stones preferred vaginal lithotomy with immediate closure of the wound, a proceeding which experience had proved to be remarkably successful.

Mr. G. Southam, F.R.C.S. (of Manchester), read a paper "On Excision of the Tongue." After making some remarks on cancer of the tongue, Mr. Southam referred to the difficulty which surgeons frequently experienced in excising the entire organ, or even a large portion of it, for this affection. The safest mode of removal is that by the écraseur, but this instrument frequently fails to effect the purpose in consequence of the shape of the tongue, and the peculiar arrangement of the muscles, causing the chain of the écraseur to slip towards the diseased portion, in which it often becomes embedded before the operation is completed. Some cancerous deposit is therefore left, and, though it may not be in sufficient quantity to interfere with the healing of the wound, usually leads to an early return of the affection. To remedy this defect in the operation, Mr. Southam has had constructed a pair of forceps with a movable hinge, which completely grasps the tongue at its root, and confines the action of the écraseur to the part where it is first applied. A case was described in which with this instrument, and without making an opening in the floor of the mouth to pass the chain through, the tongue, of which the body was effected with cancer, was excised beyond the foraman cæcum and circumvallate papillæ, these structures being included in the separated portion.

"Supplemental Mechanical Force during Parturition, regulated by a Dynamometer" was the subject of a paper by Dr. Protheroe Smith. After some allusion to the physiology of labour, and to the agents of force exercised in parturition—viz, that of the voluntary and involuntary muscles—the author spoke of the injurious consequences when the normal balance of these powers was disturbed, specially marking the distinction between the capabilities of the uterus and of the trunkal muscles, to obviate which, as well as to subsidise the power at fault, he advocates the judicious employment of an artificial force, according to certain rules, by means of his "obstetric pelvic band," which was described and exhibited. The peculiarity of this instrument is that it forms, with the pelvis itself, as it were a solid basis, which, by virtue of its immobility, allows the accoucheur easily to employ the required aid to assist and expedite expulsion, and so, by following the natural movements, manifestly to shorten the period of labour and to lessen its risks. This is regulated by a dynamometer, described and illustrated by a drawing. It is so constructed as to measure and record accurately the force employed, imitating the normal throes when wanting, especially by interrupted efforts like those constituting "the compound character" of such pains. In demonstration of this, Dr. Protheroe Smith narrated a case of labour in which such means were employed, with the result. In this, each pain, as well as the amount and duration of every artificial effort is recorded, and some valuable calculations and observations were appended from the pen of Professor Haughton of Dublin, from which it appears that the force used in parturition is much greater than is generally supposed by obstetricians.

Mr. S. M. Bradley, F.R.C.S. (of Manchester), read a paper "On the Unity of the Syphilitic Poison." Mr. Bradley commenced by showing that in order to demonstrate the unity of the syphilitic poison it was necessary to produce a soft sore upon a virgin subject by direct inoculation from a hard, infecting sore; to produce, in other words, a sore indefinitely capable of auto-inoculation, but never followed by constitutional symptoms, from a sore which was (very generally) incapable of auto-inoculation, and which was followed by constitutional taint. He went on to say he had made numerous experiments to ascertain whether this interchange did or did not ever occur, and with the results which are now made public. His subjects were monkeys, kittens, and guinea-pigs; the virus he employed was obtained from cases of syphilis met with in private practice in the Lock Hospital, and in the venereal wards of the Manchester and Chorlton Workhouses. He obtained the matter for inoculation by scraping the surface of the sore prior to cicatrisation, with either a piece of glass or an ivory vaccination point. The great majority of the experiments gave negative results. In two instances, however (one in a guinea-pig, and one in a kitten), the inoculation was followed after the interval of two or three weeks by local thickening at the site of puncture, and later by the outbreak of constitutional symptoms. The guinea-pig died within a month from the commencement of thickening, with disorganisation of one eye, and extensive ulceration about the mouth and soft palate; in the kitten killed at the end of the eighth week were found gummata in kidney and liver. Omitting failures, and the two cases of syphilis mentioned above, he obtained three successful results, the details of which were given. In these experiments, the initial lesion was never irritated by any application; Mr. Bradley merely used the secretion obtainable from the surface of the untreated sore. When the sore was irritated by savine, it was comparatively easy to procure abundant and, as rule, readily inoculable pus. He never succeeded in obtaining positive results with matter taken from a phagedænic sore, or with scraping the surface of one which was entirely void of all secretion. Mr. Bradley proceeded to draw a parallel between the two forms of syphilis and the evolution of the vegetable parasites, or epiphytes; alluding to the fact that all the fungi infecting the human subject are interchangeable, and mutually producible, and yet, as a clinical fact, it was well known that this interchange took place but very rarely,—it seemed probable to him, indeed, that the same causes which operated in this low region of the vegetable kingdom (i.e., differences in the soil and in the age &c. of the seed) were also the efficient causes in determining the character of the syphilitic sore, although we were not yet in a position to decide the precise force which each of these causes possesses.

Dr. Morrell Mackenzie read a paper entitled "Growths in the Larynx: the comparative advantages of Laryngoscopic treatment, and direct Incision into the Larynx." He stated that the relative advantages of these two methods must be considered in relation (1) to the quickness of cure, (2) completeness of removal and probability of recurrence, (3) danger to life, and (4) restoration of voice. From an experience of 100 cases treated, a month was estimated to be the average duration of laryngoscopic treatment. External treatment, on the other hand, required only a fortnight. As regards the second question, complete removal was able to be effected in 97 per cent. of the cases which underwent the full course of laryngoscopic treatment, and recurrence took place in about 7 per cent. In 28 cases of direct incision, collected from all sources, 10 died in a short time, and in the remaining 18 the growth was incompletely removed in 3 cases, and recurrence took place in 3 cases, or, in other words, in 20 per cent. As regards danger to life, no death occurred in the laryngoscopic cases, whereas of the 28 treated by external operation, 3 immediately terminated fatally, 6 died at the end of a few months, and 1 from an independent disease. With reference to restoration of function, perfect voice was regained in 77 per cent. of those who underwent laryngoscopic treatment, and a more or less serviceable voice was restored in 16 per cent. Of the 18 patients who survived direct incision more than a few months, only 9[*] completely recovered their voice, 4 had persistent hoarseness, and 6 permanent aphonia. Consideration of the above statistics establishes the permanent value of laryngoscopic methods of treatment, and justifies one in saying that extra-laryngeal treatment ought never to be adopted unless there be danger to life from suffocation or dysphagia.

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[*] These cases are tabulated in the Thyrotomy table in author's "Essay on Growths in the Larynx." In this table, however, the result of Case 27 is entered as "not stated," but since the publication of the volume Dr. S. Cohen has informed the author that the result was "complete restoration of voice."

Dr. Davey's paper on "Jenner and his Teachings," is designed to prove, from the early writings of Jenner, that we, of this day, have failed to practise vaccination after the manner, or rather in the light, of the first great teacher of the art; that we have ignored the preliminaries to which he attached very much importance, and have fallen therefore out of the path along which Jenner travelled to reach the success he did, but which we, of this day, fail to realise. If we accept the statement made by Dr. Davey, it follows necessarily that Jenner's practice was very much unlike what our own is at this time, and that, in both theory and practice, the practitioner of the day must have fallen away from the teachings of the famous physician of Berkeley. If Jenner doubted (as it is affirmed he did) the virtue of the vaccine lymph after even five gradations, that is, after its passage through but five persons or children successively, and if he then thought it prudent after only five gradations to seek other and fresh lymph from the cow or heifer, it becomes us to ponder awhile over the very dissimilar views so generally entertained, as well as on the practice which these views beget. But, according to the Jennerian aspect of the whole question, we are led to believe that other and deeper causes for the altered or diluted and comparatively valueless quality of the vaccine lymph now too much in use, exist. The teachings of Jender prepare us to accept as indispensable, for the production of a pure, fresh, and trustworthy vaccine lymph, whereby to insure the safety of our patients from the variolous contagion, the aid of the horse. The grease from his heel, but taken before it has lost its early pellucidity, was, we are assured, the mainspring or starting-point of that virus on which Jenner relied to carry all those he vaccinated secure from the possible invasion of small-pox through the period of their natural lives. The same grease it is, according to Jenner himself (or so says Dr. Davey), which, in its passage through the cow, is converted into what is the only reliable antidote to that most terrible of all contagions—small-pox. Assuming the above to be an exposition of Jenner's views and teachings in regard to this all-important subject of vaccination, no time can be so opportune as this present to ask the best attention of medical men to them.

Dr. J. H. Aveling read a paper "On the Value of Arsenic in Menorrhagia and Leucorrhœa." He believes that this remedy has not received from the profession the attention it deserves. Dr. Henry Hunt used it successfully in uterine disorders, and published his experience in 1838. Dr. Aveling has employed it in cases of menorrhagia for twelve years with great advantage. Besides improving nutrition, respiration, and secretion, he finds it to have a powerful decongestive action upon all mucous membranes. He administers small doses of arsenic either in solution or in granules, and continues them for weeks or months, as the necessities of the case may require. He believes the forms of menorrhagia and leucorrhœa most amenable to the arsenic treatment are those which have their origin in a hyperæmic condition of the uterus, which state the remedy cures by acting as a tonic and stimulant upon the vaso-motor nerves, causing the capillaries to contract and expel the superabundant blood.

Dr. Alfred Meadows read a paper "On the Treatment of Fibroid Tumours of the Uterus," in which he combated the notion that these growths can be in any way diminished in size, still less cured, by any known therapeutical agent. Discussing the question from a histological point of view, he felt convinced that it is impossible to procure the absorption of any part of the solid constituents of these tumours; at the same time he showed that there is ample scope for the exercise of the greatest skill in the medical treatment of such cases, especially in regard to the two most constant and important symptoms—viz., hæmorrhage and pain. The chief object of the paper was, however, to advocate more frequent resort to surgical treatment. The author expresses his belief that much more might be done in many of these cases than has been hitherto. Even in the subperitoneal variety he thought that, in cases where much distress exists, abdominal section ought to be resorted to more frequently; while in the interstitial and submucous forms, it ought to be the rule in practice always to endeavor to assist Nature in her method of cure—viz., by expulsion. For this purpose three objects should be kept steadily in view. 1st. That all obstruction should be removed by freely dividing the cervix in several directions. 2nd. That the tumour should be separated from its attachments, not necessarily all at once, but by successive stages. 3rd. That as far as possible continuous uterine action should be maintained by the exhibition of ergot and other oxytoxic agents. A case was cited in illustration of the value of this combined method of treatment.

Dr. Clifford Allbutt, in a paper on "The Lesions of Enteric Fever as an Occasional Cause of Permanent Injury to Nutrition," drew attention to the convalescence from enteric fever, which is well known to be often so tedious; and he raised the question whether the specific lesions of that disease, affecting as they do the instruments of absorption, might not sometimes be the cause of permanent marasmus. In enteric fever the local mischief falls not only upon the patches of Peyer in the ileum, but spreads itself throughout the network of the mesentery. If a rat be fed upon tallow candles and then killed, the presence of the fat in great quantities in the mesenteric network and glands shows how active is that system in taking up this element of nutrition. Any disease, therefore, which interferes with this system, like enteric fever within it, or chronic peritonitis outside it, would have its visible effect in hindering the absorption of fat and in preventing the laying on of adipose tissue. These considerations occurred to the author in consequence of his advice being sought in several cases of marasmus, pure and simple, without local disease, without fever, and without adequate loss of appetite. In all of these a severe attack of enteric fever had preceded the marasmus. The patients, who were almost denuded of all adipose tissue, had, previous to the attack of enteria, been in good health. The only explanation which he could give of these cases was, that the fever had acted upon the fat-collecting system in the way already pointed out. The notes of six cases of this kind were then read, but in one of these the marasmus had been preceded, not by enteria, but by a protracted affection of the bowel, improperly named dysentery.

Dr. Wynn Williams read a paper "On the Treatment of Cancer of the Neck of the Uterus and Allied Structure, by the Injection and Application of Bromine." The author commenced by making some remarks on the spontaneous removal of malignant tumours, from the study of which he was led on to the injection of bromine into cancerous tumours of the uterus and other parts. He stated that the eight cases published in the last volume of the Obstetrical Transactions still continued well. He entered into full details as to the manner of injecting these deposits and the care required in the use of bromine both as an injection and application, and that before its use the surrounding parts should be well protected by a solution of soda. He exhibited the various instruments he had had made for the injection of bromine. He gave the history and successful treatment of a well-selected case of medullary carcinoma of the uterus in the state of disintegration and ulceration by this method. He also gave the particulars of a case of epithelioma of the lower lip which had been previously removed by operation. On the return of the disease the patient was sent to Dr. Wynn Williams, who, by two injections of bromine, caused the entire and, so far, permanent removal of the disease. A vignette of the patient was exhibited.

Dr. Braxton Hicks read a paper "On a Rare Form of Hæmorrhage." After quoting the remarks of Dr. Blundell on a form of concealed hæmorrhage, caused by the falling down of the membrane, and consequently the retention of blood within the uterus, he brought forward three cases in which, the membranes remaining attached all round the lower portion of the uterus, and blood being effused between the upper part of the uterus and the membranes and margin of placenta, the membranes and a portion of the placenta were inverted so as ultimately to be driven through the os into the vagina some distance, imitating the bag of membranes in a twin-case after the birth of the first child. The uterus in the meantime became distended with blood, and serious symptoms arose without any sign externally. The treatment was pointed out, and some short remarks made on the mode of expulsion of the placenta.

Dr. Braxton Hicks also described four cases of Acute and two of Chronic Inversion of the Uterus, following each case by remarks on the causation, prevention, and treatment. He showed some apparatus he had devised for the restoration of chronic inversion, consisting first of india-rubber pessaries, and then of an apparatus to keep up constant pressure on the fundus, a simple kind of which he had found successful.

Dr. Barnes exhibited an enlarged drawing, taken from a specimen recently put up at St. Thomas's Hospital, which showed, with an accuracy probably unique, the anatomical characters and relations of Hypertrophic Elongation of the Cervix Uteri. The entire length of the uterus was seven inches. The two lips of the os uteri were much hypertrophied, somewhat everted, and formed a large mass external to the vulva. The whole vagina was everted to clothe the protruded mass. The retro-uterine pouch of the perineum was extended so as to be outside the vulva. The value of the preparation was great, as giving a typical illustration of a not uncommon affection, the relation of all the parts being accurately preserved.

Dr. Julius Althaus read a paper "On Paralysis of the Bladder, and its Treatment by the Constant Galvanic Current." In this paper the author, after referring to some important researches on the physiology of micturition lately made by Professor Budge, of Greifswald, enters fully into the pathology of paralysis of the bladder. He eliminates from this affection all cases of mere atony of the viscus from over-distension owing to organic obstructions, such as stricture of the urethra and hypertrophied prostate, and other causes; and likewise all cases of incontinence of the urine which is generally ascribed to paralysis of the sphincter vesicæ. Real paralysis of the bladder is, according to Dr. Althaus, only observed (*a*) when the conduction of nervous influence from the pedunculus cerebri to the bladder is interrupted; (*b*) when the the lower part of the lumbar portion of the spinal cord is diseased; and (*c*) when the normal excitability of the motor or sentient nerves of the bladder is pathologically altered without any central affection being present; most cases of this latter class being of the kind termed reflex or inhibitory paralysis. After reviewing the treatment generally adopted for this condition, the author expresses his opinion that, both in efficacy and quickness of action, the constant galvanic current properly applied is superior to all other remedies which are used for this affection. He then describes the best mode of applying the current in such cases, and winds up by relating three cases illustrative of the different varieties of the complaint. One of these was owing to syphilitic disease of the pedunculus cerebri; another occurred in a hysterical patient; and the third arose from disease of the lumbar cord. In all these cases the paralysed bladder recovered rapidly under the influence of the constant galvanic current.

Mr. Furneaux Jordan read a paper "On the Extension of Inflammation from the Epididymis to the Urethra." The author remarked that inflammation of the prostatic urethra from any cause (injuries, operations, foreign bodies, urinary obstructions, or adjacent inflammations), may extend to the epididymis. It would be an original discovery to find any variety of prostatic inflammation which might not run by continuity along the submucous connective tissue of the vas deferens. He was not aware that anyone had observed the converse of this. He had recently watched a case in which inflammation unmistakably travelled from the epididymis to the urethra. A married man, free from disease and the history of disease, suffered from the effects of a severe blow on the scrotum. On the subsidence of scrotal swelling, the left epididymis was found to be enlarged, painful, and tender. The next day the adjacent portion of the vas deferens was tender, and swollen to the size of a goose's quill to near the inguinal ring. The following day the swelling of the cord extended into the ring. A few days later a slight urethral discharge appeared, and all the symptoms of a mild urethritis. Mr. Jordan believed any new facts to be of value which would help to explain urethral discharges.

Dr. Victor Jagielski read a paper "On Cow's Milk Koumiss." He said that koumiss is a pure animal milk in a state of fermentation. In its composition it combines all the requirements for a wholesome nutrition of the human body; while certain products of the fermentation add to it important therapeutic properties. All animal milks are convertable into koumiss, and the general qualitative composition of the products is the same for all. In all, the act of fermentation sets free the casein, albumen, and butter in a highly attenuated form, and develops alcohol and carbonic and lactic acids, together, according to Morfit, with certain fragrant volatile compounds. Once started, it continues until all the lactose of the milk has been transformed, and this transmutation is more rapid in proportion to the rise in the temperature of the air. Dr Jagielski distinguishes the three gradations thus formed, according to their respective physiological effects; but though each may have a specific application, the general properties are retained by all. No. 1. This is the freshly made koumiss, with the minimum of the products of fermentation. In three to five days it becomes—No. 2, which is more acidulous than sweet, and so sparkling that it requires to be drawn from the bottle through a tap. In ten to fifteen days it has changed into—No. 3, when the fermentation is more developed than in No. 2, and extends to the maximum with time. Consequently the taste is acid, and the koumiss rushes through the tap as a rich, creamy, foaming liquor. After quoting many distinguished European and American authorities in its favor, the author noted some of the favorable results of his own professional experience with the use of koumiss in cases of constipation, impaired digestion, debility, chronic bronchitis, consumption, diarrhœa, in the adynamic stage of febrile diseases, after confinements, operations, in diabetes, &c. In conclusion he urged a thorough trial of it in both hospital and private practice, believing that, though it is not a specific for any disease, it merits the most serious consideration, as a most benign medicinal agent.

Dr. Protheroe Smith read a paper on "A Successful method of Treating certain Cases of Dysmenorrhœa and Sterility." After giving the pathology of obstructive dysmenorrhœa, with the usual mode of treatment, Dr. Protheroe Smith called attention to that of dilatation by bougies, suggested by Dr. Mackintosh, and the modification of it by Sir James Simpson, and his operation of incision of the cervix by the hysterotome. Dr. Protheroe Smith's experience having suggested a doubt of the advantage of this practice, he was led to adopt the plan he now advocates in certain cases indicated by conditions which he mentioned. To overcome the stricture of the os internum he uses extension force by means of his uterine dilator, described and exhibited, which is peculiarly suitable to this purpose. And, to restore the os tincæ to its natural form, per speculum, be incises it laterally at the commissures of the labia. Particular instructions were given for this treatment, which during the the last twenty-five years he has employed with considerable success in remedying both obstructive dysmenorrhœa and sterility. Six cases were cited in illustration.

Dr. Wm. Roberts (of Manchester) read a paper "On Intemperance as a cause of Chronic Bright's Disease." The author said that the generally received opinion that the abuse of alcoholic liquors is a frequent cause of Bright's disease has been called in question by Dr. Dickinson, in an elaborate argument in his recent work on Albuminuria; and the scope of the paper was to examine the evidence on which Dr. Dickinson had relied in coming to an opposite conclusion. The writer endeavored to show that the pathological facts adduced by Dr. Dickinson were either untrustworthy or that they had been incorrectly interpreted. Dr. Roberts especially pointed out the fallacy of the argument drawn from the reports of the Registrar-General. It was quite true, as Dr. Dickinson had stated, that the districts which yielded the largest returns of deaths from intemperance did not return an excessive proportion of deaths's from Bright's disease; but exactly the same result was obtained when the same statistics were applied to the mortality from cirrhosis of the liver—a disease which is notoriously and chiefly the product of intemperance.

Dr. Swayne read a paper "On the Treatment of Hæmorrhage arising from the Retention of the Secundines after Abortion." Treating chiefly of abortions in the third, fourth, and fifth months, he pointed out their dangers from special liability to retention of the secundines, and consequent hæmorrhage and septicæmia. Obstetric authorities were divided as to treatment, some favoring an expectant plan, with the use of plugging, ergot, styptics, and disinfectants to obviate hæmorrhage and septicæmia, and others advocating manual interference. He remarked that the plug might sometimes cause an accumulation of blood in the uterine cavity, and showed a pad for preventing this, by making pressure on the fundus uteri. He pointed out the risks of intra-uterine injections, when a large amount of fluid is thrown up with too much force. After remarking that the weight of obstetric evidence in the present day is in favor of manual interference, he stated his concurrence in this view; but that he preferred, instead of using the hand for removing the placenta, to employ an ovum forceps, so modified as to act both as a dilator and an extractor.

Dr. Thos. Underhill read a paper "On the Treatment of certain Cases of Placenta Prævia, and of Post-partum Hæmorrhage." The author was opposed to the maxim so strictly enforced by most authorities, that in cases of "unavoidable" hæmorrhage delivery should not be attempted whilst the patient is in a state of syncope. During that condition the hæmorrhage ceases, the patient is in a state of anæsthesia, and the soft parts are relaxed—these being three desiderata for the safe and speedy performance of podalic version. By waiting until the circulation is re-established, and consciousness restored, there will be most probably a recurrence of the hæmorrhage, and the patient will have the dread of a formidable operation. Cases were given in support of the practice suggested. In the second part of his paper, Dr. Underhill advocated the view that in cases of post-partum hæmorrhage, should syncope supervene, it was more prudent to remain passive for a time than to resort to hasty measures to restore consciousness. He based his argument upon the fact that, during syncope, the circulation being either languid or altogether suspended, coagula would be more likely to form and occlude the patulous orifices of the vessels than when subjected to the artificial impetus.

Mr. Lund (of Manchester) read a paper "On Antisepticity in Surgery." It was argued by the author that antisepticity might be brought about by any means through which the excreta of wounds were protected from putrefactive change, and the surfaces on which they rest preserved from contact with the irritating chemical products thus elaborated. In recent wounds the strict observance of antisepticity saves much constitutional distress, tends to keep the tissues from loss of substance by sloughing and ulceration, and the patient's strength by needless suppuration. But in healing-wounds it was admitted that greater time will be often required to complete a perfect cicatrix, although this, when formed, will be more pliable and natural than under other modes of treatment. The claims of many antiseptics were considered, but preference was given to carbolic acid in very weak solution, or in that form of composition which Professor Lister has described as his carbolised muslin, a modification of the antiseptic cere-cloth first proposed by Mr. Lund at the meeting of the Association at Leeds in 1869.

Mr. Thomas Littleton read a paper "On the Effects of Submarine Descent on Man, and the Limits to his Capability." The paper consisted of extracts from the communication alluded to in the *Association Medical Journal* (Feb. 9th, 1855, p. 127), with a confirmation of the pathological views therein maintained. And, in addition, some allusions to the history of diving operations in the port of Plymouth. Also an induction from the experiments at the Royal Albert Bridge, &c., and a comparative consideration of the anatomy of the whale, as to the probability of man's capability, from a physical point of view, to compete with the whale in diving, as he has successfully with the eagle in balloon ascent.

Dr. Merryfield discussed "The Meteorology and Climate of Plymouth," under the following heads:—1. Distinction between weather and climate. 2. Mean temperature, how found. 3. Hottest and coldest months of the year, time of day of maximum heat, &c. 4. Humidity, as obtained from the hygrometer; uses of wet and dry bulb readings in producing an artificial climate for invalids. 5. Effects of Gulf Stream on S.W. coast. 6. Ranges of temperature. 7. Atmospheric pressure. 8. Prevailing winds. 9. Rainfall; extremes and draughts. 10. Ozone; storms, electrical and atmospheric. 11. Conclusions.

Mr. W. J. Square read a paper "On Loose Cartilages in the Knee-joint, and their removal by Subcutaneous Incision." The first part of the paper contains remarks on the dangers of valvular and direct incision for the removal of loose cartilages from the knee-joint, and a reference to a few cases illustrative of these dangers; next a record of twenty-four consecutive and unselected cases of removal of loose cartilages by subcutaneous incision, without anxiety, let or hindrance; an appeal to the profession to adopt the operation; and lastly, a few remarks on the varieties of loose cartilage, their structure, and origin, illustrated by a series of preparations.

Mr. W. J. Square also read a paper "On the Influence of Nerves in the Repair of Fractures." The paper is founded on a case of simultaneous fracture of the spine, ulna, and femur. The patient survived three weeks. As the fractures were placed under precisely similar circumstances, the difference between the two (femur and ulna) was presumed to be due to the abstraction or otherwise of the influence of nervous centres.

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ITALY

(FROM A CORRESPONDENT.)

SOME exceedingly interesting experiments in physiology have recently been carried on by Prof. Schiff in the Physiological Laboratory of the Institute of Florence, with the object of arriving at some correct conclusion touching the pretended motor excitability of the anterior part of the brain in mammifera. These experiments had been suggested by a work published last year in Berlin, and stating that excitement of the anterior lobe of the brain produced movements in the muscles of the opposite side of the body. The authors concluded that the anterior portion of the brain was endowed with motor faculties. The results of Prof. Schiff's experiments confirm the fact of muscular movements being produced through irritation of the frontal lobes, but they show at the same time that these movements do not proceed from a motor property pecular to that part of the brain, but are reflex movements excited by irritation of certain parts of the brain which preside over the tactile (non-painful) sensibility of the body. The excision of these parts does not directly prevent movements, but modifies them indirectly, the abolition of tactile sensibility. One of the chief results of these researches is that certain degenerations of the brain, especially of the frontal lobes, may be liable to produce a kind of locomotor ataxy.

Foreign Gleanings.

THE FORMATION OF PUS.

PUS was formerly considered as an altered liquor sanguinis, in which the fibrin has assumed a molecular and corpuscular form. The corpuscles were looked upon as having much resemblance in size and general appearance to the white corpuscles of the blood, though the circumstances which determine the change of fibrine into corpuscles of pus were not established. It has more recently been supposed that the pus-globules were the result of the proliferation of the corpuscles of the connective tissue. But Cohnheim, in 1867, maintained, after experiments, that pus globules are simply leucocytes, or white corpuscles of the blood, which traverse the parietes of vessels by means of their amœboid movements. Kremianski and Hayem confirmed these views, repeating the experiments, especially on the tongue and mesentery. Dr. Charlton Bastian in this country also adopted this opinion, and the name of "migration of leucocytes" was given to this phenomenon. The well-known M. Vulpian also supported these views, finding, like M. Hayem, the globules lying along the vessels, and filling up the intervascular spaces.

But in 1870 a controversy arose on the subject, and M. Feltz, of Strasburg, after carefully experimenting on the mesentery and tongue of the frog and the mesentery of the mouse, showed that new leucocytes formed around the vessels, that these leucocytes did not arise from the corpuscles of the connective tissue, and had in no way traversed the parietes of the vessels. M. Feltz resorted, like Cohnheim and Kremianski, to the coloring of the leucocytes with aniline and cinnabar, and maintained his opinion after a second series of experiments, presented to the Academy of Sciences of Paris on June 6th, 1870.

This physiologist was followed by M. Picot, of Tours, who, in July, 1870, also sent a paper on the subject to the Academy of Medicine of Paris, and proved that

Cohnheim was wrong—so much so that, according to M, Picot (from whose letter in the *Gaz. Hebd.*, Sept. 22nd, 1871, we have extracted these details), the globules of pus newly formed and found far from the vessels.

M. Picot, in a few sensible remarks, shows how difficult it is to make out the migration above alluded to, and enters into minute details as to the manipulation of these experiments. He contends also that, independently of experiments, the fact of finding leucocytes surrounding vessels is no presumption that they have traversed their coats. Thus stands the matter at present. It is to be hoped that the praiseworthy endeavors of the young generation of pathologists will ere long solve the problem.

A CASE OF CONGENITAL OBLITERATION OF THE ŒSOPHAGUS.

Dr. Porro published in the *Annali di Med.* of Milan, September, 1871, a case of this kind. The child was brought to the Foundling Hospital of Milan in August; 1870. She was noticed to take the breast with avidity, but after a little suction she suddenly left the breast, coughed, became livid, rejected most of the milk through the nose, and was nigh suffocating. The utmost care was employed to discover the cause of this, and some suspicion of the actual state of things was raised by the following examination:—An elastic tube was introduced into the pharynx, and, being gently pushed downwards, was arrested as in a cul de sac. The child died on the third day, and it was found, on a post-mortem examination, that the larynx and pharynx were normal.' The cervical portion of the œsophagus presented parietes rather thicker than are generally seen in infants, and was of a larger size than usual. It ended in a cæcal pouch, about an inch below the inferior portion of the glottidean aperture. The posterior part of the trachea was lined by streaks similar to the muscular portion of the œsophagus. These fibres could be traced from the œsophageal cul de sac to the point where the trachea divides into the two bronchi. The cul de sac being longitudinally divided, no communication could be discovered between it and the laryngo-tracheal tube. Dr. Porro now passed a stylet through the pyloric orifice of the stomach, and, by gradually working it upwards, made it emerge at the glottis. The anterior and lower portion of the trachea was now longitudinally divided, and it was discovered that a circular opening, about half a line in diameter, existed where the trachea divides into the two bronchi, and exactly in the space which inferiorly divides the two canals. The distance between the superior cæcal pouch of the œsophagus to the point of communication of the inferior part of this canal with the trachea measured about one inch. The author remarks, after this description, which is rendered very plain by two excellent plates, that Mr. Holmes, in his work on Surgical Diseases of Children, mentions three cases of congenital obstruction of the œsophagus; but in none was the interruption of the continuity of the œsophagus so considerable as in this case.

ASPIRATION OF GAS AND FLUID IN STRANGULATED HERNIA.

M. Duplouy, Professor of Clinical Surgery at Rochefort (*Gaz. Hebd.*, February 6th, 1781), having been consulted in a case of strangulated hernia of three days' standing, in a man of eighty-two, had recourse to the above-mentioned operation. Herniotomy was not thought advisable on account of the patient's age, and the needle and aspirating syringe were used. Only gas was at first evacuted, and by continuing the play of the piston, fluid tinged with fæcal matter was obtained. The bowel thereupon became flaccid, and was easily returned. The case did well, and it is supposed that, the puncture being very fine, the fibres closed the minute aperture by lying against each other, fatal effusion being thus prevented.

Evacuation of gas by puncturing has been in vogue of late with French surgeons in cases of tympanites, the result being in some instances favorable. Dr. Rahn (*Montpellier Med.*, June, 1871) succeeded in a case of strangulated hernia by injecting in the vicinity of the tumour the twenty-fifth part of a grain of sulphate of atropine.

We may add that, in 1865, M. Tartarin published several cases in the *Bulletin de Thérap.*, in which he succeeded in reducing strangulated hernia without kelotomy, by the use of the following liniment:—Glycerine and croton oil, of each 45 minims. With this compound three frictions were made on the tumour, the first at 2 o'clock, the second at 3, and the third at 4. Before the last two frictions the skin was washed with soap and tepid water, and the spot carefully dried with a piece of fine linen. At 5, a copious stool was obtained, and the contents of the hernia were easily reduced. The case did well.

CARBOLIC ACID IN VETERINARY PRACTICE.

La Tribune Médicale (Sept. 24th, 1871,) publishes a letter from a medical man of Savoy, M. Lochon, who states that, in a distressing epidemic of cattle-plague, which had been very destructive, the following treatment arrested the spread of the disease:—1. Bleeding to the extent of three quarts. 2. Adminster a quart of lukewarm water in which are dissolved two drachms and a half of crystallised carbolic acid. 3. Give, once or twice, four quarts of strong infusion of coffee. 4. Energetic frictions to excite cutaneous reaction. 5. Scarify the infiltrated regions. 6. In the course of the complaint give several quarts of infusion of gentian. 7. Dress with the solution of carbolic acid mentioned above the pustules which form on the hide. In this manner were treated fifty cows, seven horses, and one pig. Of all these only one cow died, whilst previous to these measures the mortality was considerable.

News Items, Medical Facts, &c.

Parasitic Diseases of the Scalp.—To destroy the parasites, Küchenmeister advises the use of alcohol, which retards the development of the sporules. It has, however, been shown that the alcohol does not reach the follicles. Tincture of iodine is more efficacious, but its use must be kept up at least three months, even in the most favorable cases. Prof. Von Erlach, of Berne, applies oil of turpentine with a camel's-hair brush, and maintains that this application is more certain of success than all others, and requires less time than the generality of applications. He states that he succeeded in curing porrigo decalvans in fifty days, and several cases of sycosis in a week.

The authorities at the island of Teneriffe have recently instituted a most rigorous quarantine on account of cholera, and it is rumored that no vessels of any nationality are permitted to communicate with the port.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 2. NEW YORK, FEBRUARY, 1872.

PRACTICAL PHYSIOLOGY.

Lectures

ON

EXPERIMENTAL PHYSIOLOGY.

By **Wm. Rutherford**, M.D., F.R.S.E.,

Professor of Physiology in King's College.

LECTURE VIII.

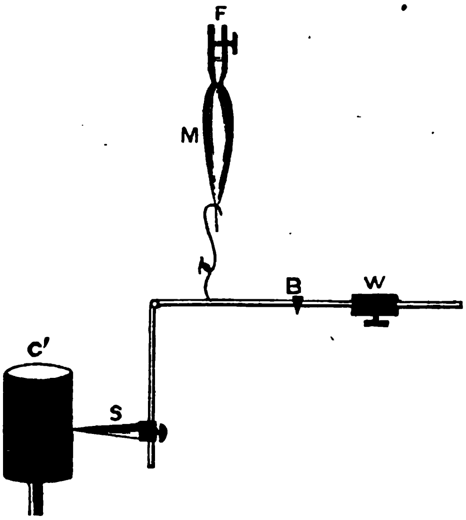
MUSCLE.

Gentlemen,—Having studied the experimental physiology of the nervous system, we shall now occupy ourselves with the practical physiology of muscle.

The forms of energy produced in muscle are:—1. Molar or mechanical motion. 2. Molecular motions—to wit, thermal, sonorous, and electrical motions. These molecular motions will be studied by us upon a future occasion. To-day we shall confine ourselves to the ordinary or mechanical motion of the muscle.

The irritants which cause a muscle to contract have already been studied by us.* Let us now study the contraction itself. To enable us to do this we must register the movement of the muscle by means of a recording apparatus. When we measured the rapidity with which nerve-force passes along a nerve, we employed a myograph invented by Helmholtz. We blacken the cylinder of the machine by means of turpentine smoke, attach a muscle to the lever (see Fig. 26), and stick the ends of two thin wires connected with a Du-Bois Reymond's induction coil (see Fig. 9, June No. 1871) through the muscle, one close to the femur, the other close to the tendo Achillis. We set the machine in motion. As you already understand from my description of the instrument (p. 282, June No. 1871), the machine causes a single induction shock to pass through the muscle the moment that the cylinder, driven by clockwork, attains a speed that has been regulated beforehand. The writing-stillette (s) scratches a horizontal line upon the blackened surface of the cylinder while the muscle is at rest; but when the muscle contracts it raises the lever and the stilette, so that a curve is produced upon the cylinder. By taking off the cylinder and running it along a sheet of moist gelatine, we readily print such a tracing upon a flat surface. (Fig. 27.) When we examine this myogram, we find that there are two lines—a horizontal line (a, x) and a curve (a, z). The horizontal line—also called the abscisse—is produced by the writing-point when the muscle is at rest. In this myograph, the cylinder revolves several times, with the stilette or writing-point in contact with it, before contraction of the muscle is produced. In

Fig. 26.



A portion of Helmholtz and Du-Bois Reymond's myograph. C′, Revolving cylinder. M, Muscle (gastrocnemius of frog). F, Forceps holding the femur. B, A lever. W, A movable weight for the purpose of balancing the lever upon the fulcrum (just under B). S, A steel needle for the purpose of scratching the soot off the blackened cylinder, and thereby producing a tracing.

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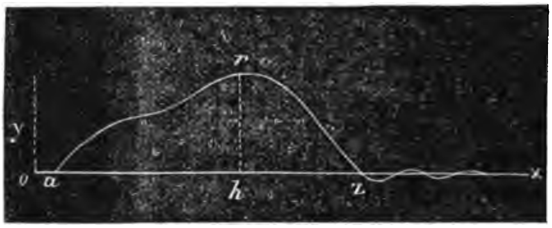
* Lecture II.

other kinds of recording apparatus the normal line requires to be drawn under such a curve. This abscisse is, as it were, the zero line; it indicates no action. The length of the curve indicates the time occupied by the contraction of the muscle; and this time may be readily ascertained by measuring the length of the horizontal line under the curve, and comparing it with the speed with which the cylinder moves when the curve is produced. I shall presently explain to you how, in another kind of recording apparatus, the speed of the cylinder's motion at the time when the curve is produced is readily ascertained by recording upon it the vibrations of a pitch-fork. The *length* of the abscisse, or horizontal line, serves then as an index of the *duration* of a movement such as that of a muscle recorded in this way. The *height* of the curve above the horizontal line indicates the *intensity*, degree, or extent of the movement. The higher the curve, the more has the muscle contracted. We measure the height of the curve by drawing at any number of points vertical lines or ordinates (such as that at *h r*). We shall have to examine many tracings produced with the aid of instruments for recording movements. Remember these two points—that the duration of the movement is indicated by the length of the *abscisse*, while the intensity or degree of the movement is indicated by the length of the *ordinate*. Duration and intensity are the two essential points to be attended to in studying the motions which are indicated by such curves. But there is a third point—to wit, the general character of the movements as indicated by the form of the tracing. The form is the resultant of the duration and intensity. Duration, intensity, form, are, then, the three things to be borne in mind in the study of such tracings. In the above myograph, we know the point on the horizontal line at which the electrical shock is transmitted through the muscle; that point is indicated by the dotted line (*o*, *y*). The muscle begins to contract at the point *a*. The contraction increases till it reaches a maximum; it then gradually diminishes, and finally disappears, as indicated by the right side of the curve. Notice that a distinct interval elapses between the application of the irritant to the muscle and the contraction. This interval has been named by Helmholtz the period of latent contraction. A scale-pan with weights may be attached to the lever just under the hook which holds the muscle, and in this way the muscle may be caused to lift various weights, and the extent to which it raises the weights can be accurately observed. When we merely want to ascertain the extent to which the muscle can raise a weight, it is not necessary that the writing-point should write upon a moving surface. A vertical line, produced by causing the muscle to contract with the stilette in contact with a motionless surface, is sufficient. Pflüger constructed a myograph in order to give such vertical tracings only. The surface on which the tracing is produced is a motionless piece of flat smoked glass. Such a tracing, however, indicates only the extent or intensity of muscular contraction. The duration and the general character are not shown as they are when the tracing is produced upon a moving surface. Many forms of myograph have been invented; but the most convenient and the most satisfactory one in every way for studying the nature of muscular contraction is Marey's. Fick's myograph—consisting of a flat glass plate fixed at the free end of a pendulum—and Helmholtz's myograph[*] are still used, however, for estimating the rapidity of nervous transmission. Much service has been done to physiology by Marey having insisted on the necessity for having the apparatus to be used for recording movement as light and of as little bulk as possible. This great brass lever of Helmholtz's myograph is a heavy thing. No doubt it can be balanced by means of the weight (w, Fig. 26), so that scarcely any weight has to be borne by the muscle. At first you might be inclined to suppose that this balancing of the lever and consequent lightening of the weight borne by the muscle ought to serve the same purpose as if the lever were made of a light thing like a straw. It is not so. The mass of the one is far greater than the mass of the other. The thick brass beam, however delicately balanced, requires far more force to set it in motion than is required to set in motion a straw similarly balanced. And just as it is more difficult to move the one than the other, so it is more difficult to stop the motion of the one than that of the other. The one most difficult to move is also the one most difficult to stop. In other words, the momentum of the heavy body is greater than the momentum of the light one. In consequence of this, very fine movements are lost when, in order that they may be registered, they require to throw heavy bodies, such as a heavy lever or a column of mercury, into motion; and, moreover, the great oscillations of the heavy body which follow from its being thrown into motion introduce sources of fallacy in the tracings of movements effected by their aid. To register movement, Marey has therefore introduced the use of a light instead of a heavy lever, and, where it is possible, a column of air instead of a column of mercury. We are not to-day going to transmit any movement through a column of air, or a column of mercury; but it is convenient that I just allude to these things in this connexion.

FIG. 27.



a, z, A myogram or tracing produced by the gastrocnemius of the frog, with the aid of Helmholtz's myograph.

The recording apparatus employed by Marey is one which may be conveniently adopted for registering the movements of the muscular, circulatory, and respiratory organs. It consists of a brass cylinder, turned by clockwork, moved by a powerful spring. The cylinder may be placed horizontally (Fig. 28) or vertically (Fig. 29). It is covered with paper when a tracing is to be taken. The paper should be glazed, in order to diminish the friction of the writing point. Tracings may be made by writing upon the paper with ink, or by scratching the soot from paper that has been smoked by causing the cylinder covered by paper to revolve rapidly in the smoke of a paraffin lamp, or, indeed, in the flame of ordinary gas. When tracings are taken upon a sheet of paper longer than the circumference of the cylinder, smoked glazed paper, although it is extremely convenient, and gives the finest tracings, cannot be employed. There are three axles upon which the cylinder may be fixed. These give different rates of speed. The speed at which the paper moves depends, of course not only on the rate of the axle's motion, but upon the extent of the cylinder's circumference. The axle's motion remaining the same, the paper would move more rapidly with a large than with a small cylinder. When a very great speed is wanted, a glass disc or cylinder of cardboard may be fixed upon the top of the axle which bears the regulator, R (Fig. 28). The great speed obtained in this way is only useful for measuring the rapidity of nerve-force. Marey has estimated the rapidity with the aid of this instrument (see Lecture IV., The Lancet, June No. 1871). The motion of this registering apparatus is extremely regular when it is placed upon a level surface. Its motion, too, is more regular when the regulator is placed upright (Fig. 28) than when it is laid upon its side (Fig. 29). In order to provide against errors which would inevitably arise from unequal motion of the cylinder, we require, for all work that must be perfectly exact, to register upon the paper a tracing produced by some instrument which marks time with precision—e. g., the oscillations of a pendulum, a metronome, or a pitch-fork. The latter instrument, first used for this purpose by Thomas Young, is employed when the duration of rapid motions like muscular contractions is to be ascertained. I show you the pitch-forks employed for this purpose. (See Fig. 30.) This pitch-fork apparatus is constructed by König, of Paris. It consists of several pairs of pitch-forks of different lengths. One ought to be provided with two pairs, one giving 30, the other 120, double vibrations, in a second. A *pair* consists of two forks which vibrate in unison. One fork (B) is employed for recording the vibration; the other (A) is used for the purpose of causing the first to vibrate. The forks are fixed in steady wooden frames, and they can be moved backwards or forwards in these frames, and fixed in position by screws (S and S′). There are two electro-magnets (*a* and *b*), one in relation to each fork. The soft iron cores of these magnets do not touch the forks in any way, but when electrified they attract the limbs of the forks, and so cause them to move. Indeed, the forks vibrate before the magnets just as Neef's hammer vibrates before the electro-magnet of an ordinary galvano-induction machine. The forks are kept constantly vibrating by a galvanic current derived from two Grove's cells. Sometimes you may require three cells, but the current must not be too strong, else it causes the forks to rattle against the soft iron of the magnets, thereby greatly disturbing the vibrations of the forks. We always require to set the first fork A, and it may be the fork B also, vibrating by striking it with a pencil, or some such thing (never touch the steel with you fingers), and once they are set agoing the electricity keeps them vibrating in this way: The electric current passes along the wire to S, thence to the coil *a*, thence to mercury in a cup (*c*). When the upper limb of the fork A descends, it dips into the mercury a platinum point, and so forms the electrical contact; the electricity passes along the fork A, the wire connecting this with the coil *b* by the screw *f*, from thence to the battery along the wire joining it with the screw *f* at the other end of the coil *b*. During the vibration of the fork A the platinum point is

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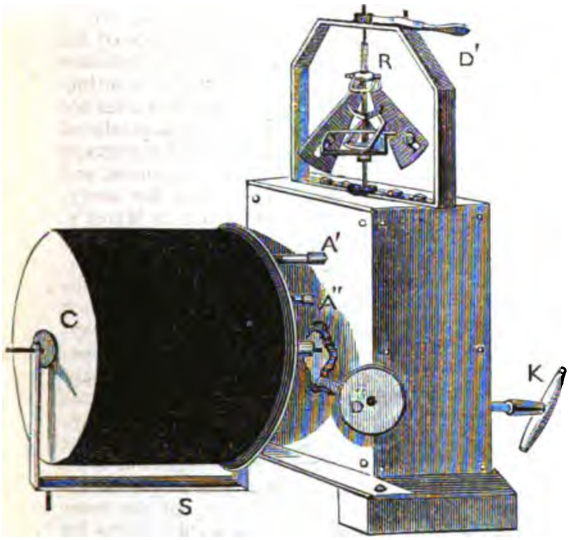
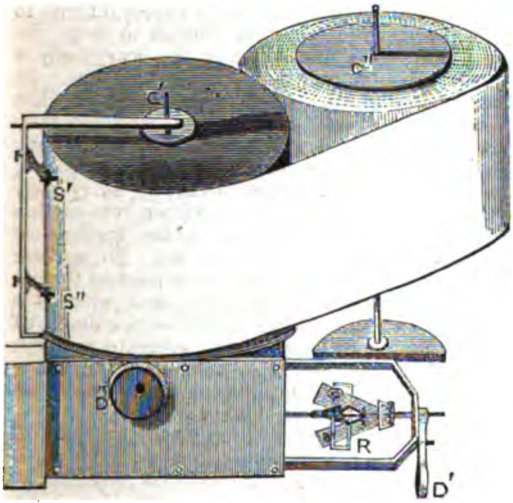
[*] Loc. cit.

Fig. 28.



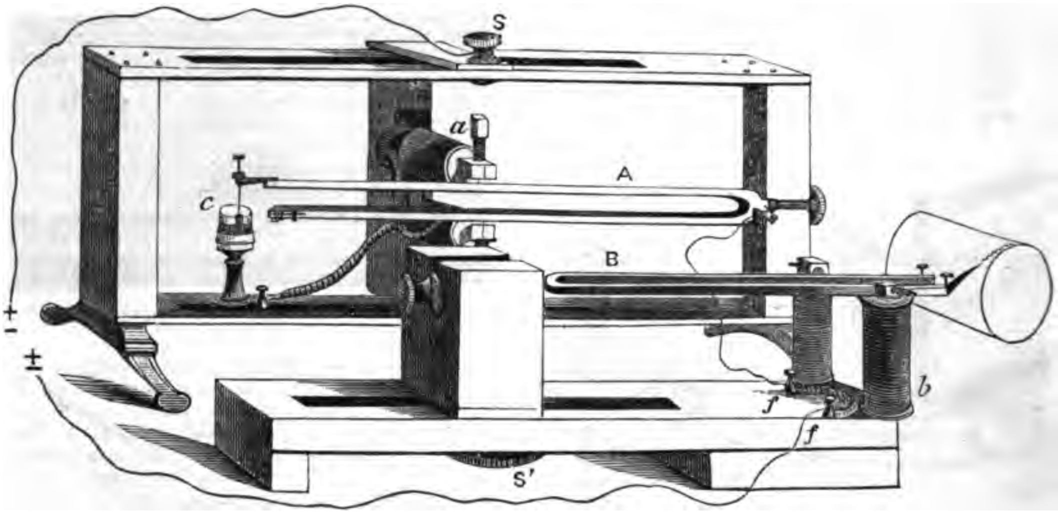
Marey's registering apparatus, with Foucault's regulator, R. C, Cylinder, here covered with smoked paper. S, Support for the cylinder. K, Key for winding up the clockwork. D″, A drag for stopping the movement of the cylinder by retarding the motion of its axle. D′, A drag for stopping the motion more rapidly by arresting the revolution of the axle of the regulator, R. A′, A″, Axles upon which the cylinder may be fixed, in order to obtain different rates of speed.

Fig. 29.



The same apparatus as that represented in Fig. 28, laid upon its side. Its cylinder, C′, is now in a vertical position. The apparatus is here adjusted for taking a tracing upon a continuous piece of paper, of indefinite length. The paper is wound upon a spindle, C″; it is firmly held upon the cylinder, C′, by means of springs with ivory wheels, S′ and S″. When the cylinder, C′, is rotated by the clockwork of the machine it drags the paper off the cylinder, C″. The graphic instrument is placed against the paper on the cylinder, C′, just above D″. The other letters are the same as in Fig. 28. (This arrangement of the instrument has been adapted by Hawkesley, of Blenheim-street, from Ludwig's registering apparatus.)

FIG. 30.

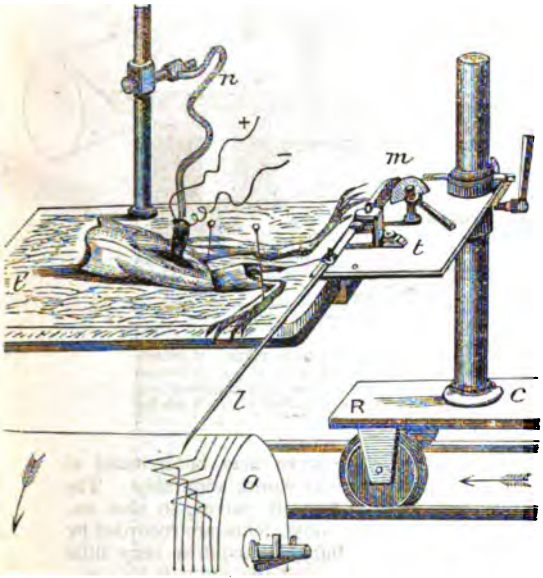


B, Pitch-fork for recording its vibrations upon a cylinder, or any surface on which tracings are to be taken. S', Screw for fixing the block holding the pitch-fork, so that the latter may be drawn backwards and forwards. A, A Second pitch-fork; one limb has a stilette which, during the vibration of the pitch-fork, dips into mercury contained in a glass cup *c*. S, A screw corresponding to S'. *a*, An electro-magnet for driving the pitch-fork A. *b*, A similar magnet for driving the fork B.

repeatedly breaking and forming the electrical circuit, so the electro-magnets are being magnetised and demagnetised with every vibration, and their attractions for the forks in this way established and undone. Hence the fork A merely serves to drive B. You may possibly say, Why have two forks? Would not the fork A alone suffice? Certainly, if the cylinder, for example, on whose surface its movements are to be recorded is placed vertically. But if we place the cylinder horizontally (as in Fig. 30), we cannot on account of the mercury turn the fork upon its side, and so we require another fork placed horizontally. The pitch-fork which I advise you to commonly employ is the one which gives 120 double vibrations per second. If you use a lever for recording the movement of a frog's muscle, for example, upon a cylinder, the writing point of the pitch-fork should be placed directly below the writing point of the lever, so that the time occupied by any part of the tracing produced by the muscle may be ascertained by comparing it with the vibration of the pitch-fork written immediately below. (See Fig. 32.)

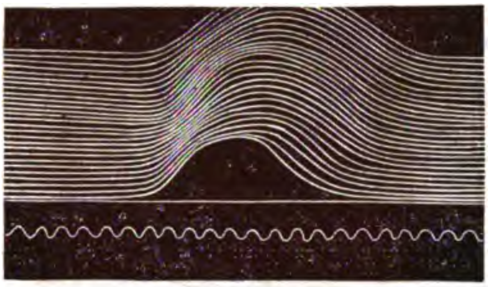
Marey has invented for us two most simple and useful myographs. One of these (Fig. 31) is used for frog's muscles; the other (Fig. 36) is employed for human muscles, or for examining the muscles of the limbs of the larger animals, without removing them from the body. The frog myograph (Fig. 31) is simply a light lever of the first kind (*l*), bearing at its one end a pointed piece of steel or aluminium for tracing upon blackened paper or glass, or a curved aluminium pen for carrying ink. A spring is fixed to the other end of the lever. The end of the spring is in contact with an eccentric, which can be adjusted so as to cause the muscle to overcome greater or less resistance during its contraction. The gastrocnemius muscle is that usually employed; it is attached to the lever by means of a thread or wire wound round or passed through a hole in the tendo Achillis. The limb is fixed by pins to a cork table (*t'*). The sciatic nerve is exposed, and two thin wire electrodes are passed under it, so that the nerve may be irritated at pleasure. This apparatus works admirably. The lever is very light and easily moved, so that extremely fine muscular movements are recorded by it; moreover, being light, it acquires very little momentum, and hence oscillations due to this momentum are almost quite absent—indeed, if the point of the lever press a little upon the paper, so as to cause a little friction, the tracing produced by the writing-point is practically a faithful record of the condition of the muscle. This apparatus may be employed for many purposes—e. g., to study the effect of cooling, heating, or electrifying the nerves or muscle, depriving them of blood, or supplying them with poisoned blood, &c. Generally it is necessary to study the curves produced by contractions repeated at intervals extending over a considerable period. These successive curves might be written with ink upon a long piece of paper (see Fig. 29); but this is inconvenient on account of the difficulty experienced in keeping the pen supplied with ink, and also the waste of paper which it involves. It is better to take the successive tracings upon smoked paper in a single coil around the cylinder. For the purpose of readily comparing one curve with another, the muscle or nerve may always be irritated exactly when the pen, during the revolution of the cylinder, reaches the same point upon the cylinder's surface. The irritation of the nerve can be readily effected by causing the cylinder, when it attains the same point in its revolution, to break and rapidly close an electrical circuit. In this way, when the cylinder revolves, the muscle is made to contract always when the cylinder has completed its revolution. When the muscle is at rest, the lever describes a straight line upon the cylinder's surface; but when the muscle contracts, a curve is the result. If this were repeated many times, the successive curves would all be made on the same part of the cylinder, and nothing but confusion inevitably result. To avoid this, the lever must be made to describe its curve upon a new part of the cylinder at each muscular contraction. This may be effected by causing the myograph to move slowly from the one extremity of the cylinder to the other, so that it may describe a gentle spiral upon its surface. When the cylinder is placed horizontally (as in Fig. 31), the myograph may be placed on a carriage (E) slowly moved along a little railway. Marey moves this carriage by watchwork. It can, however, be moved more simply by attaching a thread to the end of the carriage on the one hand and the axle of the cylinder on the other, the thread being carried round a little wheel fixed to the table opposite the end of the cylinder to which the carriage is destined to move. In this way the axle of the cylinder winds up the thread and draws the carriage with the myograph slowly along the railway. The nerve being irritated always when the cylinder attains the same point in its revolution, we obtain a tracing such as that which I show you. (Fig. 32.) The curves indicate successive contractions of a healthy frog's gastrocnemius. The lowest curve was first produced. The curves become longer and lower as fatigue of the muscle appears. The curves begin on the left side of the figure. The vibrations of a pitch-fork are represented below. In Fig. 33 each great curve is, as in Fig. 32, produced by a *single* shock of faradic electricity transmitted through the sciatic nerve. Observe the vibrations at the crests of the curves, due to slight tetanus. As in Fig. 32, the commencement of the contraction is indicated by the left side of the curve. Another simple mode of altering the position of the tracing upon the cylinder's surface is that which we owe to Dr. Sibson. It is used with the cylinder in a vertical position. There is a little square board which serves as a table, on which the myograph is placed.

FIG. 31.



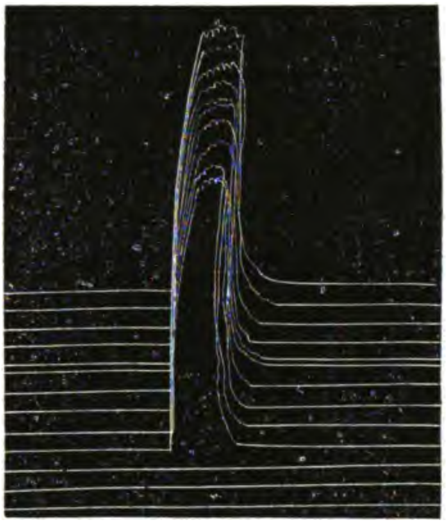
Marey's frog myograph.—*l*, The myographic lever, to which the tendo Achillis is connected by a fine wire. *m*, Spring at end of lever, in contact with an eccentric. *t*, Table bearing the myograph. *t'*, Cork table on which the frog is laid; the leg is fixed in position with pins. *n*, A leaden rod bearing electrodes for irritating the nerve; it is made of lead in order that it may be bent into any form, so as to reach any part of the table. *o*, Cylinder of register moving in the direction indicated by the arrow. (The cylinder ought to have had a blackened surface, and the lines upon it ought to have been represented white.) The myograph is screwed to a vertical rod (*c*), and this is borne upon a carriage which moves upon a railway in the direction indicated by the arrow under the carriage. (After Marey.)

FIG. 32.



Tracings produced by Marey's frog myograph. (See text.) (Diapason, 100 double vibrations per second.)

FIG. 33.

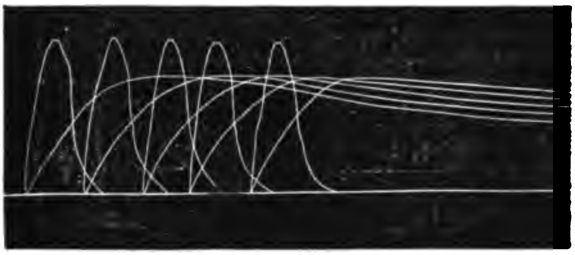


A tracing, taken in the same way as that in Fig. 32 (see text), from the gastrocnemius of a frog poisoned by strychnia. (After Marey.)

Attached to the lower surface of the board is a tube. This fits into a second tube attached to the upper surface of a pedestal. The two tubes glide within one another like the tubes of a telescope. When we begin the experiment, we draw out the telescope tubes, and place the myograph upon the little table which they support. The tubes are now full of air, which can only find exit through a stopcock. When the air is allowed to escape, the table with the myograph descends, and the rapidity of its descent is regulated by the rate at which the air is allowed to escape; this can be nicely regulated by the stopcock. This apparatus is the simplest of all.

But the curves may be still further separated from one another by causing irritants to act, not always when the cylinder has arrived at the same point of revolution, but when it has passed the preceding point for a short distance. (Examine the tracing on the cylinder in Fig. 31; and for an account of the manner in which this is done, see Marey "Du Mouvement dans les Fonctions de la Vie," Fig. 100. Paris, 1868.) Or, instead of causing the myograph to move along the cylinder so as to describe a spiral, it may be left at rest, and lateral position of two curves along a horizontal line (Fig. 34) be the result of irritation being communicated when the cylinder has described a little more than a complete revolution.

FIG. 34.



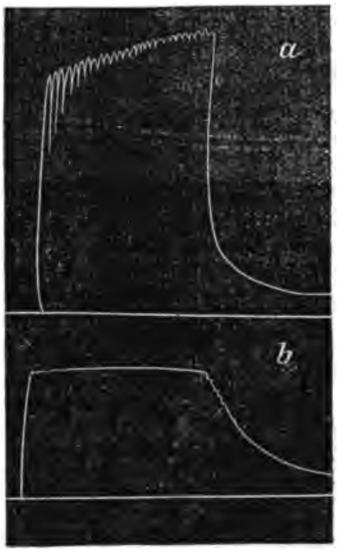
Curves produced by a double myograph.—A lever being connected with each gastrocnemius, the pairs of curves starting together from the horizontal line are due to simultaneous irritation of the sciatic nerves of both limbs. One gastrocnemius, that producing the lowest and longest curves, had been cooled; the other gastrocnemius was normal. The curves at the left hand of the tracing were first produced. (After Marey.)

A single myograph may be used in any of these ways, and much may be learned from it; but often when the actions of toxic agents on nerves and muscles are investigated, it is necessary to compare one limb of the frog acted on by the poison with the other protected from the toxic agent by ligature of the bloodvessels of that limb. For this purpose we require to connect a lever with each gastrocnemius. The points of the two levers are placed one above the other on the cylinder's surface, so that the tracing produced by the one can be readily compared with that of the other. Indeed, the two points may in some cases be placed quite close together when they are at rest without the movement of the one lever interfering with that of the other. In this way we arrive at comparative results of the greatest value. For example, here is a tracing (Fig. 34) taken in this way when the gastrocnemius of one limb had been cooled. It shows at once that by cooling the muscle the contractions take place more slowly, and last longer, than they do in the normal state. Without a doubt this comparative myograph will yet render great service to the therapeutical investigator.

When we give a single galvanic or faradic shock to a motor nerve, or to the muscle directly, we produce a simple or single contraction of the muscle, provided the nerve and muscle be in a healthy condition. Even when the electricity is transmitted through the muscular fibres directly, there is, as we have seen, a distinct interval (*o*, *a*, Fig. 27) between the application of the irritant and the ensuing contraction. This interval—the period of latent contraction—occupies on an average the one-hundredth part of a second, while the entire contraction is, in normal circumstances, usually about the one-tenth of a second in the frog. (Compare the diapason vibrations with the muscle curves in Fig. 32.) Anæmia, cold, exhaustion, and various other influences lengthen the period of contraction. Sometimes in exhausted states of the muscle a single electrical shock gives rise to a number of contractions. A similar result is well seen when a motor nerve receives a single electrical shock in cases of strychnia poisoning. (Fig. 33.)

When I cause a rapid succession of irritants to act on the muscle directly or on the nerve, you see that a rapid succession of short contractions take place; and when the succession of the irritants is rapid enough an apparently constant contraction of the muscle is produced. This is tetanus. When we record the movement, the state of the muscle is rendered more evident than it is to the naked eye. (See Fig. 35.)

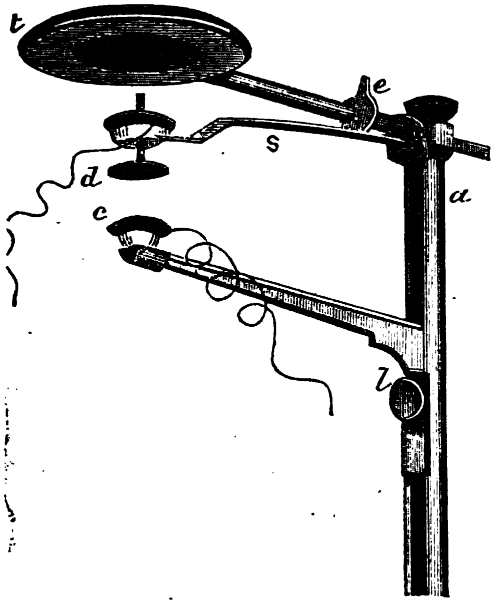
FIG. 35.



a, Curves produced by rapid contractions of frog's gastrocnemius. The first electrical shock produces a great rise (on the left side); the rapidly succeeding shocks produce short contractions. The great fall of the curve on the right side is due to cessation of the irritation. *b*, A curve produced by a succession of shocks as rapid as in *a* communicated to an exhausted muscle; the curves are almost completely fused together, indicating an almost constant contraction of the muscle. This is tetanus.

The myograph which we have used records the shortening of a muscle. Marey has invented another for the purpose of recording the muscular movement by registering the thickening which takes place in the contracting muscle. This myograph is chiefly used in experiments upon the rapidity of motor nerve transmission in man. It is worth while examining the instrument, however; for it is the parent of some instruments recently made for registering respiratory, pulse, and cardiac movements. This myograph is commonly called the "myographic forceps." (See Fig. 36.) It is applied to the adductor muscles of the thumb. These are placed between two knobs (*c* and *d*). One of the knobs (*d*) is fixed to the end of a spring (*s*), the other (*c*) is fixed to a bar that can be moved up and down upon a supporting rod, by means of a screw (*l*). Wires to serve as electrodes, for irritating the clasped muscle if need be, are attached to the knobs. There is a little tambour or drum (*t*) containing air. The lower surface is covered with thin caoutchouc membrane, in the centre of which is a thin plate of aluminium. When the muscle contracts, the upper knob (*d*), already in contact by means of its supporting screw with the aluminium disc, is pressed upon the disc, so as to force the air out of the tambour. This air is expelled through a piece of tubing into another tambour, having a light lever which traces the movement upon a cylinder. (This will be illustrated in next lecture.) In connexion with the spring (*s*) there is an eccentric (*e*), which can be turned so as to vary if need be, the tension of the spring. This myograph, although valuable, is not of such general application as the one about which I have said so much. By means of this myograph, however, it has been ascertained that a voluntary muscular effort does not consist of only a single contraction, such as results from a single electrical shock to the nerve, but that it is made up of a rapid succession of short contractions, due, as Weber long ago suggested, to a rapid succession of nerve influences transmitted to the muscle. A voluntary contraction is, therefore, not a simple but a compound contraction. It is an interesting fact that at each systole of the heart a single contraction occurs. There is not a rapid succession of short contractions such as take place in the muscles of the thumb if they be kept in a state of contraction for a period as long as the contraction of the heart.

FIG. 36.



Marey's improved myographic forceps. (See text.)

Observations

ON

HYPO-VENOSITY OF THE LOWER LIMB.[*]

By JOHN GAY, F.R.C.S.,

Senior Surgeon to the Great Northern Hospital, Etc.

THE term "*hypo*-venosity" is used to express a condition of the limb in which there is a deficiency in the number and size of the veins belonging to the saphenous system; as *hyper*-venosity might be used to express the opposite condition, or that in which there is an excess in the development of these veins, even to varicosity.

The characteristic features of a *hypo*-venous limb are the reverse of those which obtain in a limb affected with *hyper*-venosity; for whereas in the latter, especially with varicosity, the limb is usually lean, and the outlines of bone, muscle, and tendon are, as a rule, sharp and well defined, in the former these outlines become gradually effaced, the skin becomes of a dusky color, the whole limb dense or brawny, and muscular action difficult and painful.[†] With the exception of sometimes, it may be, a few dilated or varicose venous twigs below the ankles, or on the dorsum of the foot, there is scarcely a vein to be seen, except, as it may be, here and there a thin blue line, which exercise or heat fails equally in filling.

The *objective* symptoms of this disease, to which its nomenclature applies, imply imperfect venosity in the subcutaneous or saphenous system of veins, with thickening or hypertrophy of the dermoid and subdermoid tissues; whilst the *subjective* indicate a state of the muscular system in which its fibres are under restraint, and their action attended with difficulty and pain.

The cases are such as these:—

1. An eminent physician broke his ribs. Slight pneumonia followed, and, six weeks afterwards, crural phlebitis, with œdema of the whole limb. He completely recovered. Two years passed away, during which time he favored his leg, and wore an elastic bandage. He then began to complain of rheumatism of the muscles of the thigh. To this phlebitis, in one of the saphenous branches, ensued and passed away; succeeded again, at short intervals, by like attacks in different parts of the limb, excited by slight and those usually physical causes such as rubbing a vein against some foreign body, even against the edge of his elastic stocking. The limb was constantly rested, and gradually became dense and brawny; the skin of a dusky hue. The veins on the surface gradually disappeared; locomotion became difficult and painful, and attended with cramps. There was no œdema.

2. A gentlemen, aged forty-eight. The limb in this case presented the same external features. It has come on gradually, and of its origin or causes the patient could give no account. He was ordered rest and elastic stockings; but with no advantage,

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[*] A paper read at the Medical Society of London.

[†] Complications occur which apparently compromise this rule, such as varicosity in obese persons, or in persons with disease which interferes with the ordinary nutrition of the limb; but a *typical* case of varicosity, as seen in the lower limb, is usually thus characterised.

for the symptoms have progressed, and all traces of superficial veins have disappeared, save only a few tegumentary venules around the malleoli, which are varicose. Some weeks before I saw him he galled his leg, and an attack of phlebitis in a vein near the knee-joint ensued. There is no œdema. The gentleman is goutily disposed. The date of this case is rather more than a year.

3. A gentleman aged forty-six. In this case the attack was preceded by pains across and on either side of the patella; at times across the leg three or four inches above the ankle-joint, and at others across the dorsum of the foot. These pains set-in usually after he had gone to bed, and became more severe after entire rest in the recumbent posture. They occur also by day, but are not so constant or severe. At times he is obliged to dart suddenly from his chair or bed, and to walk about, which invariably mitigates his discomfort. There is always some difficulty in getting his muscles into play, which exercise removes. The leg is somewhat dense; its muscular outlines are becoming effaced; the skin is slightly dusky; and, with the exception of a few small veins on the foot, the external inferior communicating veins, and a small varix on the middle and inner aspect of the thigh, there is no visible trace of a vein in the skin.

In this case the disorder commenced gradually, about six months ago; and has gradually increased, although, under advice, he has rested the limb and worn elastic stockings.

The disease appears to commence insiduously; to be advanced by rest and elastic stockings, the remedies usually employed; and, with its advance, the skin and subdermoid fat layer becomes dense and lose their elasticity. Degeneration and consequent incompetence of the saphenous veins and their branches appear to be associated with its progress in a manner to justify the conclusion that these are its direct or exciting cause; whilst, it is presumed, the deep trunk veins become secondarily dilated and their valves partially inert, and fatty deposit and degeneration take place in the muscles and their connective tissues.

The grounds for this inference are: first, with regard to the saphenous system, that functional deterioration of the saphenous system, through thickening, atrophy, retrecissement, or thrombus, is an occasional pathological fact; and, secondarily, with regard to the deep veins, that saphenous inefficiency, as shown by a varicose condition of the venous radicles—tegumentary or figurative varicosity,—as well as by a dusky color of the skin, otherwise than from melasma or skleroderma, is habitually associated with, and indicates, dilatation of the deep trunk veins. These facts I observed in the course of dissections made some years since, with a view of elucidating the subject of varicosity.

Antecedent to and during the advance of this disorder, phlebitis affecting the superficial veins is a common occurrence. But it is not more than this; it is not, as in phlegmasia dolens, its cause.

Throughout the systems of organic and muscular life the venous system is double—the *superficial* and *deep*, or in other words, the *complementary* and *main;* the former playing to the latter the part of a waste-pipe, or compensating system, ready to relieve it when its vessels are unduly filled; or, to use Mr. Nunn's words, in his essay on the Venous System, "a safeguard against the contingency of the active organs of locomotion. One may compare them," continues Mr. Nunn, "to the adjustment of the chronometer, both having for their object a uniformity of performance under varying conditions." The *deep* veins—viz., those which are radically interwoven with the organs of vegetative and animal life—constitute, therefore, the *real* venous system. The current through these veins, in the healthy performance of the double circulation, is consequently maintained by a combination of forces, which, like those of the heart, are independent of the will, and continuous during rest as during periods and conditions of muscular activity. The current through the *superficial* veins, on the other hand, derives its chief importance from the fact that it receives its principal impulse, in the limbs more especially, from voluntary muscular action; so that, in case its vessels become inefficient, the surplus quantity of blood due to muscular exercise, which they should receive, is poured into the deep veins with a force that, expended on their coats, results in dilatation, valvular incapacity, muscular deterioration, and such other secondary changes in related tissues as prolonged imperfection of the functions of the venous system gives rise to.

It seems to follow, then, in relation to the class of cases to which I am referring, that the practically entire absence of the superficial or *complementary* system of veins, accompanied as it is with the particular condition of the limb that has been described, points inevitably to the *deep* venous system as that to which the symptoms are directly and principally due; and that, as the occasion for the use of the collateral veins is of constantly repeated occurrence, so these, the *deep* trunks, become passively dilated and incompetent to the discharge of the sum of the duties of both systems combined. Even in case of the ordinary competence of the saphenous systems, an excess of muscular action would be attended with like results, but for the development of those supernumerary branches which are often met with as an unusually complex network of veins, and which frequently become varicose. Hence the too common treatment of veins by ligature, *simply because they are varicose*, is not only a useless but a detrimental practice.

As bearing upon this subject, though somewhat discursively, I will call your attention for a few minutes to the forces by which the return of the blood from the extremity of the arterial system is effected. These forces are complex, of which the principal are alleged to be the heart's action, arterial elasticity, and the influence of the nervous system. These are coefficients, each to a certain extent only; for experiments seem to show that each may separately be cut off and yet the blood will find its way back to the heart; and, moreover, that the blood will pass from the arteries into the veins during the suspension of all these forces. Thus the blood still passes into the femoral vein although the corresponding artery is tied, its flow being only retarded; and separation of the nerves of a part from their connexion with the spinal cord does not appear materially to interrupt the some process. To complete the sum of the required forces, therefore, another has been hypothecated—a "capillary" force. Can any such force be shown to exist? or, rather, is there any other force to which the capillary system is especially subjected which will explain the phenomenon? I made the following experiment on dogs some few years since in the presence of many members of the profession.

The whole of a dog's thigh was enveloped in ligature with the exception of the femoral artery; the free ingress of blood into it was thus permitted, whilst its exit was completely prevented. The result was that, after complete repletion, the whole of the saphenous system, to its minutest capillary vessels, and the deep trunk veins were filled to their utmost point of endurance, in parts even to ecchymosis, but there was no corresponding repletion of the muscular tissue. From it the blood was as much expressed as in dogs in which no such experiment had been made. It is to be observed that the *muscular* capillaries were alone emptied, and not the *tegumentary*. This, with another ascertained fact—viz., that after death the temperature of the body may rise and muscular contraction occur, giving rise to its well-known "hum" and the peculiar disposition of the minute capillaries in relation to muscular tissue(as shown by Bowman in his "Anatomy and Physiology," and in some beautiful injections by my friend Dr. Dempsey)—together go far to prove that such a force exists, and that it is in a condition of perpetual activity. It would appear, moreover, that the capillaries derive it from their connexion with muscular tissue, in which it might be supposed to be a power akin to that organic molecular action which is said, with much appearanee of truth, to be an attribute of all sarcode, from that of the amœba to the higher organisms; and that to this force the venous current is mainly due;—that, in fact, as an agent in the circulating system, it is to the *capillary* very analogous in some respect to what the heart is to the *arterial* system. In illustration of this force, and its relation to and independence of the nervous system, I might refer you to the experiments of Dr. J. Reid, from which he conclusively showed that the only ascertained final causes of all endowments bestowed on nerves in relation to muscles is, not to make muscles irritable, but to subject their irritability in different ways to the domain of the acts and feelings of the mind, and, I might add, to the exigencies of the organism. I cannot but regard this force or power as inherent to each form of muscular tissue, for I think, if we take a general view of the relations of the *main* venous tubes throughout the body, with the exception of those of the brain, that their radicles, one and all, commence in muscular capillaries—some in those belonging to the striped, and others to that of the unstriped class; whilst the capillary radicles of the *complementary* veins have no such origin.

It follows that any degeneration of muscular tissue, either as a concurrent or consequent process in this disorder, must necessarily add fresh difficulties to the venous circulation, so that the sum of the final organic changes, of a pathological nature, will be thereby greatly augmented.

It will now be clear, I think, that embarrassment to the current of blood through the venous system at large must inevitably result in defective elimination of the hydrocarbons and their allies from the tissues implicated,—in solid œdema superfascially fatty deposit and degeneration subfascially, and ultimately in elephantiasis, or a disease closely akin to it. Are not other like changes in tissues due to embarrassment to the venous current appertaining to them respectively? and moreover, would not a more close investigation into the mutual organic changes between *venous* blood and the capillary and the next succeeding portion of the venous system throw a considerable amount of light upon the nature of morbid changes generally?

As the *deep* or *main* system of veins is thus especially associated with the nutritive processes, so the *superficial* or *complementary* is as especial eliminatory, for it is from these veins in the lower limb, at all events, that dropsical effusion takes place. Œdematous fluid is chiefly and usually solely superfascial; and it is not improbable that in no other dropsies, as of the pericardium, pleura, or peritoneum the fluid escapes from veins of the same class—viz., from those of their respective complementary systems.

The disorder I have attempted to describe is due, then, *primarily* to inefficiency in the saphenous system of veins, followed by deep vein dilatation and embarrassment.

The treatment is the reverse of that ordinary employed—viz., entire freedom of the limb from all compresses; enforced walking exercise, begun in moderation, and periodically increased; hot applications, especially hot seawater, to the limb; friction; and it may be the internal administration of the liquor potassæ. In short, by the use of all those measures, hygienic and therapeutical, which can, on the one hand, restore the circulation of the limb, upon the principle that its right use is its stimulus to health and perfection; and, on the other, relieve it of its superabundant fat.

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Clinical Lecture

ON THE

SIGNIFICANCE OF UTERINE AND VAGINAL DISCHARGES.

By Robert Barnes, M.D. Lond.,

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We have ranked discharges amongst the most pressing indications for instituting local exploration. In health it may be said that, excepting the monthly discharge of menstruation, there is no escape of fluid from the vagina. It is true that in some women leucorrhœa to a moderate extent precedes and follows the menstrual sanguineous flow; it is also true that, in some, leucorrhœa continues throughout the intermenstrual period without in any obvious way entailing local or constitutional distress. Admitting this, the rational and safe rule in practice still is, to examine in all cases where a discharge at all copious escapes from the vagina attended by pain and signs of constitutional impairment. This may be stated as a general proposition without distinction as to the nature of the discharge. But we will now examine what the discharges are, and what is the special significance of each. And, *in limine*, let us agree upon the meaning to attach to two words which we shall frequently have occasion to use. "Secretion" and "excretion" must be accurately applied. Following continental custom, I shall use the word "secretion" to distinguish the act of separation of the discharges from the free surface of the organs; and the word "excretion" to distinguish the act of voiding from the body altogether. To take an example: the menstrual fluid may be poured out from the mucous surface of the Fallopiad tubes and uterus—that is secretion. The escape of the fluid by the vulva is excretion. Excretion is the natural complement of secretion. But the process may stop short at secretion—that is, the secreted fluid may be retained. Thus, if there be occlusion of the genital canal at any point below the os uteri internum, the secreted menstrual fluid will be shut up in the cavity of the uterus and in the Fallopian tubes. There is no excretion, and therefore, apparently, no discharge.

Taking the discharges as they first come under the notice of the clinical observer, that is, after their excretion, they may be roughly classed under the following heads:—1. Sanguineous. 2. Mucous. 3. Purulent. 4. Watery. 5. Membranous. 6. Solid or fleshy. 7. In the case of fistulous openings into the bladder or rectum, urine or fæces may escape. 8. Then there are foreign matters, fluid or solid, which find their way into the uterus and vagina from without. Amongst these may be mentioned semen distinguished by spermatozoa.

If we limited our inquiry to the examination of these discharges when excreted, we should hardly attain to any more precise knowledge than is expressed in the general terms by which we have designated them. We cannot arrive at a certain knowledge of their source, or from a trustworthy estimate of their pathological significance, unless we examine minutely the organs from which they are secreted.

I may state another proposition: almost all the diseases of the uterus and vagina are attended by discharges.

Of all the discharges, the only one which can be called strictly normal is blood; and this is only normal within certain conditions of circumstance, time, and quantity. Previous histological study will lend the most material aid to direct observation in determining the sources and significance of discharges. We may start from the proposition that, with one or two rare exceptions, all the discharges we have to deal with come from mucous membrane, or at least from organs normally clothed by mucous membrane. The discharges will generally bring with them some of the distinctive elements of the part of the mucous tract from which they are secreted. Hence microscopical examination of a discharge will almost always reveal epithelium-cells which tell their own tale as to the region they come from. In this way we can distinguish uterine mucus from vaginal.

The whole genital tract secretes mucus. It is only when excessive in quantity, or altered in quality, that the secretion of mucus acquires a pathological significance.

The natural mucous secretions are:—

1. A whitish mucus from the Fallopian tubes and cavity of the uterus proper. This probably comes principally from the uterine glands. It has an alkaline reaction. It is distinguished under the microscope by the presence in it of columnar ciliated epithelium-cells. In health this secretion is moderate in quantity, and attracts no attention. But in the condition known as uterine catarrh, it is very abundant, sometimes, especially in aged women, accumulating in the uterine cavity, and causing colic pains to expel it. The uterine mucous membrane may also be stimulated to excessive secretion by gonorrhœal infection spreading from the vagina.

2. A transparent viscid mucus in the cervix uteri. This is also alkaline. It consists chiefly of mucous corpuscles, caudate corpuscles, minute oil-globules, and occasionally dentated epithelium, all entangled in a thick tenacious plasma. In health this secretion is rarely formed in such excess as to appear externally, but it is almost always found in the cervix filling up the canal. The mucous plug thus formed is washed away at each menstrual flow; it exists generally throughout pregnancy. Its uses are probably to shut off the uterine cavity, so as to protect it from external agencies, and to form a suitable medium for the passage of the spermatozoa. At the beginning of labour this secretion is formed in increased copiousness, and serves to lubricate the passages, and to facilitate their dilatation. In certain morbid conditions the cervical glandular structure also acquires extraordinary activity, and then the proper cervical mucus assumes the character of a *discharge*. It is poured in large quantity into the vagina, so freely, indeed, as to be a serious drain upon the system, and a source of weakness. It constitutes the most frequent form of so-called "whites" or leucorrhœa. If the speculum be used, it may be seen issuing from the os uteri as a glairy, albuminous fluid, resembling unboiled white of egg. This exaggerated secretion is almost always the consesequence of inflammation, more or less acute, of the cervical canal—endocervicitis, or of a condition analogous to catarrh of the bronchial or intestinal mucous membrane.

3. A mucus consisting of plasma, not viscid, but containing multitudes of scaly epithelium-cells. This comes mainly from the external surface of the cervix uteri, labia uteri, and the fundus of the vagina. It is of acid reaction. The proportion of epithelial cells to that of the fluid plasma varies considerably. In some cases the fluid part is so scanty that the secretion adheres to the mucous membrane, covering the os uteri as with flakes, or a layer of opaque yellowish-white friable membranous-looking substance, simulating and suggesting diphtheria. Under the microscope this is found to consist almost entirely of scaly epithelium and oil-globules. In other cases, the plasma being a little more abundant, the secretion looks like cream or pus. But in these cases the microscope reveals the same constituents—namely, scales of epithelium. These forms of secretion depend upon chronic or subacute inflammation of the mucous membrane—vaginitis, not necessarily accompanied with abrasion or ulceration. The puriform mucus, more or less opaque and viscid, varying in tinge from creamy-white to yellowish or light green, is often due to gonorrhœal infection, or to suppuration from surfaces denuded of epithelium and granulating. When due to gonorrhœal infection, the mucous membrane from os uteri to vulva is swollen, angry-red, and painful, and the meatus urinarius partakes of the same character. The creamy form of secretion is frequently found during pregnancy on the vaginal portion of the uterus. It is the result of the active throwing off of squamous epithelium due to hyperæmia.

4. The remaining or lower tract of the vagina secretes an acid mucus. Under morbid states this sometimes contains pus-globules, an infusorium, the Trichomonas vaginalis of Donné, and a fungus, the Leptothryx buccalis of Robin. But these para-

sites are really mainly due to neglect of cleanliness. Whitehead suggests that the use of the acid of the vaginal mucus is to prevent the coagulation of the catamenial fluid in the vagina. It certainly seems to possess the property of coagulating the alkaline mucus coming from the cervix. I doubt the correctness of Whitehead's theory. It is important that the blood should not coagulate in the uterus, because clots there cause severe pain and congestion, and are apt to keep up hæmorrhage; but a clot in the vagina is of little consequence. Pus stops coagulation; so does mucus provided the proportion of blood is small. I believe it is the normal mucus which maintains fluidity. Whenever the proportion of blood is greatly in excess it is apt to coagulate.

5. There is a clear viscid secretion from Bartholini's glands, which is discharged in jets during copulation. It has been seen to escape on irritation, expelled by the action of the muscular fibres in the ducts. It is also poured out freely during labour, serving to lubricate the vulva.

6. The small sebaceous and mucous glands of the vulva and labia majora secrete an oily mucus serving for lubrication. This is sometimes increased in quantity, becoming puriform.

We may here refer very briefly to one or two other points connected with mucous discharges. Donné says when the acidity of the vaginal mucus, or the alkalinity of the uterine secretion, is morbidly exaggerated, the spermatozoa are killed. Hence one explanation of the frequency of sterility when there is inflammatory disease of these parts; and of the recurrence of pregnancy when the disease which gives rise to the morbid secretions is cured.

Many years ago* I made the observation that acute exanthemata, as small-pox and scarlatina, which we know affect the whole mucous tract as well as the skin, occasionally left, as sequelæ, vaginitis and leucorrhœa even in children. Graves, Scanzoni, and others have confirmed this observation.

There is a form of leucorrhœa not uncommon in scrofulous children. In such subjects all the mucous membranes are apt to be excessively developed and active. The discharge is chiefly, if not entirely, vaginal and vulval. It is important to bear in mind these sources of leucorrhœa in children, lest we fall into unfounded suspicions that may be suggested to us by others.

Many discharges which to the naked eye cannot be distinguished from pus are really mucus. The microscope discriminates them easily. The distinction is important, because it is generally true that the unbroken mucous membrane of the genital tract does not yield pus. When true pus appears, it is therefore mostly an indication of erosion, ulceration, or abscess. As Virchow has pointed out, all mucous membranes with cylinder-epithelium are little disposed to form pus. The matter which is produced is found on accurate examination to be only epithelium, though it may have a thoroughly purulent appearance. The intestinal mucous membrane rarely produces pus without ulceration. The mucous membrane of the uterine tubes, which is often covered with a thick mass of entirely puriform appearance, shows almost always only epithelial elements.

On other mucous membranes—the urethra, for example—we observe copious discharges of pus without the least ulceration.

The Purulent Discharges.—We have seen that some purulent-looking discharges are in reality mucus, the appearance being due to epithelium-cells, not to pus-globules. When pus-globules in large proportion are found, they indicate generally a breach of continuity of the mucous surface—that is, a granulating or ulcerated surface. When pus escapes in quantities, suddenly at intervals, and sometimes by continuous draining, the source probably is an abscess whose seat is outside the uterus or vagina, as in what is called pelvic cellulitis, opening into the vagina. In such a case examination by touch internally, and externally in the iliac regions, will reveal the extra-uterine disease. The uterus will be felt set fast by surrounding firm plastic effusion. The os uteri will generally be found in the centre of the pelvis, low down, or inclined to one side, if the pelvic peritonitis is chiefly unilateral. This position of the os uteri distinguishes pelvic peritonitis from retro-uterine hæmatocele, which pushes the os uteri forwards, close behind, and sometimes above, the symphysis pubis, and which may also be attended by suppuration.

I have now under my care a case in which pus is voided by the vagina, the origin of which is an abscess in the left hypochondriac region opening into the intestine, and which at a lower part has formed a fistulous communication with the vagina. You thus see how numerous and strange are the sources of pus in the vagina, and that a purulent discharge is no sure evidence of disease of the uterus or vagina. Your exploration must extend beyond these organs.

The watery, sanguineous, membranous, "fleshy," and other discharges we shall study on ensuing meetings.

Original Papers.

ON THE

FUNCTIONS OF THE SYMPATHETIC SYSTEM OF NERVES, AS A PHYSIOLOGICAL BASIS FOR A RATIONAL SYSTEM OF THERAPEUTICS.

By EDWARD MERYON, M.D., F.R.C.P.,

Late Lecturer on Comparative Anatomy at St. Thomas's Hospital.

THE studious research which has occupied the minds of many physiologists of late years, relative to the special functions of the sympathetic system of nerves, has induced me to enter the list of inquirers; and although I can only claim, as a qualification for the task I have undertaken, a somewhat extensive course of dissection, not only of the human subject, but of the lower animals, together with the possession of numerous facts accumulated by the observations of others, and which I have in common with every physiologist, I venture to offer such generalisations as will, I trust, lead to more positive knowledge than we now possess.

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* Medical Gazette, 1850.

In the first place, I have long thought that there is great significance in the fact that every sympathetic ganglion is connected with both motor and sensory nerves, as well as with its own special nerve-fibres (the so-called nerves of Remak).

Thus the superior cervical ganglion, in addition, to the fibres of the sympathetic proper, receives branches from the three or four upper cervical nerves: one branch from the hypoglossal, one from the pneumogastric, and one from the glosso-pharyngeal.

Its affiliated ganglion, the petrosal, has a branch given off to it from the facial nerve, and one from the glosso-pharyngeal.

When the carotid ganglion exists in the cavernous sinus, it invariably receives a branch from the sixth nerve, and one from the vidian nerve, proceeding from Meckel's ganglion.

The lenticular ganglion has a communicating branch from the motor oculi (the third), and one from the nasal branch of the ophthalmic nerve.

The otic ganglion receives a branch from the motor branch of the inferior maxillary nerve, one from the facial, and one from the glosso-pharyngeal.

The spheno-palatine ganglion, or the ganglion of Meckel, communicates with the facial nerve, through the intervention of the vidian nerve, and with the superior maxillary nerve.

The submaxillary ganglion has a communicating branch from the hypoglossal and chorda-tympani combined, and one from the gustatory nerve.

The middle cervical ganglion receives combined motor and sensory nerves from the second, third, fourth, fifth, and sixth cervical nerves.

The inferior cervical ganglion has also communicating with it branches from the seventh and eighth cervical and first dorsal nerves; it acquires branches likewise from the phrenic and lower laryngeal or recurrent nerves, the latter consisting of sensory fibres from the vagus.

The cardiac ganglion is formed by the cardiac nerves from opposite sides, which contain fibres from the cerebro-spinal system; also by branches from the main trunk, and from the recurrent division of the pneumogastric.

Each of the thoracic ganglia receives two communicating branches from the corresponding spinal nerves.

The semilunar ganglia are formed chiefly by the great splanchnic nerves, the phrenic, and the pneumogastric nerves.

The renal ganglia receive compound branches from the lesser splanchnic and the lumbar nerves.

The lumbar ganglia have each two communicating branches from the spinal nerves.

Finally, each of the four pairs of sacral ganglia and the terminal ganglion on the coccyx receives two cummunicating branches from the spinal nerves.

The same combination of motor and sensory nerve-fibres obtains in every plexus formed by the sympathetic system. They are not very abundant, however, in the mesenteric plexuses, but they exist nevertheless. In the hypogastric plexuses they abound, as they do in the parotidean plexus, likewise in the inferior and uterine, which receive two or three branches each from the sacral nerves.

An interchange of fibres takes place between each pair of ganglia in the main cords of the sympathetic system and the corresponding spinal nerves. White nerve-fibres proceed from the spinal cord to the ganglia, and grey nucleated fibres proceed from the ganglia to the spinal cord; and it has been shown by Kölliker that in man and the higher animals more grey fibres pass from the ganglia to the cord than white fibres from the cord to the ganglia. He also confirmed an observation made by Dr. Beck, that the grey fibres give off minute branches to the bloodvessels in their passage to the cord. The remaining branches have been traced by Müller both to the anterior and posterior roots of the spinal nerves.

It is this interchange of fibres, probably, which determines the position of the principal sympathetic ganglia in the immediate vicinity of the spinal cord.

Each ganglion consists of cellular and fibriform substances, surrounded by a comparatively dense tissue, which is a continuation of the so-called sheath of Schwann.

The structure of the ganglionic cells has been carefully studied and described by Dr. Lionel Beale as possessing the same general character in all animals.*

The substance of the cell consists of more or less granular material, and near the fundus there is a large circular nucleus, with its nucleolus. The size of the cell with its investing membrane, which is also a continuation of the sheath of Schwann, is about the 0·060 of a millimeter in diameter; the granular matter may have a diameter of about 0·080 of a millimeter; the nucleus, of about 0·012; and the nucleolus, of 0·002.

In the centre of each cell the granular matter gradually assumes the form of a nerve-fibre, which projects to constitute a process or pole of the cell, and is continued outwards in the form of what Dr. Lionel Beale has called the straight fibre. At the circumference of the cell the granular matter also assumes the form of a nerve-fibre, and projects in close proximity with the pole of the straight fibre, around which it winds in a spiral manner. These latter, or spiral fibres, according to Beale, after having surrounded the straight fibres, are continued in a direction parallel with them for a short distance; but eventually turn and take a course diametrically contrary to that taken by the straight fibres. Both straight and spiral fibrils can be shown to be continuous with the granular matter of which the body of the cell is composed.

In the sympathetic ganglia unipolar cells also exist, each one appearing to be a mere enlargement of the axis-cylinder of the nerve; but it may contain many nuclei and nucleoli.†

On emerging from the ganglia the nerve-fibres have a very complicated and intricate arrangement; but it is enough for our purpose to be certain of the continuity of the several nerve-fibres with the unipolar or multipolar cells. Dr. Beale, in the paper above referred to, asserts that nerve-cells are always connected with nerve-fibres; and Max Schultze endorses the fact by the statement that the processes of cells are nerve-fibres, as was first observed by Remak in the vertebrata, and by Helmholtz among the invertebrata.‡

Valentin and Bidder originated a doubt relative to the nature of the so-called grey fibres; Bidder supposed them to be a variety of areolar tissue;

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* Philosophical Transactions, 1863, vol. cliii., p. 539.

† Human and Comparative Histology, by Stricker, translated by Mr. H. Power for the New Sydenham Society.—General Characters of Nervous Tissue, by Max Schultze, p. 175.

‡ Ibid., p. 174.

but they are now generally accepted as nerves, and are recognised as such under the several names of nuclear fibres, fibres of Remak, non-medullated sympathetic fibres of Max Schultze, and gelatinous fibres of Henle.

Remak, in his essay on multipolar cells, maintains that these bodies in the main cords of the sympathetic become continuous, by means of their caudate processes, with the axis-cylinders of both the broad and fine cylindrical white nerve-fibres, and with the gelatinous grey fibres. It may therefore be affirmed that every form of nerve-fibre in the ganglia is connected and continuous with the ganglionic cells.

Every ganglion thus possesses all the elements of a nervous centre; and the researches of physiologists tend to confirm the inference that such, in effect, is the case; and that each, in its own sphere, is capable of receiving, transmitting, originating, and reflecting impressions on which the healthy functions of the organs to which its nerves are sent depend.

The question relative to the manner in which nerves terminate is an exceedingly interesting one; and if the sympathetic nerve-fibres could be unequivocally traced to the special tissues wherein they end, a fair inference might be drawn as to their function. Some have been followed to their peripheral extremities by Dr. L. Beale, and he has described them as distributed over the walls of vessels. Dr. Tyson, of Pennsylvania, has endorsed his views; and Professor Eberth, of Zurich, has demonstrated the presence of nerves in the coats of all vessels, the capillaries excepted, even in the tunica adventitia of the non-muscular veins of the pia mater. And these, be it observed, *partly consisting of dark-edged, and partly of pale fibres*, which break up after they have penetrated the tunicia adventitia into a fine net-work.*

There can be little doubt, however, that the sympathetic nerves also stand in intimate relation to the secreting cells of glands. Their distribution in the salivary gland has been described by Pflüger.† In this organ the dark-edged or medullated nerves constitute the greatest number and accompany the salivary tubes, perforate the membrana propria, then divide into innumerable fibrils, each of which becomes continuous with a salivary cell. The pale or non-medullated nerves are composed of extraordinarily fine fibrils, each of which is continuous with the fibrillated substance of the epithelial cells.

Such being the anatomical relationship subsisting in the so-called sympathetic system of nerves, much circumspection is necessary in drawing conclusions from experiments, seeing that the cerebro-spinal nerves are intimately commingled with the nerves of Remak, all running parallel to each other.

I may observe incidentally that such compound nerves appear to fulfil all the conditions which are said to be necessary for what is-called electro-tonus (a state whereby one nerve is rendered active by the activity of another in close proximity with it), and many phenomena which I may have to adduce seem to point to such influence; but I shall endeavor to confine my remarks to such experiments and observations as have reference to single nerves only.

This, however, can be done in no other way than by comparing the phenomena induced and presented by the cerebro-spinal nerves alone, with such other phenomena as are produced by the special agency of the fibres of Remak, and by investigating in what respect the mode of action of the one set differs from that of the other; for we may assume that nature is too good an economist to endow the fibres of one with attributes possessed by the other when included in the same nerve.

It is very probable that all may be in some degree modified; and it is difficult to conceive why the ganglionic cells are the connecting media between the three sets of nerves if some change of function be not the result. In illustration of this, the sensory nerves appear to lose much of their peculiar sensibility, as we recognise it in their ordinary impressions on the brain, when they proceed from an organ to a ganglionic centre of the sympathetic system; and if so it follows that, by virtue of the reciprocal action which sensitive and motor nerves exercise on each other and on the organs to which they are distributed, the motor fibres must be invested with properties derived from the modified sensitive nerves.

What, then, is the special function of each different form of nerve-fibre respectively which goes to or proceeds from every ganglionic centre?

With the view to an explicit answer to this question, the most obvious method is to select from the three forms of nerve-fibre the single one which is invariably associated with a peculiar phenomenon, and without which that phenomenon does not occur. The logical "method of difference," in effect, is that which I shall endeavor to pursue.

In M. Claude Bernard's experiments on the sympathetic nerves, the results of which he communicated to the French Academy,* the interesting fact was educed, that whenever the sympathetic nerve in the neck of a rabbit is divided an elevation of temperature occurs in the tissues on the corresponding side of the head, amounting to 7° Fahr. when contrasted with the injured side. This increase of heat was plainly perceptible by the hand, and admitted of accurate measurement by the introduction of the bulb of a thermometer within the nostril or into the external auditory meatus. The whole body shared, to a certain extent, in this development of heat, and exhibited evidence of a temperature exceeding the natural standard; but it was most evident on the side of the neck where the sympathetic had been divided, and least so on the opposite side where it had been left uninjured. The mercury rose to 72° Fahr. on the affected side, but only to 68° on the uninjured side. Nor was this elevation a transitory phenomenon, for it continued with remarkable steadiness until the animal was killed, and even after death the side of the neck on which the experiment was practised was the last part of the body to lose its vital heat. In some other cases the increased heat disappeared; but in no instance was there œdema, nor any morbid phenomenon resembling inflammation.

In addition to the above phenomena, M. Bernard subsequently noticed an increased temperature of the cerebral hemispheres, as well as of the blood itself in the internal jugular vein on the side in which the sympathetic nerve had been divided.†

Dr. W. Ogle communicated to the Royal Medical and Chirurgical Society the history of a suppurating tumour in the neck of a man which produced a lesion equivalent to a division of the cervical sympathetic.[*] In this case the ear on the affected side was redder, and warmer by two degrees Fahrenheit, than that on the opposite side; and there was a total cessation of cutaneous secretion on the right (*affected*) side of the face, head, and neck, although the skin of the right cheek was pinker than that of the left.

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* Stricker, op. cit., vol. i. p. 266.

† Ibid., vol. i., pp. 433-448.

* Comptes Rendus, vol. xxxiv., p. 472. Fevrier, 1852.

† Memoires de la Societe de Biologie. 1853.

We may infer, then, that the disseverance of the minute arteries from the influence of the nerves of Remak is productive of increased vascularity and an elevation of the temperature of the parts so disconnected.

M. Claude Bernard exposed in a dog the gustatory nerve, the chordæ tympani (before they receive communicating branches from the lingual nerve), and the submaxillary ganglion. Having thus before him a sensory nerve, a motor nerve, and a ganglionic centre, he divided the gustatory nerve, and the secretion from the gland was immediately stopped. He then pinched the centripetal end of the cut nerve which communicates with the brain, and a large quantity of saliva was secreted, whilst the ducts of the parotid and sublingual glands remained dry.[†] He subsequently varied the experiment by isolating the submaxillary ganglion entirely from the brain and the spinal cord. He then inserted a tube into the Whartonian duct, and communicated a weak current to the peripheric end of the divided nerve. Every time that this was done a drop of saliva was seen to fall from the tube. Thus secretion was arrested by section of the tympano-lingual nerve, and reproduced by the stimulus of electricity communicated to it. When applied to the centripetal end of the divided nerve, the electric current had no effect.

These experiments supply us with something like evidence from which to infer that secretion may be, in some way or other, dependent on cerebro-spinal influence; and as the nerve-current of a sensitive nerve, on which the first experiment was performed, is centripetal, the irritation of pinching the central portion of the cut nerve could only be conveyed to the gland by the reflex action of the returning motor nerve. Whereas when the ganglion was entirely isolated from the brain and spinal cord, secretion was re-established by communicating an electric current through the peripheric end of the combined motor and sensory nerve. But the electric current has no effect when applied to the peripheric portion of a divided sensory nerve; and we are therefore warranted in the assumption that it is through the influence of the motor nerve that secretion is re-established.

Something more, however, is suggested to the mind by the fact of the ducts of the parotid and sublingual glands remaining dry, whilst the Whartonian duct poured out saliva; and that is, that every gland stands in relation to a special act, and that its function is determined by a special and independent influence.

In another dog M. Claude Bernard exposed the nerves and ganglion as before; but, instead of the nerves, he made a section of the ganglion, and left the commingled nerves going to the tongue intact. The secretions of the gland immediately increased and became continuous, and the amount of saliva was still more increased by the application of an excitant to the tongue. But the influence of this latter operation can only be effected by reflex action, as in the case of stimulating the central cut end of the gustatory nerve through the sensory or centripetal nerve-fibres, and back again through the centrifugal or motor fibres to the gland.

And such is the result of similar experiments upon every secreting organ in the animal body, only in some they may be made more satisfactorily than in others.

If the facial nerve be divided as it passes out of the stylo-mastoid foramen, the secretion of the parotid gland is immediately arrested; but if that nerve is left uninjured, and the nerves of Remak (which proceed from the superior cervical ganglion to be distributed to the ramifications of the internal maxillary artery) are cut, the secretion of saliva goes on more abundantly and continuously. It has also been observed that, when the floor of the fourth ventricle is slightly wounded in the immediate vicinity of the nuclei of the fifth pair of nerves, well-marked salivation from the parotid glands is set up. If the wound be in the mesial line, ptyalism is induced on both sides; but if the wound be on one side, then the increased secretion is occasioned in the parotid of the opposite side.[*]

The results of these experiments are strictly in accord with those on which I have already commented: arrested secretion by section of the motor fibres of the vaso-motor nerves, increased secretion by section of the fibres of Remak, and increased secretion by excitation of the sensory nerves.

M. Claude Bernard divided the sympathetic in the upper part of the dorsal region of a horse; a greatly increased vascularity was the immediate result, and the corresponding parts of the surface were bathed in sweat.[†]

He also found considerable distension of the pericardial vessels, and serous exudation from them, after injuring the cardiac ganglia of the sympathetic. This experiment was repeated and verified by Schiff; and Remak explained the phenomena by the assumption that when the bloodvessels are deprived of the inhibitory influence of the sympathetic nerves proper, they dilate and allow blood-corpuscles to penetrate into those minute arterioles through which blood-plasma only should be propelled.[‡]

On injuring the solar plexus, or on dividing the main trunks of the sympathetic, Budge ascertained that the circulation of the blood in the liver is increased and the secretion of bile augmented, and in two cases he found the liver itself enlarged. On extirpating the mesenteric plexuses in rabbits, the fæcal pellets became so soft that none of the ordinary rounded masses were found in the rectum. The fæces were pulpy, and covered over with a slimy mucus.[§]

Jaschkowitz, by dividing the sympathetic nerves of the spleen in cats and dogs, caused an increased flow of blood to that organ, and a copious deposit of hæmatin pigment in its cells.[||]

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[*] Medico-Chirurgical Transactions, vol. lii., p. 151. 1869.

[†] Comptes Rendus, vol. xxxiv., p. 474.

[*] Medical Times and Gazette, 1860, p. 362.

[†] Ibid., 1861, p. 544.

[‡] The ulterior stages of inflammation have been still further explained by Waller, Reicher, Cohnheim, and Stricker, by the proposition that pus-corpuscles are nothing but the colorless blood-corpuscles filtered from the blood through the walls of capillary vessels. In a very elaborate paper contained in Virchow's *Archiv* for Sept. 1867, Cohnheim demonstrates the truth of this proposition by exciting inflammation in the peritoneum of frogs.

[§] Nova Acta, Acad. Cæs. Leop. Car. Nat. Cur., xix., p. 257, 1860.

[||] Jaschkowitz de Discisionis Plexus Lienalis efficitate in Lienem.

In the experiments instituted by Bernstein on the pancreas, he discovered that, notwithstanding the absence of secretion in that organ during fasting, on dividing the sympathetic nerves going to it a continuous flow of pancreatic fluid is produced.* The originating power of the ganglionic centres is indicated by the fact, that when secretion is thus artificially produced it is not arrested by placing the animal under the influence of woorara. The special relation, moreover, in which the action of the pancreas stands to the digestion of food, increasing immediately after a meal, attaining its maximum about three hours after, and then gradually diminishing,† shows the local centralisation of nervous influence.

The structure of the kidneys is almost suggestive of the function of each component part.

The Malpighian tufts, and the special capillaries surrounding them, shut up within the Malpighian capsules, in which there is but little of epithelial cell structure, exhibit the most perfect contrivance for that simple and independent filtration of fluid which is known as exosmose—a simple outpouring of the watery part of the blood into the capsules, to flush, as it were, the tortuous tubuli uriniferi in front.

The contorted tubuli, surrounded by *their* special capillaries, to which the ultimate fibres of the renal plexus have been traced, and constructed of basement membrane, lined with the rounded, glandular variety of epithelial cells, indicate a secretory structure; and here, undoubtedly, it is that the salts which characterise the urine are eliminated from the blood.

Section of the splanchnic nerves, as performed by Eckhardt, induces hyperæmia of these latter capillaries, albuminuria, and increased renal secretion; and Dr. James Tyson, of Pennsylvania, has shown that by section or stimulation of the gangliated nerve-fibres, the terminations of which he has traced to the minute vessels surrounding the contorted tubuli uriniferi, he produced either distended vessels and an abundant secretion, or a diminished and slower blood-current and a corresponding secretion.

The question as to which of the three forms of nerve-fibres it is that is subservient to the special function of the secretory tissues admits of a positive answer. It cannot be the fibres of Remak, because when they are cut secretion is increased; neither can it be the sensory fibres, for they are centripetal nerves, and their influence can only be of a reflex character. The motor fibres are those only which, going to a gland, cannot be cut without destroying secretion; and those fibres, therefore, must be the cause or condition of the phenomenon. The sensory nerve-fibres may endow the secretory cells with their vital sense, as exemplified in Bernstein's experiments on the pancreas; but the motor fibres must be immediately concerned in supplying blood for increased secretion.

The following case is illustrative of my inference. R. de L——, aged forty-three, presented herself at the Infirmary for Epilepsy and Paralysis in July, 1869, complaining of spasm of the muscles supplied by the facial nerve on the left side. Some two years before she had a severe headache, after which she awoke one morning with her mouth drawn to the left side, with a feeling of stiffness, but no anæsthesia. From that time any extraordinary action of the muscles of the face induces a contraction of the buccinator, masseter, digastric, stylo-hyoid, and orbicularis palpebrarum muscles. The act of sneezing, gaping, or blowing her nose, will produce this spasm, which continues for five or six minutes, when the face regains its tranquil appearance. She has occasional dyspnœa, *a copious flow of saliva from the left parotid gland*, and the left side of the face and head often sweats when the right does not.

Let us now inquire,—What is the agency or influence of the sensory nerve-fibres in the sympathetic system?

When any pungent substance, such as mustard, is placed in the mouth, it produces larchrymation and an increased flow of saliva. Such increment of secretion results from the application of any excitant to the extremities of the sensitive nerves which emanate from glandular organs. Professor Ludwig has affirmed that the augmented exudation is owing neither to contraction of the muscular elements of the glands, nor to a change in the pressure of blood in the organs, but apparently to the simple influence on the secreting tissues; that a stimulus to secretion is set up, not by any direct influence on the bloodvessels, but by an incitement set up in the secreting glandular tissue. This may be a refinement on a physiological definition; for it is difficult to conceive how secretion can be effected without a corresponding increase of vascular action, but it is the expression of an ideal reciprocation of tissue with tissue, which is suggestive of spontaneity.

The act of blushing is an apposite instance of the correlation of sensitive and vaso-motor influences. But paralysis of the sensory nerves, although it may diminish, does not stop vascular action; or, on the occurrence of anæsthesia, gangrene should supervene in the parts deprived of sensation. The tendency to such destruction may exist by virtue of the responce given by the motor nerves to those which are rendered insensible, and therefore, of the loss of balance between the incentive and restraining forces which act upon the bloodvessels.

Paralysis of the trigeminus, however induced, causes anæsthesia of the face and scalp, injection of the conjunctivæ, loss of secretion of the larchrymal glands, leucoma, and ulceration of the corneæ. A case of this kind is reported by Dr. Althaus in the 52nd volume of the "Medico-Chirurgical Transactions." In that instance there was also a continuous rushing noise in the head, an abundant discharge of acrid mucus from the nose, and a copious buccal exudation, although the secretion of healthy saliva was arrested.

It is a curious fact, and a very interesting one, that the section of a sensitive nerve is very apt to be followed by a pseudo inflammation in the parts to which the peripheral extremities of that nerve are distributed. Professor Graefe has abundantly proved that such apparent inflammation in distant parts is very apt to follow the section of a sensory nerve by which those parts are supplied. According to the view here taken of the special function of the sensory sympathetic fibres, it is reasonable to suppose that the phenomenon is due to a loss of tonicity of the walls of the capillary vessels, owing to a suspension of the inhibitory influence of the vaso-motor nerves; and that blood-corpuscles may thus penetrate into minute vessels, where only blood plasma, nutritive fluid, or effete matter should be carried to the surface.

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* Sachs Akad. Sitzungberichte Math. Phys. Class, 1869.

† Arbeiten aus der Physiologischen Anstaltzen. Leipsig, Vierter Jahrgang, 1869.

It is this peculiar nerve influence, modifying indirectly the nutrition of organs, which has given rise to the idea of trophic nerves; and in all cases the nerves so implicated are sensory nerves. Their action is exemplified in gastrodynia and dysmenorrhœa, in which they induce an increased flow of mucus; but in both diseases the pain is referable to nervous irritation, and independent, as Ludwig would have it, of vascularity of the secreting membranes.

Many instances are on record in which the so-called trophic nerves have manifested their influence on the nutrition of parts after accidents by which the sensory nerves of those parts have been divided or destroyed. Three cases of the kind are related by Mr. Simon in Holmes's System of Surgery; * and one has been described by Mr. Nunn,† in which the ulnar nerve was divided in a boy. The temperature of the hand fell to 10° Fahrenheit below that of the sound side; and the muscles supplied by the divided nerve were wasted in the course of two months to one-third of their former bulk. The London Hospital Reports for 1866 contain the details of seven cases of a like nature; and Dr. John Ogle describes a case of disease, in which the sympathetic must have been implicated, and a changed condition of the vital attributes in the parts ensued,‡ which, as in all the other instances, manifested itself a long time after by a lowered temperature, a defective nutrition, and a disposition to textural degeneration.

All these phenomena are necessarily induced by reflex action, either through the cerebro-spinal axis or the sympathetic ganglia; and according as the afferent nerve is irritated or rendered apathetic may it either exalt or repress the functions of the nervous centres upon which it acts. On this theory Dr. Handfield Jones and Mr. Lister § explain the distant effects of morbid changes.

In the foregoing experiments the effect of section of the fibres of Remak has been shown, in order to judge of the resulting action of the motor and sensory elements of the sympathetic system; and it is obvious that in all cases the attributes of the sympathetic proper are the very antitheses to those of the cerebro-spinal nerves. But as the sensitive element exerts its immediate influence on the nervous centres only, and the motor portion of the ganglionic centres on the peripheric tissues, it may be asserted that the latter and the fibres of Remak are antagonistic in their action.

Accordingly it is found that the section of the motor nerve of a gland produces precisely the same effects as stimulation of the corresponding sympathetic branch; and, *vice versâ*, that section of the sympathetic has the same effect as stimulation of the motor branch: both pointing to the fact which M. Claude Bernard has enunciated—that the nervous filament which presides over the functions of a gland is always from a motor nerve.‖

I infer therefore that the special function of the sensory fibres of the sympathetic is to communicate a stimulus to the secretory glandular tissue, to give an organic or vital sense to those tissues, just as the muscular sense is conveyed from the muscles to the nervous centres to communicate a stimulus to muscular action.

To what particular tissues, then, and to what special function, are the sympathetic nerves *proper*, or the fibres of Remak, subservient?

Their actions are undoubtedly complex; yet I hope to show (and I cannot discover from physiologists that I am mistaken) that those actions are confined to the muscular coats of vessels only, and that their function is chiefly to control the act of nutrition.

I have had occasion to refer to the researches of Dr. J. Tyson on the mode of distribution of nerves to minute arteries, and he has shown how the primitive fibrillæ of nucleated nerves are disposed upon the arterioles, which are distinguished by transverse nuclear markings—probably the contractile elements,—and how other strands of naked axial fibres, on the uriniferous tubes of the kidneys, gradually become surrounded with a medullary tunic, and then with the tunic of Schwann, to constitute nerve-fibres which he believes to be sensory or afferent nerves, conveying to the centres an impression, the response to which is transmitted to the efferent nerves, or those which terminate on the transverse marks of the arterioles, whereby they contract or dilate their calibre. Such is the probable disposition of the grey fibres on the coats of vessels.

As before observed, I have anticipated experiments on the sympathetic fibres to show the behavior of the motor fibres without them; and in every instance in which the former have been divided, an increased vascularity, an elevated temperature, and an augmented secretion have resulted.

Another curious and instructive phenomenon occurs when the blood is thus transmitted in a preternatural quantity through the capillaries—the venous blood immediately becomes brighter in color. M. C. Bernard observed this fact in the coronary veins on the left side of the lip of a horse after he had divided the left cervical sympathetic.*

Now, the application of a weak electric current to the peripheric end of the divided sympathetic reverses all this. The calibre of the distended capillaries is quickly reduced; the temperature is lowered, and may be depressed below the existing degree in other parts, and secretion is diminished. If the power of the current be increased, the circulation may be entirely arrested; so that, if examined under a microscope, the capillary vessels will seem to be completely empty.† Such is the invariable result of stimulation of the nerves of Remak on the capillaries; and MM. Valentin, Henle, and Budge have observed that the large bloodvessels contract, when acted on by galvanism, through the medium of the grey nerve-fibres which are supplied to them.

If all this be so, we have at once a solution of very many physiological and pathological phenomena, and, what is more, a clue to the rational treatment of many diseases.

Mr. Moore, in his interesting little treatise "On Going to Sleep," shows the one way only in which sleep can occur; and *that*, the action of the sympathetic fibres in diminishing the arterial current through the brain. One step more and we have the key to the convulsions of teething; for M.

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* Vol. i., p. 36. Second Edition.

† Pathological Transactions, 1866.

‡ Medico-Chirurgical Transactions, vol. xli., 21st case.

§ British Medical Journal. Feb. 5th, 1859.

‖ Medical Times and Gazette, 1861, p. 645: Lectures on the Spinal Cord.

* La Clinique Europ., 1859; No. xxix., p. 282.

† Medical Times and Gazette, 1861, p. 543: M. Claude Bernard on the Spinal Cord.

Loven has shown that by irritating the peripheric extremities of sensitive nerves a reflex influence is produced on the vaso-motor nerves, inducing contraction of the capillaries; * and, as MM. Kussmaul and Tenner have explained, convulsions.† They have demonstrated, moreover, that a suddenly induced anæmic state of the brain produces epilepsy; and how frequently do both convulsions and epilepsy occur during sleep. Yet another step in the arrest of circulation, and we have the pathological state which M. Eckhart has produced experimentally—namely, an over-irritated condition of the sensitive nerves reflected on the vaso-motor nerves of the spine, and reflex paralysis as a consequence. It is M. Claude Bernard's experiment on the sympathetic over again; and I believe this to be the most frequent, if not the only, immediate cause of infantile paralysis, which, as far as I have been able to judge, is more relieved by the application of warmth, *as a local remedy*, to the the spine, than by the stimulus of electricity.

In the above forms of convulsions, Dr. Marshall Hall's idea, that when the influence of the cerebrum is suddenly removed the augmented irritability is reflected back on the vaso-motor nerves and muscles, would appear to receive confirmation.

Hemicrania is said by M. Du-Bois Reymond, who has studied it on his own person, to arise from spasm of the muscular coats of the arteries of the brain; and M. Möllendorf, although not agreeing altogether with Du-Bois Reymond, considers that it is dependent on an affection of the cranial vasomotor nerves.

Now the active state of organs is unquestionably the wasting state, and the quiescent condition that of restoration. During mental activity the brain is charged with arterial blood, and in the glands the active state is a state of congestion. During this period the venous blood flowing from those organs is of a bright-red color, just as was observed by M. Bernard in the venous blood of the left coronary vein in the lip of the horse, after section of the left cervical sympathetic.

The period of sleep is regarded by Mr. Durham‡ and Mr. Moore as the period of replenishing the brain, and during the state of quiescence in glands those organs recuperate themselves. Then it is that the cerebro-spinal system is in abeyance, the fibres of Remak dominate, and the venous blood flowing from the organs is very dark in color.

In the face of all these phenomena it cannot be doubted that nutrition chiefly is governed by the sympathetic nerves, whilst local independence is indicated by the local distribution of ganglionic centres, and proved by local vascularity during functional activity.

When the salivary glands are stimulated by the act of masticating and by the savor of food, the bloodvessels of the glands become turgid, and the glands themselves injected. The contact of food quickens the circulation of the mucous membrane of the stomach, and causes it to become red and tumefied. Again, during menstruation, a period of nervous sensibility, the whole uterus is congested; the mucous lining throughout its whole extent is hypertrophied and of a pink color, increasing in depth towards its fundus, where it attains a dark livid color, and is covered over with a quantity of blood; but the congestion does not extend beyond the uterus, the Fallopian tubes, and the ovaries.*

These local phenomena are but the expression of a general law, which is dependent on the distribution of the ganglionic centres of the sympathetic system to every important organ of the animal body; and if an additional proof be required that independent nervous centres exist for the purpose of regulating the local distribution of blood, I would adduce the fact, that although local functions may be disturbed by injuring portions of the brain to which the motor and sensory nerves are affiliated, those local functions are generally restored long before the injured portions of the brain have had time to recover their integrity.

The localisation of such centres is obviously contingent on the position of the organs to which they are subservient, for such is the distribution of the ganglia in all animals, from Echinodermata upwards; and each ganglionic centre, I suppose, gives nurture and individuality to its special outgoing nerves, just as a modified soil may determine the quality of a sapling which has been protruded from the root of its parent tree.

From the foregoing considerations I conclude that the sensitive nerves of every ganglionic centre impart an influence, which I would call the vital sense of an organ, affecting its histological tissues, without operating immediately upon its bloodvessels; that the motor nerves, having their terminal fibres extended to the extreme ramifications of the arterial system, incite vascular action and secretion in response to the vital sense; and that the grey fibres of Remak, having a correlative ramification, restrain and regulate the stream of nutriment which is conveyed by the arterioles into the cell territory for secretion or assimilation.

If I may be allowed to compare organic with inorganic things, I would represent the sensory nerve of every ganglionic centre as the steam of an engine, the ganglionic centre as the cylinder and piston rod, the motor nerve as the fly-wheel, and the fibre of Remak as the governors.

In concluding this part of my subject I will offer a few remarks on the pneumogastric nerves.

The nuclei from which the eighth pair of nerves derive their roots occupy the same tracts of grey nervous matter as do those from which every pair of spinal nerves originate; † and as their source is analogous so also is their function, but being distributed to very important organs, and very diversified opinions being entertained concerning them, I may be pardoned for considering them apart from all other compound nerves associated with the sympathetic system.

I need not dwell on the intercommunicating fibres between the several nuclei in the medulla oblongata, which associate the sensitive portions of the palate, the pharynx, the epiglottis, the larynx, and the respiratory mucous membrane, with the muscles of deglutition, of the voice, and of respiration; nor is it necessary to allude to the phenomena which attend either the section or stimulation of the upper branches of the pneumogastric, whether motor or sensory; the result of such experiments may be predicated. Of the vascular and cardiac branches, however, it is interesting to know that they are distributed exclusively on the surfaces of vessels, or round about the secreting cells of mucous surfaces.

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* Ludwig's Arbeit., 1867.

† Nature and Origin of Epileptiform Convulsions. By Drs. Kussmaul and Tenner (New Sydenham Society, 1859).

‡ Guy's Hospital Reports, 1860: The Physiology of Sleep.

* Pathological Transactions, vol. xiii., p. 170: Professor Harley on the Uterus and its Appendages at the Catamenial period.

† Philosophical Transactions, vol. 158, 1868: On the Intimate Structure of the Spinal Cord and Brain. By J. L. Clarke.

Division of the pneumogastric nerves above the cardiac branches is always fatal, and in almost all cases death results from an excessive congestion of the lungs, the capillary vessels being found in a dilated condition, and a bloody serum exudes from them into the bronchial cells. The secretion of mucus is arrested, as we are led to expect from corresponding results observed in all other cases, where the sensory nerve-fibres of a ganglion are destroyed, as those of the cardiac ganglion and of the minute pulmonary ganglia must be in such an operation.

It has been affirmed by some experimentalists that the division of the vagi paralyses the œsophagus, destroys the sense of hunger, and arrests the secretion of gastric juice; but many careful observers, such as Breschet, Müller, Milne Edwards, Vavasseur, Holland, and Arnold, have asserted that the gastric juice continues to be secreted after section of the vagi; * and this fact may appear to militate against the theory which I have advanced, but I am inclined to think that it rather serves to illustrate it.

The ganglionic centres which are subservient to the functional actions of the stomach are the semilunar ganglia and the coronary plexus of nerves proceeding from them. These ganglia appear to be the expanded terminations of the splanchnic nerves and the multipolar cells with which they communicate. The right vagus, it is true, terminates in the left semilunar ganglion, and the vagi undoubtedly contain both incident and motor fibres; but they do not, with the exception of the terminal branch of the right vagus, form with the sympathetic any isolating ganglion wherein the attributes of nerve-fibres are so modified as to subserve the local requirements of the stomach, either as regards its special vital sense, its secretion, or its nutrition. They may, and most probably do, impart a peculiar sensibility, which, by reflex action, places the stomach in close relationship with the brain, the larynx, the fauces, the œsophagus, and even with the widest system of bloodvessels; but, with the exception of their fibres, which enter into the composition of the pharyngeal ganglia, the cardiac ganglia, the minute ganglia in the coronary plexus, as shown by Dr. Lee, the ganglia in the pulmonary plexus which Remak first described, and, finally, the left semilunar ganglion, the commixed fibres of the vagi have general rather than special functions. When they govern the action of vessels, I am disposed to think that they do so by virtue of the minute ganglia and plexus described and figured by Lehmann as existing in the tunica adventitia of large vessels, and extending their ultimate fibrils to the minute arterioles which have their middle coat entirely composed of muscular tissue.

Guided by analogy, we may infer that nerve-fibres from the semilunar ganglion, after passing through the gastro-hepatic omentum into the transverse fissure of the liver to the coats of vessels in the capsule of Glisson, whither I have traced them, terminate as other vaso-motor nerves do, and that the terminal fibrils of the pneumogastric or sensory nerves are distributed to the acini and biliary ducts.

It is thus that the innervation of the bloodvessels of all the abdominal organs may be effected, and the heart itself may regulate the general circulation by the dictates communicated to it by reflex action from the sensitive fibres of the pneumogastric nerves, and which are responded to by the motor fibres, or by the fibres of the sympathetic proper.

MM. Cyon and Ludwig have shown that it is through the splanchnics that these responses of the heart are communicated over the widest system of vessels in the body.*

The sensitive nerve by which the reflex action of the heart is originated is a separate nerve in rabbits, and may be demonstrated as such. It is formed by two roots—one from the trunk of the pneumogastric, the other from the superior laryngeal nerve. It descends by the side of the carotid artery and sympathetic nerve, and on entering the thorax it communicates with branches from the first thoracic ganglion, and is eventually lost in the substance of the heart.

If this nerve be divided in the neck, irritation communicated to the lower cut end produces no effect; but if the upper or centripetal cut end be irritated, a very sensible effect is produced on the blood-pressure in the carotid arteries.

By such a system of innervation the heart appears to be capable of regulating its own action according to circumstances, by exerting a reflex action on the important vaso-motor nerves of the general circulation.

The sensibility of the walls of the heart being excited by too large a supply of blood, reflex action is set up, which dilates the capillaries, diminishes the blood-pressure, and causes the blood to accumulate in the periphery. If, on the contrary, the internal sensibility of the heart be too feebly excited, the peripheric vessels contract, and blood is attracted to the heart.

The facts which I have thus strung together I will further utilise to show that many therapeutical agents, which have been used without an adequate appreciation of their special mode of action, may be employed with a positive knowledge of the tissues on which they act, and how they act.

SUGGESTIONS IN SUPPORT OF A RATIONAL SYSTEM OF THERAPEUTICS.

If it be true, as I have attempted to show, that every ganglionic centre of the sympathetic system of nerves has three distinct elements over and above the ganglionic cells, and that each element or nerve-fibre has its own special attributes, I will further endeavor to interpret, by the aid of the experiments and observations which I have adduced, the *modus operandi* of many medicinal agents, and to found upon the knowledge so acquired a rational system of therapeutics.

Take, for instance, the fact shown by M. Claude Bernard, that section of the sympathetic proper induces increased vascularity and elevation of temperature in the parts to which those sympathetic nerves are supplied, together with the fact that there are certain medicines which have the faculty of diminishing vascularity and lowering temperature, and we have at once some reason to suppose that the agents in question have the power either to subdue the force which is set free by the section of the sympathetic, or to increase the force which the sympathetic exercises as a restraining or inhibitory agent.

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* Dr. J. Reid: Cyclopædia of Anatomy and Physiology, vol. iii., p. 900.

* Cours Scientifiques, 1868, p. 421.

In some forms of inflammation—and inflammation, in some form or other, lies at the root of most diseases—we have the pathological counterpart of the result induced by section of the sympathetic—a state in which, owing to the loss of the inhibitory influence of the nerve-fibres of Remak, blood-corpuscles penetrate into those minute arterioles through which blood-plasma only should be propelled.

Now in the spurred rye (ergota) we have an agent which produces the very opposite effect. By increasing the inhibitory influence of the nerves of Remak it diminishes the calibre of vessels, and not only shuts out blood-corpuscles from the capillaries, but also the blood-plasma itself, and so restrains many forms for hæmorrhage. M. Brown-Séquard affirms that he has seen a manifest diminution in the calibre of bloodvessels in the pia mater of the spinal cord take place in dogs after they had swallowed large doses of ergota; and in the convulsive ergotism which occurred in Germany in 1770 dry gangrene was a constant accompaniment.

Counter-irritants have a similar effect, but in a modified degree. M. Loven has shown that by irritating the peripheric extremities of sensitive nerves, a reflex influence is produced on the vaso-motor nerves, of an inhibitory character, effecting a diminution of the calibre of vessels.*

At a meeting of German naturalists and physicians held at Innsbrück in 1869, Professor Heidenhain, of Breslau, enunciated the fact, which he had observed from many experiments, that irritation of sensitive nerves produces a rapid diminution of the blood heat.† He also noticed a sensible decrease in the calibre of vessels, and a less frequent pulse.

According to Professor Stilling, "there is a constant reflex influence maintained by a sensitive nerve upon the bloodvessel-nerves." An excitant applied to the extremities of a sensitive nerve induces an increased flow of blood; but if the extremities of a sensitive nerve be destroyed and paralysed, a corresponding reflex action is induced on the motor fibres of the vaso-motor nerves, the ascendancy is given to the inhibitory nerve-fibres of Remak, and a diminished flow of blood ensues.

It has been objected to counter-irritants that no physiological explanation can be given of their action; but M. Loven's demonstration of the effect of irritation of the extremities of sensitive nerves appears to me a good and valid explanation. The observations which I have made on the so-called trophic nerves, and the influence they have on the vital attributes of parts, by increasing or diminishing the supply of blood, have a direct bearing on this interesting subject.

Just as the localised ganglionic centres of innervertion have an independent action, and accelerate or retard the vascular function of any organ, so have we medicinal agents which localise their power on some special organ, and excite or restrain the functions of that organ, in a similar manner as does the section of the nerves of Remak, on the one hand, or the section of the motor fibres of the ganglionic centres, on the other.

We know that opium and chloral exert their action on the brain; digitalis and aconite on the heart; mercury and opium‡ on the liver; turpentine and uva-ursi on the kidneys; phosphorus and bromide of potassium on the organs of generation; &c.

Some of these drugs undoubtedly act on other parts of the body than those with which I have associated them; but for the purpose of suggesting a system of rational therapeutics I have ventured to refer to each as the type of a category in which others may be found to have a more exclusive, though perhaps less positive action.

I have purposely named them in pairs, because I wish to show that each one has an antagonistic action to that with which I have conjoined it: thus, whilst opium stimulates the circulation, and produces an excitement of the motor nerves which often supersedes its hypnotic influence, chloral restrains the blood-current by acting as a powerful sedative to the motor and sensory nervous centres, and so suspends the activity which opium sets up. Opium accelerates the pulse, raises the temperature of the skin, increases the secretions of saliva and sweat, injects the conjunctivæ, and—according to Dr. J. Harley—if these manifestations of vascular excitement are prolonged, as they may be by repeating the medicine, they end in dilatation of the capillaries, general congestion, and imperfect oxidation of the blood; whereas chloral is a powerful sedative to the nervous system. Instead of accelerating the pulse, it reduces its frequency; instead of raising the temperature of the skin, it lowers it; and instead of increasing the secretion of saliva and sweat, it diminishes both.

I will quote one instance out of many already recorded, and from a source which may be the more readily accepted because no theory is sought to be established in the description. In a case of puerperal mania treated by Dr. Head in the London Hospital, two drachms of the hydrate of chloral were given to the patient between 3 P.M. and 6 P.M., after which she slept twenty-four hours. Previous to the first dose her pulse was 120; her temperature 102·4°; and her respiration 30; after the chloral her pulse was 96; her temperature 99°; and her respiration 21; she complained moreover of great thirst.

Now in cases of congestion of the brain, where stupor but wakefulness are the characteristic symtoms, surely it is more consistent with reason to give the chloral hydrate, or conium, than opium to procure sleep. I had a patient the subject of sunstroke, which occurred in Cuba in the summer of 1870. The gentleman came to England suffering from the effects in a state of mental hallucination, perfectly oblivious of having been to Cuba, feverish and wakeful, and in a condition of constant restlessness. After several days, during which all the symptoms continued, two doses of chloral completely changed the whole aspect of the case. He fell into a state of almost constant sleep, but on waking had glimmerings of consciousness and memory; and in short, somewhat to my surprise and that of Dr. Burrows, whose kind assistance I had, the patient eventually recovered.

Chloral, by diminishing the activity of the motor element of the vaso-motor nerves, appears to give the supremacy to the restraining or inhibitory fibres of Remak, and to diminish the calibre of vessels.

In an able address, read by Professor Laycock before the British Medical Association at Newcastle in 1869, it was shown that "no change in the mind or consciousness can or does take place without a coincident change in the brain"; "that the potential energy of the brain is the greatest of all kinds of vital energy, and *requires a larger supply of blood to maintain it*"; "that much more force as motion is used in mental than in bodily activity in the same time"; in other words, the larger the supply of blood the greater the energy of function. And in what diseases do we find the greatest amount of hyperæmia of the brain? Why in cerebritis and mania, in which the physical results are precisely those induced artificially, by M. Claude Bernard, by section of the sympathetic in the neck, and, in a less degree, by opium. And what remedies have we which induce a directly opposite effect on the cerebral circulation? Chloral, as I have said, is one; and the results of the tentatives, in the way of drugs, by physicians whose attention is directed to insanity exclusively, begin to show that on chloral will they mainly depend in that disease. Need I quote a better authority than that of Dr. Clouston, of Carlisle, who recommends to the Medico-Psychological Association the use of chloral, *to reduce the temperature, to lower the pulse, and to induce sleep;* whilst (says he) opium and cannabis have an opposite effect.[*]

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* Brit. Med. Jour., June 11th, 1870.

† Ibid., Dec. 18th, 1869.

‡ The *ulterior* or sedative influence of opium on the sensory nerves is here alluded to.

Conium, when taken in large doses, also acts as a sedative on the motor fibres of the vaso-motor nerves, and produces anæmia of the brain,[†] as does cicuta virosa, the aconitum napellus, and many of the Ranunculaceæ.

On the other hand, if we are satisfied that a sudden interruption of the nutritive supply of blood to the brain be the cause of an epileptic fit, as Kussmaul and Tenner have attempted to prove,[‡] then it would appear that opium or medicines producing directly or indirectly analogous results, should be combined with that which is known to prevent spasm of the glottis, on which the sudden arrest of blood to the brain may depend. I say *directly or indirectly* because I am satisfied that I have often found the addition of digitalis, which stimulates the heart, and induces an efficient contraction of the ventricles, with bromide of potassium, which prevents spasm of the glottis, effective in suspending epilepsy, when the bromide of potassium alone has failed. The oxide of silver, I think, acts precisely in the same way.

M. Du-Bois Reymond attributes hemicrania to spasm of the muscular coats of the arterioles of the brain; and Möllendorf recognises a form of anæmic hemicrania as resulting from irritation of the sympathetic, causing contraction of the minute vessels of the brain. On such theory M. Bornetwick explains the curative action of quinine and caffein, as stimulating the motor fibres of the vaso-motor nerves. In these cases opium is known to be of great use. Möllendorf also describes a form of congestive hemicrania, in which mild galvanism to the sympathetic in the neck is an effective remedy. I suppose the hydrate of chloral would be equally effective.

We have seen that section of the nerves of Remak, which supply the parotid and submaxillary glands, is productive of an increase both in the circulation and secretion of those organs. Stimulation of the motor fibres by the administration of mercury superinduces the same phenomena. The antagonistic result, occasioned by section of the motor fibres, or by stimulation of the sympathetic proper, which go to these glands, is also induced by atropia. This latter alkaloid produces complete dryness of the tongue, roof of the mouth, and soft palate, extending more or less down the pharynx and larynx, rendering the voice husky, and often inducing dry cough and difficulty of deglutition, a parched state of the lips, and, occasionally, dryness of the mucous membranes of the nose and eyes.[*] That atropia localises its action in this way on the salivary glands and on the mucous surfaces of the pharynx and larynx, the epithelial structure of which belongs to the category of glands, is proved by the occurrence of the same phenomena when atropia is injected into the subcutaneous tissue in any part of the body. We have therefore medicinal agents which stimulate or restrain the functions of the salivary glands, just as section or stimulation of the different nerves stimulates or restrains them; and I can imagine no other means of accounting for such medicinal action than its influence on the vaso-motor nerves.

When M. Claude Bernard divided the thoracic portion of the gangliated cord in the horse, he found increased vascularity of the lungs and pericardium, together with serous exudation, were the results. Lobelia and digitalis also induce increased vascularity; but when the sympathetic nerve fibres retain their integrity, it would be unreasonable to expect the blood-statis which generally results from section of those fibres. Chloral, acetate of lead, and according to Mr. Blake,[†] the injection of the salts of soda into the blood, causing contraction of the pulmonary arteries, have the contrary effect.

I have a patient, a lady aged forty, who has occasionally attacks of asthma. Sometimes it assumes the dry form, when she is generally relieved by smoking stramonium until expectoration occurs. Sometimes the disease appears as *humoral asthma* of the old authors, when chloral relieves her quickly. She has once or twice taken chloral in the dry catarrhal attacks, and on each occasion—to use her own expression—has felt as though a tiger had hold of her throat. In the humoral form, if she smokes, the breathing is not relieved, but cóngestive headache follows for a day or two.

According to Schiff, Rosenthal, and others, very feeble electric excitement of the vagus, below the inferior cervical ganglion, increases the force and frequency of the heart's action, probably by acting on the special cardiac accelerator nerve, which, as M. Cyon has shown, emerges from the spinal cord with the third branch of the inferior cervical ganglion; more violent excitement exhausts the irritability of the vagus, and stops the action of the heart.

Small and repeated doses of digitalis also increase the force and frequency of the heart's action. It may be objected that the physiological action of digitalis on the heart presents certain anomalies; and that some authors have ascribed to it an enervating influence, although others regard it as essentially stimulating. Dr. Sanders, of Edinburgh, was one of the first to affirm that, in small doses, digitalis occasions an increased action of the heart and febrile symptoms generally. Orfila endorsed that opinion, and Professor Christison has added the weight of his testimony to the same. In point of fact, it is now generally admitted that digitalis acts as a stimulus to the heart; producing, where deficient power, irregular action, and dyspnœa exist, a more complete ventricular contraction, a firmer and more regular pulse, and relief from impeded respiration.[*] During the stimulating influence, secretion is increased, by virtue of the law which I have quoted, as dependent on the distribution of the ganglionic centres of the sympathetic system to every important organ. Where it is an object to enlarge the capillaries, and diminish the blood-pressure within them—and I have alluded to some cases of epilepsy as examples,—it seems but reasonable to give digitalis.

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[*] British and Foreign Medico-Chirurgical Review, October, 1870.

[†] Old Vegetable Neurotics (Dr. J. Harley), p. 13.

[‡] Kussmaul and Tenner on Convulsions, New Sydenham Society, 1859.

[*] Old Vegetable Neurotics, pp. 203-4.

[†] Edinburg Medical and Surgical Journal, vol. liv., pp. 341-346.

Excitation of the sympathetic trunk alone in the neck (and it may be done in rabbits, in which the vagus and sympathetic run separately) always causes increased blood-pressure, a lowering of temperature, but no alteration in the frequency of the cardiac beats;[†] and such are the effects of aconite. Dr. Wilks has given it in active inflammatory diseases, in acute rheumatism associated with endocarditis, and with satisfactory results. It appears to contract the arterioles, to prevent the accumulation of blood-corpuscles and their filtration through the capillary walls, to lower the temperature, and to abate the symptoms of fever generally. In short, it stimulates the dormant activity of the fibres of Remak, and by so doing diminishes the calibre of the arterioles. It is an interesting fact also that this drug appears to possess some of the attributes of a counter-irritant, for when chewed it produces intense tingling of the lips and tongue, and instances have occurred in which the leaves have blistered the skin. Dr. Fothergill has shown how that digitalis and aconite produce opposite actions.[‡]

I have described how the peculiar innervation of the heart renders that organ capable of regulating its own action, by exerting a reflex action on the widespread vaso-motor nerves of the general circulation.

Fever presents us with a pathological instance of the independent condition of the organs of circulation. With a contracted pupil there is a rise of temperature, the walls of the heart are preternaturally excited, the balance of function is suspended, the motor fibres of the vaso-motor nerves obtain the supremacy, and it is precisely those medicines which depress their function, or which stimulate the fibres of Remak, which are found to be most effectual as remedial agents.

The alkaloid of veratrum, extolled as a remedy in fever by Trousseau and Aran in France, and by Vocher in Germany, has been carefully studied by Dr. Horatio Wood, of Philadelphia, who has determined that it exerts no direct influence on the brain; but that it depresses the functions of the spinal cord and heart, diminishes sensibility, restrains the action of the vaso-motor nerves, renders the respiration slower, and reduces the temperature of the body.[§]

We have the testimony of Dr. J. Harley that belladonna localises its action on the sympathetic nerves; that in moderate doses it induces a tonic and slightly contracted condition of the whole of the circulatory tubes, accompanied by increased force and frequency of the heart's action. "The blood," says he, "is equally distributed, and the circulation in any given part is so *tight* and rapid that it really contains a little less blood than when in a quiescent state, and the tissue is consequently a little paler. But the quantity which passes through it in a given time is generally increased."[*] After the use of moderate doses these effects are maintained throughout. Hence it suggests itself as a remedy in fever, and the more so as, being eliminated by the kidneys, it increases the secretion of urine. According to Dr. Meuriot, it also increases the secretion of bile, and predisposes to perspiration.[†] Mr. Christopher Heath, who has found it useful in fever, states that it reduces vascularity by diminishing the calibre of small vessels. We have seen how it exercises a direct action on the salivary glands.

Chloral, also, as a sedative to the sensory and motor nerves, reacts on the vaso-motor nerves, lowers the temperature of the body, diminishes perspiration, and relieves the symptoms of fever generally, except that, like atropia, it does not allay thirst;[‡] and that is perfectly consistent with its action generally of diminishing secretion. May not the apparent discrepancy in the fact that atropia increases abdominal secretion whilst chloral diminishes it, yet that both are beneficial in fever, be explained by the stimulating influence of atropia on the fibres of Remak and by the sedative action of chloral on the motor fibres of the vaso-motor nerves? If so, we have remedies for fever generally, yet calculated to meet contrary exigencies in the course of that disease. It has been said that chloral was used in some of the French ambulances during the late war as a remedy against pyæmia, and this faculty, I apprehend, can depend only on the fact that, by deadening the influence of the motor fibres of the vaso-motor nerves, it virtually puts the fibres of Remak in a condition to occlude the minute vessels.

(*To be concluded.*)

ON SOME PHYSIOLOGICAL ERRORS CONNECTED WITH CHOLERA.

By William Sedgwick, M.R.C.S., &c.

It has been alleged in my late communication to The Lancet that the radical error in Dr. Johnson's theory of cholera is unquestionably the attempt to prove that defective oxygenation of the blood from the assumed action of an assumed blood-poison is essentially connected with the phenomena of the disease. On the present occasion it is proposed to show the extent to which this error has involved its otherwise distinguished author in numerous and very serious difficulties; so that when, from time to time, his position has become physiologically less and less tenable, the unscientific tendency to substitute assumption for fact has become increasingly more pronounced. It is obviously owing to the difficulties of his position, and to an unmistakable inability to refute the evidence of a long list of trustworthy observers who have been cited in my paper "On the Continuance of the Mammary Secretion when the Urine is suppressed in Choleraic Collapse" (*Brit. Med. Journ.*, Sept. 19th, 1868), that Dr. Johnson has felt it necessary to turn aside from evidence which is fatal to his theory, and to proceed once more to assume that "this alleged immunity of the reproductive functions is a figment of Mr. Sedgwick's vivid imagination," (THE LANCET, January No., 1872.) In the following remarks on some of the evidence which has been lately adduced by Dr. Johnson himself in favor of defective oxygenation of the blood as essentially connected with the phenomena of cholera, it will be seen that no reliance can be placed either on the direct or the indirect support on which this theory has hitherto been supposed to rest.

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[*] Cases illustrative of such influence are given by Dr. B. D. Taylor, of the Bellevue Hospital, New York, in the New York Medical Journal for Nov., 1870; and Dr. Milner Fothergill, in the Hastings Prize Essay for 1870, has shown that digitalis is a member of a group of agents possessing such qualities. See also the British and Foreign Med.-Chir. Review, July, 1871: "Digitalis and Heart Diseases"; by Balthazar W. Foster, M.D., Professor of Medicine in Queen's College, Birmingham.

[†] Gmellin's Pflanzengifte, p. 717.

[‡] Hastings Prize Essay, 1870.

[§] American Journal of Medical Sciences, January, 1870.

[*] Dr. J. Harley: The Old Vegetable Neurotics.

[†] Dr. Meuriot: De la Methode Physiologique a l'Etude de la Belladonna.

[‡] Bulletin General de Therapeutique, Nov. 30th, 1869.

The evidence contained in my paper "On some Analogies of Cholera in which Suppression of Urine is not accompanied by symptoms of Uræmic Poisoning" (*Med. Chir. Trans.*, vol. 51, 1858) has lately proved that excessive gastro-intestinal irritation leads to a central arrest of nutrition with the same condition of collapse as is met with in cases of cholera. This evidence, which has been fully corroborated by that of other observers both abroad and at home, has assisted to establish the fact that suppression of urine associated with arrested formation of urea is the correlative of suppression of the pulmonary function associated with the arrested formation of carbonic acid, and that both of these phenomena are equally referable to the condition of the alimentary canal. Among the corroborative evidence referred to above is an interesting "Case of Gastro-enteritis from local irritants simulating Cholera on two occasions in the same individual," observed by Dr. Murchison (*Trans. Clin. Soc.*, vol. 2, 1869, pp. 178-181), in which "not only was the collapse identical with that of cholera, but as in cholera, the suppression of urine was on both occasions followed by temporary albuminuria." This occurrence of temporary albuminuria after previous diminution or suppression of the urinary secretion has been observed in a very large number of cases of gastro-intestinal irritation attended with collapse simulating the collapse of cholera; and I have succeeded in collecting a large number of trustworthy observations on this subject. It will be sufficient, however, for our present purpose, to cite the observations of M. Abeille, who, in his well-known work on the Urine, published in 1863, has called attention, under the heading of "Albuminuria in Affections of the Digestive Tube," to the frequency of its occurrence in certain cases of diarrhœa, to which he has applied the epithet of "choleriform." In eight out of fifteen cases of this kind which came under his own observation, there was vesical retraction and a very great diminution of the urinary secretion, with temporary albuminuria, which, at the most, lasted only from one to two days. In one of these cases, which M. Abeille relates as a type of the rest, a corporal, previously chilled whilst on duty, *drank a large quantity of cold water*. Symptoms closely resembling cholera soon afterwards supervened, including suppression of urine. "Not more," writes M. Abeille, "than fifteen grammes of *clear urine* were withdrawn by the catheter, which tested by nitric acid and heat, was found to be albuminous. On the following day albumen was in like manner detected in the urine, but it subsequently disappeared." M. Abeille has also recorded the case of a soldier, who died from intestinal perforation (ileo-cæcal), in whom the symptoms closely resembled those of cholera. There was vomiting, coldness, *violet discoloration of the skin*, and scanty urine, which was alkaline and albuminous. As regards the urinary secretion, there is, indeed, throughout the progress of these cases, essentially the same sequence of phenomena as occurs in cholera—namely, temporary diminution or suppression of the urinary secretion; the development of a blue coloring matter in the urine; temporary albuminuria; and, finally, slight and temporary glycosuria, which usually occurs during convalescence, and precedes the re-establishment of the normal secretion. Whilst the order in which the constituents of the serum are drained off in the rice-water flux, as observed by Schmidt, of Dorpat, accords with that which takes place during the action of some purgative medicines, such as elaterium (*Dub. Quart. Journ.*, August, 1866, p. 152.) It would consequently be a physiological error to assume that defective oxygenation of the blood from the localised action of a specific blood-poison is necessarily connected with the phenomena of cholera; since there is a corresponding suppression of urine, followed by temporary albuminuria, in many cases of excessive gastro-intestinal irritation which are unconnected with epidemic cholera, and in which it must be admitted that the *origo mali* is the alimentary canal.

Notwithstanding the unexceptional character of the evidence derived from analogy in favor of the collapse and the resulting condition of the urinary secretion in cases of cholera being essentially the same as in cases of excessive gastro-intestinal irritation, Dr. Johnson, in what can only be regarded as an unfortunate attempt to weaken its influence, has, on the one side, denied that cyanosis occurs in those cases which have been referred to as analogous to cholera; and, on the other side, has asserted that temporary albuminuria is conclusive evidence of blood-poisoning. In a paper on "The Collapse of Cholera compared and contrasted with other Forms of Collapse" (*Med. Times and Gaz.*, May 5th, 1866, p. 465), he has informed us that "whereas in all other forms of collapse there is great *pallor* of the surface in consequence of a defective supply of blood to the minute vessels, in the collapse of cholera the surface is more or less *blue* in consequence of defective respiration and an accumulation of blood in the veins." Although italics have been introduced by the author in the above-cited passage to mark the importance which he has assigned to cyanosis as a diagnostic symptom, yet there can be no hesitation on the part of any clinical observer who has carefully watched such cases in at once admitting that Dr. Johnson has on this occasion committed a grave error in diagnosis, with reference to this supposed clinical discovery, for the distinction which he has here pointed out does not exist. If we examine the previous literature of the subject, together with the cases published since the date of the above-cited paper, it will be found that in many of the cases belonging to the class to which Dr. Johnson has specially referred, including those in which, as he informs us, the blood constituents "are drained away by excessive purging," those in which the effect has been produced by "perforation of the stomach or bowels,"

&c., cyanosis has been referred to as a frequent and a characteristic symptom, when the resulting collapse has been strongly marked. In a case of croton-oil poisoning fatal in four hours, cited by Dr. Taylor (On Poisons, 2nd edit., 1859, p. 568), "the extremities and face were as blue as in the collapsed stage of cholera." In Dr. Headlam Greenhow's case of croton-oil poisoning fatal in ten hours (*Med. Times and Gaz.*, Aug. 11th, 1866, p. 142), the skin was "even more blue than is usual in cases of true cholera." Cyanosis was likewise observed in two cases of poisoning by croton oil which were published more than ten years ago, and which have lately been cited by M. A. Chevallier (*Annales d'Hygiènene Publique*, Avril, 1871, pp. 409, 410). Whilst, on the other side, in a case of poisoning by the inhalation of the dust of croton seeds, observed by Dr. Pereira ("Elements of Materia Medica and Therapeutics," 4th edit., 1855, vol. ii., part 1, pp. 406, 407), in which the irritant effect of the drug was limited to the lungs, and the "bowels were not acted on," no cyanosis is said to have occurred. With regard to cases of perforation, obstruction, and acute inflammation of the alimentary canal, it may be sufficient to state that there was more or less well-marked cyanosis in cases of perforation of the stomach observed by Dr. C.-S. Cliet (*Gaz. Méd. de Paris*, 2 sér., t. xiii., 1845, p. 706), and by M. Salneuve (cited by M. A. Luton, "Recueil des Travaux de la Société Médicale d'Observation de Paris," t. i., 1857–58, pp. 382—386); in a case of perforation of the duodenum observed by M. Petrequin (*Archives Générales de Médecine*, 2 sér., t. xii., 1836, pp. 483, 484); in intestinal perforation by M. Abeille (*op. cit.*, 1863, p. 186); and in some cases of perforation observed by M. Louis, in which, as Dr. Stokes informs us (Cyclopædia of Practical Medicine, "Peritonitis from Perforation of the Serous Membrane," 1834, p. 313), the surface was "of a violet hue." Cyanosis was also observed in a case of rupture of the jejunum, by Mr. Curling (London Hospital Reports, vol. ii., 1865, pp. 287, 288); and in a case of obstruction of the jejunum, by Dr. Barlow (Guy's Hospital Reports, vol. ii., 1844, p. 379); in a case of internal strangulation, by Dr. Delaye, of Toulouse (*L'Union Médicale*, 1855, pp. 467, 468); in a case of strangulation of the ileum, which was almost unavoidably mistaken for cholera, and admitted as such into the Hôtel Dieu, Paris (MM. Fournier and Olivier, *Gaz. Méd. de Paris*, 15 Août, 1868, pp. 476—479); in a corresponding case observed by M. de Wouves, in November, 1867 (Du Choléra, 1868, pp. 216—218); and in a case of acute inflammation of the whole of the gastro-intestinal mucous membrane with peritonitis simulating cholera, observed by M. Champouillon at the Val-de-Grâce Hospital, Paris, in December, 1853 (*Gaz. des Hôpitaux*, 1854, p. 73). Cyanosis has frequently been observed also in rapidly-fatal cases of irritant poisoning by arsenic, corrosive sublimate, the mineral acids, &c., in which the primary effect produced is on the gastro-intestinal mucous membrane.

In addition to the occurrence of cyanosis in a large number of cases of excessive gastro-intestinal irritation, which has been emphatically denied by Dr. Johnson, there has not unfrequently been observed, as I have already stated in my paper on the analogies of cholera, well-marked pulmonary collapse in the same class of cases after death. This was specially noted in Dr. Cotton's case of perforating ulcer of the stomach (*Med. Times and Gaz.*, Jan. 12th, 1856, pp. 38, 39); and in M. Bucquoy's case of triple descending invagination of the small intestine at its superior part, in which the left lung was found completely collapsed, leaving a considerable void in the corresponding pleural cavity, and the right lung was found to be collapsed, though to a less extent on account of adhesions; both lungs in this case being otherwise healthy. The same condition of pulmonary collapse has likewise been observed in rapidly fatal cases of gastro-intestinal irritant poisoning, but the limited space at my disposal will not allow of more than a brief allusion to some of the cases of this description recorded in Casper's "Hand-book of Forensic Medicine," especially to case No. 194, of poisoning by sulphuric acid, in which perforation occurred, and in which the lungs after death were found to be "remarkably anæmic"; and to two other cases, Nos. 197 and 198, also of poisoning by sulphuric acid, in both of which a corresponding condition of the lungs was observed.

On the other hand, it has been long since ascertained that there is a local disappearance of cyanosis during choleraic collapse, when galvanism is applied to a limb, which is independent of any effect produced on the pulmonary circulation. This was well illustrated by Dr. Favell, who was physician to the Cholera Hospital at Sheffield, during the first epidemic of the disease in this country, and who satisfactorily demonstrated that when galvanism was applied, for example, to the right upper limb of a cyanosed cholera patient, the blueness of that part disappeared; and that on galvanising the left upper limb, a correspondingly local effect was produced (THE LANCET, 1832–33.) And, lastly, the fallacy of assuming that the cyanosis in cholera is due to defective oxygenation of the blood may be illustrated by what has been observed in the case of a warm-blooded animal during hybernation, when its condition, as regards both the circulation and the respiration, is reduced below that of a reptile, and yet the diminished supply of oxygen to the system is found to be accompanied by an increased redness of the blood.

The imaginary importance which as we have already seen, has been very hastily and very incorrectly assigned by Dr. Johnson to cyanosis as a diagnostic symptom between cholera and gastro-intestinal affections simulating cholera, could not be expected, apparently even by the author himself, to give any real or permanent support to his theory if defective oxygenation of the blood; and, consequently, it was soon afterwards found to be necessary again to have recourse to fresh assumptions, one of the most noticeable of which is contained in his paper "On some points in the Pathology of Cholera and Apnœa" (*Brit. and For. Med.-Chir. Rev.*, April, 1870), in which the following statement occurs:—"Temporary albuminuria is, to my mind, conclusive evidence of blood-poisoning." The immediate occasion for this *ex cathedrâ* announcement was the publication of Dr. Murchison's case, already alluded to, of gastro-enteritis from local irritants simulating cholera on two occasions in the same individual, in whom suppression of urine was twice followed by temporary albuminuria; and it is obvious that if Dr. Johnson's statement, founded it is said on "conclusive evidence," were correct, the previously assumed existence of a specific blood-poison in cholera would immediately become an incontestable fact. In the apparently unavoidable absence of any information from Dr. Johnson himself respecting the nature and the extent of the "conclusive evidence" referred to above, and in the very unsatisfactory state of our knowledge respecting the character and the effect of blood-poisons in general, it will be difficult, though not perhaps impossible, for us to acquire some information on this subject for ourselves, which may assist us in proving that the application of the phrase "physiologically weak" to Dr. Johnson's theory of cholera is, strictly speaking, more courteous than correct, for the theory has been found to be physiologically unsound, and the extent of this unsoundness becomes more apparent after every fresh effort on the part of the author to retrieve his position. Since the time when, as Dr. Carpenter informs us, Magendie demonstrated that the injection of water into the veins of a dog was sufficient to cause the urine to become quickly albuminous, down to the time when Overbeck made the curious observation that compression of the renal arteries was also followed by temporary albuminuria, physiologists have usually entertained the opinion that temporary albuminuria is not necessarily due to the effect of a previous poison in the blood; and this opinion accords with the fact that a puncture in certain localities at the base of the brain is followed by the appearance of albumen in the urine. Moreover, in the temporary occurrence of albumen in the urine as a result of gastro-intestinal affections allied to cholera, the supposition of the previous existence in the system of a blood-poison is on many occasions precluded by the nature of the case itself; whilst its probable dependence in these cases on a central diminution or arrest of nutrition is strengthened by the observations of Rosenthal and others on albuminuria in inanition, especially when taken in conjunction with Ranke's experiments on himself, and those of Pettenkofer and Voit on dogs, illustrating the remarkable diminution of urea which occurs as a result of mere starvation; and, lastly, by the fact that towards the close of fœtal existence, when the gastric function is still undeveloped, the urine contains albumen, which there is no "conclusive evidence" to show is physiologically dependent on any sad tendency in the mother to poison unconsciously her offspring.

Again, in a lately published communication on the "Action of the Cholera Poison" (*Brit. Med. Jour.*, Aug. 13th, 1870, p. 180), Dr. Johnson, whilst endeavoring to secure some additional support for his theory, refers to "the interesting physiological correlation of the lung, liver, and kidney, as illustrated by their functional inactivity in the fœtus, and their energetic co-operation, immediately after birth, when respiration is established." The inactivity of these organs in the fœtus, and the suppression of their secretion, are said to be "a necessary result of arrested pulmonary circulation and consequent apnœa," in consequence of which "the secretion of bile and urine by the fœtus is even more scanty than that of a patient in cholera collapse, while the abundant formation of those products by the new-born infant is analogous to the restoration of the biliary and renal secretions during the reaction from cholera." As the same argument was brought forward and discussed in a previous number of the same journal (February 17th, 1866), it is evident that considerable importance has been attached to this "interesting physiological correlation," and that it has been much relied on for the purpose of proving that the suppression of urine in cholera is due to defective oxygenation of the blood. Now, as a preliminary step in the inquiry, it would no doubt be useful to prove that the assumed action of the assumed cholera poison is, with regard to its effect on the renal secretion, similar to that which naturally occurs during the pulmonary inaction of fœtal existence. But the argument from analogy which Dr. Johnson has here sought to establish between fœtal existence and the collapse of cholera cannot possibly be maintained, for the simple reason that suppression of urine does not occur in the fœtus. Abernethy, it is said, entertained the opinion that the kidneys did not secrete till after birth; but since Abernethy's time some progress has been made in the study of physiology, and it is now commonly taught and generally believed, on what deserves to be called good authority, that functional inactivity of the lungs is not associated with functional inactivity of the kidneys during fœtal existence. The allantois, it is now well known, receives in the first instance the secretion of the corpora Wolffiana, and at a later period the secretion of the true kidneys; the phenomenon of fœtal glycosuria, in the human subject and in some of the lower animals, has been specially studied by M. Claude Bernard and other distinguished physiologists; distension or the fœtal bladder, as a result of the urethra being imperforate, has been observed by Dr. L. Lehmann, Dr. W. D. Moore, and others who have written on the subject; difficulty in parturition from the enormous amount of this vesical distension consequent on urethral obstruction has been well illustrated in two interesting papers by M. Duparque and Dr. Depaul; and lastly, chemical analysis has shown that, in addition to albumen and some of the usual salts, fœtal urine, especially at the full term of pregnancy, contains a highly nitrogenous principle which is closely allied to urea. It must consequently be admitted that a theory of cholera which depends for support on "the interesting physiological correlation" referred to by Dr. Johnson is physiologically weak; whilst the remarkable inaccuracy of his statement, that the secretion of urine by the fœtus "is even more scanty than that of a patient in cholera collapse," can only serve to strengthen the arguments which have been already urged against this theory in the recently published portion of the present paper.

As the chief importance of theories in disease is derived from the extent of their influence on practice, it has been necessary to notice somewhat more fully than would otherwise be the case some of the leading errors in this particular theory of cholera, in consequence of the attempts which have been made, through the medium of the non-medical press, to popularise the cathartic treatment which has been founded on it, and which has failed to gain the approval of the medical profession. The absence of any allusion in the late publications on the Pathology and Treatment of Cholera, by Sir Thomas Watson and Dr. Johnson, to the results of this treatment at any of the metropolitan hospitals, cannot fail to be regarded as a very significant fact. For, during the last epidemic of the disease in 1866, all the cholera patients admitted into King's College Hospital were under the immediate supervision of Dr. Johnson himself, and it is well known that the statistics of treatment on this occasion, showing a mortality of about 62 per cent. are too unfavorable for quotation. In the University College Hospital, during the preceding epidemic of cholera in 1854, eight of the patients were treated with castor oil; but as only one survived, the treatment in consequence was changed. And the committee appointed during this epidemic by the Medical Council of the General Board of Health, to consider and report upon the treatment of cholera by castor oil, ascertained "that in eighty-nine cases of cholera treated by fourteen different practitioners with castor oil, on the plan reommended by Dr. Johnson, sixty-eight were fatal, recovery having occurred in only fifteen cases, while the six remaining cases were still under treatment." (*Med. Times and Gaz.*, Sept. 1854). Similar, if not even more disastrous, results have attended this practice in India; and it would perhaps be impossible to cite a more painful illustration of the failure of the cathartic treatment of cholera than that which is derived from the experience of Dr. Macnamara ("A Treatise on Asiatic Cholera," pp. 460, 461, 1870), who "was acting as house-physician to King's College Hospital in 1854, when (during the decline of the epidemic) Dr. G. Johnson was treating his cholera patients on eliminative principles." Having sailed for India "full of confidence and hope in castor oil," Dr. Macnamara was left, he informs us, in the following year (1855) in sole charge of a field hospital at Bhaugulpore during a severe outbreak of cholera. There he "went boldly to work with castor oil (both among the Europeans and natives under his care); but it absolutely and completely failed, and the mortality from the disease was fearful." Notwithstanding this unfavorable result, he again tried it on several occasions, but subsequently the "castor oil method" was abandoned as "worthless." Since therefore the cathartic treatment, which has been founded on this theory, has signally failed, it can no longer be of any avail, in the struggle to support the theory itself, to act the part of a master of fence, or to be prematurely praised as its "triumphant advocate"; for the victory which has been claimed was virtually lost when the showy weapons relied on for defence had been proved to be unworthy of trust, and when the skill which might have been turned to good account had been wasted on a series of subtle feints, which may perhaps have delayed, but which could not ultimately avert, defeat.

Park-place, Upper Baker-street, Oct. 1871.

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ON VENEREAL DISEASES.

Being an Abstract of Clinical Lectures delivered in University College Hospital,

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No. II.

EARLY SYPHILIS.

Before showing to the class examples of syphilitic eruptions, the lecturer sketched an outline of the ordinary course of the disease, omitting any description of chancres, which will be demonstrated with other venereal sores on another occasion. After contagion, if the vehicle of the virus be an irritating secretion, such as the matter of a chancre, irritation is at once set up at the point of contagion, and a suppurating sore is produced of more or less active progress; or, if the vehicle is not irritant, no change is immediately apparent at the point of entry. The abrasion or puncture heals and disappears during the incubation stage. Hence either no change whatever is apparent during the incubation, or the interval is occupied by local processes not necessarily connected with the disease. This period of quiescence varies within certain limits; but in thirty-nine experimental cases of inoculation, twenty-five days was the most common length. When incubation ends, changes distinctive of the disease commence; the tissue round the point of entry grows hard and elevated, the simple suppurating sore becomes a hard-based indolent one, or, should all local irritation have already subsided, a reddish-brown papule develops, which sometimes ulcerates, but also often remains quite dry and covered by a thin scab. After induration has begun at the point of entry—usually about eleven days afterwards,—the neighboring groups of lymphatic glands undergo general enlargement. While acquiring this increased size, slight tenderness may attract the patient's attention to what is going on; commonly, he is quite unaware of any change in his lymphatic glands. The next phenomenon is the general eruption. When four or five weeks more have elapsed, during which the patient either enjoys apparently good health, or, less frequently, is conscious of lassitude, inappetence, and even headache or febrile disturbance, an eruption, usually of a macular kind, appears suddenly on the trunk, spreading in severe cases over the whole body, head, and limbs. After changing hue from red to brown, this rash fades in two or three weeks; while departing, it is replaced by a less general eruption of scaling papulæ, or syphilitic psoriasis. On the mucous membrane of the fauces simple red congestion is common while the roseola is present; with the papulæ, small white elevated patches or erosions occupy the fauces. Emaciation and loss of strength, if not already present, may now set in, but not invariably, for the patient often retains good bodily vigor to the end. In the course of two or three months, sometimes in less, the papulæ and erosions disappear, and the patient thinks himself recovered. But usually, at intervals during the next two years, fresh scaling papules are developed; no longer, however, widely scattered over the body, but limited to isolated groups, besetting certain favorite localities—such as the limbs, the root of the neck, the nucha, and forehead along the scalp. During this period the margins of the outlets of the body, in women especially the vulva, are liable to be the seat of elevated and often fissured broad papulæ, which, owing to the naturally vascular structure of those parts, and to their being frequently moistened, grow bright-red, and secrete a thin, very contagious discharge. In severe cases, iritis, periosteal pains, or ulcers of the skin often appear at this period of the disease. When a year and a half or two years have elapsed, all symptoms usually cease: the poison is extinct, and the patient remains through life free from further trouble. One single advantage has been acquired—namely, protection against further contagion; for in syphilis, as in other eruptive fevers, a second contagion is extremely rare.

Were these the only consequences of syphilis, would be a trifling disorder; but in a certain number of cases the disease keeps hold of the patient, and, attacking one tissue after another, gradually exhausts his strength. Moreover, its activity may cease for years, and then, probably through some change in the patient's bodily system, be roused into new vitality, may attack some organ essential to life, or be transmitted to his children; by which action, if it do not destroy life directly, it so deteriorates the structure that a passing inflammation annihilates the organ, and death ensues. Compared with the number infected, such cases are happily rare: how rare, the following may help us to comprehend. Boeck, the Norwegian surgeon, has given in his "Recherches" a large statistic of persons whose cases, untreated by mercury or other specific, allow us to observe the natural course of the disease. In 3560 cases, 2181 had cutaneous affections; 601 had affections of the fauces; 50 had iritis; 33 had affections of the bones, and 16 had gummy internal tumours. Though probably the proportion of severe to mild cases is not everywhere the same, this return shows that only a small proportion of those attacked are subject to the damaging sequelæ of syphilis. Yet so common, so almost universal, is syphilis, that the actual amount of permanent mischief and mortality it causes is very great.

The rashes of the skin in syphilis, or syphilides, have been named as they resemble the several idiopathic eruptions. But they are distinguished from their prototypes by many characters common to them all. They are bad representatives of their class—that is, for instance, the pustular forms do not produce good pustules, nor do the scaling forms desquamate freely. Then almost all syphilitic rashes begin in a solid elevation. Irritation and smarting are rare even in those forms which most resemble the itching eruptions. The favorite localities of syphilitic rashes are not those of the corresponding ordinary rashes. For example, idiopathic psoriasis selects the outer aspects of the limbs and rougher parts of the skin; scaling syphilides prefer the delicate skin of the root of the neck, and the flexures of the joints, and leave the elbows and kneecaps free. The several varieties of syphilides are present together; hence are called polymorphic. The color of syphilides is less distinctive than is usually supposed. Their most characteristic hue is that of raw lean ham; but in early rashes it is often rosy, fading to brown or coppery as the eruption dies away. Easily dispersed by proper treatment, syphilides almost as easily recur again and again, if treatment is discontinued before the disease becomes extinct. Specimens of nearly all the eruptions on the skin were then brought before the class. Roseola in its two varieties of large and small spots, appearing while the sore and glands were still indurated; of the scattered papular syphilides were shown, the variety of minute papules widely spread over the back and loins, forming S. lichen; that of papules as large as a lentil, S. lenticularis; papules desquamating by a loose silvery border instead of by closely-set imbricating scales (the desquamation of idiopathic psoriasis), called S. psoriasis if scattered, and S. lepra if arranged in groups of circular or serpentine shape. The change of dry scaling papules into mucous patches was also demonstrated; some completely dry, some dry at one part and moist at another, and some secreting a copious thin pus. Though not usual situations in fat persons the umbilicus, the skin beneath a pendulous mamma, and even that of the back of the neck if chafed by a dirty chignon, are moist enough to produce mucous patches. Attention was also called to examples of vesicular and pustular syphilides, the chief characteristic of which is the solid papule on which the vesicle forms; rupia and pemphigus being perhaps exceptions, though with the former there are generally present solid papules melting away in ulceration.

Dismissing the patients, the lecturer then made a few remarks on the treatment of early syphilis, in which he said the following points must be borne in mind. "First, as the disease does not show itself until the incubation has passed, it is impossible to prevent the disease by destroying the sore. At one time Ricord maintained that this could be done, on the score that constitutional disease never followed a sore destroyed within five days after contagion. This was when the incubation of syphilis was not recognised; hence his error. Ricord was working with local sores—those that appeared within five days after contagion. Few hold this belief now. Next, it must not be forgotten that in most cases, not in all, the patient would get well ultimately, though less speedily, if no medicine were given. Thirdly, that any habit or state of the system which lowers the patient's vigor increases the hold of the poison, and the liability to severe forms of the disease. Lastly, that at present we cannot in any particular case foretell with certainty whether it will be among the few severe, or among the many slight, ones. For this reason no case of syphilis should be allowed to run its course uncontrolled. Moreover, by persevering treatment, the poison may be rendered inert in every one, and its dangerous effects prevented. Acting on these principles, we have to preserve the bodily vigor, to control the poison, and to dissipate the several affections of the disease as they arise. For this are needed good food, abstinence from alcoholic excess (the less taken the better), abstinence from violent exercise (such as training for athletic sports, rowing matches, late dancing parties, &c.) Moderate exercise is advantageous, and the patient should be urged to follow his ordinary avocations that he may not be low-spirited about his disorder.

"For specific treatment, we come to the question of employing mercury. Now, without detaining you with the arguments for or against the use of mercury, the following effects, it is agreed, result from giving mercury in early syphilis. By it induration is dispersed speedily from the point of entry and from the lymphatic glands; the arrival is delayed and the departure hastened of the roseolar, papular, and other rashes; and a few doses dissipate the malaise that precedes the eruption in a most remarkable manner. The drug need never cause permanent injury, even when it cannot be tolerated, as sometimes happens. By watching the patient carefully, by giving him small quantities, and by warning him not to take the drug unknown to his doctor, no results more injurious than a passing inflammation of the salivary apparatus or an attack of diarrhœa need ever occur, and even these most rarely. Several of you have seen a very interesting case of syphilitic inoculation at the upper lip now attending among the out-patients, where an excavated ulcer with thickly-spread roseola were evident when he applied for treatment. Four grains of grey powder were taken twice daily for four days; then slight signs of salivation appeared, and his mercury was at once stopped. Yet considerable swelling of the gums and salivary discharge set in; and, what is worse, the dry tuberculated eruption began to melt away by ulceration, and the sore on the lip began to extend across the mouth. The effect of the mercury subsided in a few days, and the patient is now making good progress under iodide of potash. In this case, nevertheless, the ordinary indications for mercury were present—a dry, rapidly extending rash at an early stage of the disease, and the patient in tolerable health. To ensure the proper effect of mercury, give the drug, at first, but once daily; in four or five days, if this amount is insufficient, give a second dose. The quantity sufficient in most cases is five grains of blue-pill in male adults, and two grains in females and lads, every twenty-four hours; of more active forms much less—thus, one grain and three-quarters of a grain of the green iodide, four grains and three grains of grey powder, or one-third of a grain and one-sixth of a grain of perchloride. The influence of the drug is best known by the behavior of the syphilide: if the color fade quickly to dull-brown, if the papules soon sink to the level of the skin, or the induration of the sore passes away, enough mercury is being absorbed, even if the gums do not swell. Most people require no other effect than this; but in a very few nothing short of violent salivation affects the disease. For example, two years ago a patient was at the Lock Hospital with tubercular eruption on the nose, &c. She had been attending there longer than I—ten or twelve years I think,—gaining only transitory benefit from various combinations of mercury, iodide of potash, and sarsaparilla. On one occasion I ordered her to rub in ten grains of mercurial ointment every night; it was lucky I ordered so small a quantity, for, having already often rubbed in ointment, she preferred to swallow the dose by way of variety. As closely as we could ascertain, she took about two drachms of ointment, and was for a fortnight tortured with salivation, losing two of her teeth. But her syphilide vanished, and as the mercuric poisoning wore off she regained her good looks, and has enjoyed good health ever since. She now and then takes a few doses of the iodide of potash, and when applying at the hospital for the medicine we are able to record her condition. This is not the only case where I have seen an inadvertent salivation beneficial. From feeble persons with a pustular syphilide, mercury must be withheld until a more robust condition is attained through tonics and iodide of potash. Then, if the syphilide is still present, mercury may be given with advantage, if precautions be taken, even to scrofulous patients or persons with phthisical disease. Mercury acts with equal efficacy on syphilis when introduced through the skin as by the stomach, and though more troublesome, it may be resorted to in those whose alimentary canal is greatly disturbed by the opium necessary to prevent purgation, or when that passage does not absorb the mercury. The mercurial vapor-bath may be used every night until the requisite effect is produced, then thrice weekly at bedtime. A scruple of calomel is volatilised in an atmosphere of steam in which the patient is enveloped, as you see going on in that corner. After the bath the patient should lie an hour or two in a blanket till the perspiration has subsided. In the morning he may, without detriment to his treatment, take his usual sponge or soap bath. This indulgence is by many forbidden; but I find that the mercury is absorbed as effectually, and the clean skin is a great comfort to the patient. If a scruple produces no effect, the quantity volatilised may be increased to a drachm, or even two drachms. If inunction is preferred—and it requires no apparatus,—an ounce of mercurial ointment should be divided into twenty-four packets, and one packet rubbed steadily for about ten minutes into the flank or inside of the thighs every night, changing the locality of the inunction frequently. Hospital patients often rub the ointment into the patches of eruption, and I think sometimes with good effect. Don't debar the patient from his morning tub here also, nor need he be confined to the house or from business. But he should be carefully watched, for the vapor-bath and inunction may cause severe salivation if continued too long. A preparation better known abroad than in this country is very useful in extremely obstinate cases of scaling syphilide—Zittmann's stronger decoction; the one holding mercury in solution. It is made as follows:—Macerate twelve ounces of sarsaparilla for twenty-four hours in three gallons of cold water, then suspend in the water a linen bag containing six drachms of alum, six drachms of white sugar, four drachms of calomel, and one drachm of prepared cinnibar. Boil the whole continuously, adding gradually four more gallons of water, and evaporate to two gallons. Remove the bag, and add to the decoction four drachms of anise seed, as much fennel seed, and an ounce and a half of senna and of liquorice. Then press and strain. From one to two quarts of this decoction should be drunk in twenty-four hours. The course lasts six weeks, and includes a regimen of confinement to the house, twelve hours per day in bed, and a vapor-bath for ten minutes on going to bed every night to excite perspiration. The diet should consist of two or at most three light meals per diem, without any alcohol. This somewhat tedious method is not adapted for osseous affections, or deep ulcers of the skin, being too debilitating in its effect."

NOTES OF A

CASE OF PHTHISIS AB HÆMOPTOE (?)

WITH REMARKS.

By J. B. Bradbury, M.D., M.R.C.P.,

Physician to Addenbrooke's Hospital, Cambridge.

Every practical physician will acknowledge that it is of great importance that the question should be settled, whether blood-spitting is ever a cause of phthisis or not. It is to be hoped that those who have opportunities of watching cases bearing upon this important subject will avail themselves of them by carefully recording notes of the cases which come under their observation, and publishing them, with a view of arriving at the precise relation between hæmoptysis and phthisis.

The following case seems to me important in this respect, and consequently I offer no apology for bringing the following notes of it before the profession.

John G——, aged thirty-one, single, by occupation a porter, was admitted under my care April 11th, 1871. No family history of phthisis. Patient acknowledged that he had been intemperate. He had never had syphilis, but he had an attack of gonorrhœa about five years ago. Always had good health, with the exception just mentioned, till August 1st, 1870, when he began to cough and raise blood. He about this time, on several occasions, brought up about a quarter of a pint of blood at once. Soon after this he commenced to lose flesh, but his appetite was pretty good. He has never had epistaxis. At the beginning of January last he had another attack of hæmoptysis, for which he was treated by one of my colleagues, and which left no trace of pulmonary disease behind it except some slight consolidation of the upper lobe of the left lung, as was evidenced by bronchial breathing over the spine of the left scapula, and very slight dulness on percussion over the same region. On March 28th he began to spit blood again, and the hæmoptysis was repeated daily to the time of his admission, varying in quantity from several ounces to a few streaks. At this time he had no pain in his chest, but complained of shortness of breath. Although he said he had wasted, he still appeared to be a very muscular man. Chest remarkably well formed. There was no flattening under the clavicles, and no difference in the expansion of the two sides of the chest during inspiration. There was feeble breathing on the right side of the chest anteriorly, but no other abnormal sound could be heard there on auscultation. The breathing was rather jerky on the left side, but this was probably due to nervousness. The percussion note was slightly less resonant under the right than the left clavicle. Medium-sized crepitation and rhonchus were heard over the spine of the scapula on the right side, and over this area there was also slight dulness on percussion. There was no cardiac disease.

On the evening of his admission (April 11th) the temperature was 102·2° F.; on the following morning 102°. He was ordered beef-tea and milk for diet, to be given as cold as possible, and the following mixture: dilute sulphuric acid, fifteen minims; tincture of hyoscyamus, thirty minims; sulphate of magnesia, ten grains; peppermint-water, one ounce; to be taken three times daily.

On April 14th he again coughed up 4 oz. of florid blood; the cough became very troublesome. A little medium-sized crepitation was now heard under the right clavicle, and a very little rhonchus and crepitation over the spine of scapula on the same side. Instead of the sulphuric acid I prescribed the following pills to be taken every four hours: gallic acid, ten grains; of glycerine a quantity sufficient to make two pills.

On the 15th the hæmoptysis was repeated to the extent of a quarter of a pint. He attributed the expectoration of blood to the troublesome cough. Crepitation and feeble breathing heard over the whole of the right side of the chest anteriorly; voice sounds rather more distinct on right than left side. Complained of flatulence. In addition to the pills he was ordered to take fifteen minims of the liquor of hydrochlorate of morphia every six hours, with the view of relieving the harassing cough. He was also recommended to swallow ice; and to drink soda-water to relieve the flatulence.

On April 16th, between 1 and 2 A.M., he coughed up 2 oz. more of florid blood. Cough less troublesome; physical signs unaltered; bowels regular. Instead of gallic acid and morphia, the following mixture was prescribed:—Acetate of lead, two grains; dilute acetic acid, fifteen minims; tincture of opium, five minims; water to one ounce: six times a day.

On the following day the patient said he had slept better. He had only brought up a few streaks of blood, with frothy expectoration not unlike that of acute bronchitis. The rhonchus and crepitation were not so distinct over the spine of the scapula on the right side. Physical signs in front remained the same.

April 18th.—Cough prevented him from sleeping. Expectorated three small clots of blood, not a teaspoonful in all. A linctus to be used when the cough is urgent. He wished to have mutton-broth instead of beef-tea.

19th.—Slept well; cough less troublesome; expectoration still slightly blood-tinged; pulse feeble; bowels relieved daily.

20th.—Slight eruption of acne on the nose; tongue coated; crepitation on the right side anteriorly more marked; expectoration very slightly tinged with blood. Ordered, at his own request, another pint of milk instead of mutton-broth.

From the time of his admission (April 11th) to the 20th of April the evening temperature averaged 102° F., the morning being about 1° lower. From the 20th to the 30th, with two or three exceptions, the temperature gradually declined; and on the latter day the temperature was 0·8° F. lower than normal.

On April 25th a mixture containing twenty minims of dilute sulphuric acid, five minims of tincture of opium, and one ounce of water, to be taken three times a day, was substituted for the lead mixture. All medicine was discontinued on April 29th.

The following note was made on May 1st:—Still expectorates at times a small clot of blood. Expectoration very little in quantity, and not muco-purulent. The râles over the front of the chest on the right side are still audible, and the percussion note is slightly less resonant. Breathing on this side is feeble. There is decided dulness over the spine of the scapula on this side, and also medium-sized crepitation above the spine of the scapula. Nothing abnormal can be heard on the left side except a little tubular breathing over the spine of the left scapula. Tongue clean; eats a slice of meat at dinner; cough rather troublesome; no complaint of dyspnœa.

From May 1st to May 3rd there was a gradual rise of temperature. On May 3rd and 4th there was hæmoptysis to the extent of about 1 oz. each day. The patient was sick on the 3rd, and looked very pale; the tongue became coated, appetite bad, and the cough was very troublesome; physical signs unaltered. He was again ordered the lead mixture and morphia draught.

May 10th.—The temperature was 99° in the morning, and 99·2° in the evening. Expectoration still continued to be slightly streaked with blood. Physical signs on the right side remained the same. Medium-sized crepitation was heard for the first time on the left side in front; heard all over this side anteriorly, but most markedly a little above and to the left of the nipple. Omitted the lead mixture, and substituted for it a sulphuric-acid mixture, similar to that ordered on April 25th.

17th.—Patient has continued to improve since the last note, although for the last four days the temperature has been rising. For the last five or six days the expectoration has been free from blood. No crepitation could now be heard over the left side of the chest anteriorly, and only a few moist râles on the right side in front. Posteriorly no crepitation was audible. The patient complained of slight sore-throat, probably due to cold north-east winds which prevailed about this time.

From May 18th to May 27th there was a recurrence of the bleeding; the patient brought up blood daily in quantities varying from 16 oz. to ½ oz. These attacks of pulmonary hæmorrhage were attended with considerable elevation of temperature, and an aggravation of the pulmonary symptoms. The lead mixture and morphia draught at bedtime were again prescribed.

24th.—There was much less movement of the chest on the right side than on the left; and a patch of recent lobular pneumonia could be detected on the left side, at a point midway between the clavicle and the nipple. The breathing at both bases behind was free from crepitation, but distinct on the right side. Instead of the lead mixture and morphia draught, I now prescribed ten grains of compound ipecacuanha powder, to be taken every night; also twenty minims of dilute sulphuric acid and ten grains of gallic acid in an ounce of water, to be taken every six hours.

25th.—Passed the best night he has done for a week. Cough has been less troublesome, and the quantity of blood expectorated has been slightly less.

30th.—For the last two days the expectoration has been only slightly streaked with blood. He has been very sick, and vomited on two successive evenings after taking the Dover's power; this sickness probably checked the hæmoptysis. There was now decided dulness over the right side of the chest, and also over a small patch of lobular pneumonia in the upper lobe of the left lung.

31st.—The compound ipecacuanha powder and mixture containing gallic and sulphuric acids were suspended, and the following substituted: five grains of alum and twenty minims of dilute sulphuric acid in an ounce of water, to be taken thrice daily.

June 2nd.—The evening temperature on this day was 103° F. Patient's cough was exceedingly troublesome. To relieve the cough I prescribed the following mixture: one grain of acetate of morphia, one ounce of spirits of chloroform, and two ounces of acacia mixture; a teaspoonful to be taken when the cough was urgent.

3rd.—No improvement, so a blister was applied over the patch of lobular pneumonia on the left side. The application of the blister was followed by a diminution of temperature for one day, but on the evening of the 5th the temperature was again 103° F. Since the last attack of hæmoptysis the pulse has also increased in frequency, being on the 3rd and 4th 120 per minute.

8th.—Patient felt much weaker, and perspired very much; had also lost his appetite. He was ordered a mixture containing fifteen minims of dilute hydrochloric acid, to be taken thrice daily before meals.

15th.—No improvement. A blister was applied to the right side of the chest in front, and he was allowed two ounces of port wine.

20th.—Rhonchus and crepitation heard all over right and left sides in front; crepitation heard also over the whole of right side behind. Left base behind free from crepitation. Above the spine of the scapula on this side the breathing was much drier than below that—in fact, it was almost cavernous. The right side was the duller of the two, but the left was less resonant than when the last note was made. Cough and sweating very troublesome. Temperature 102·2° F.; pulse 120. Ordered a pill composed of a grain of sulphate of quinine, a quarter of a grain of powdered opium, and half a grain of powdered digitalis, to be taken three times a day.

21st.—The pills reduced the temperature and pulse very considerably. They were only continued for a day, as the patient said they disagreed with him.

25th.—A cavity could now be detected in the right apex. The patient had lapsed into a typhoid condition; his tongue was covered with a dry brown fur; he was also troubled with diarrhœa.

30th.—Is becoming gradually weaker. The temperature for the last few days has resembled that of typhoid fever in the third week.

July 2nd.—Died at 9.30 A.M.

Autopsy, twenty-seven hours after death.—The whole of the upper lobe of the left lung was infiltrated with so-called yellow or yellowish-white tubercles. At two points on the posterior surface of this lobe, just underneath the pleura, were two small cavities, evidently formed from the softening of cheesy matter. Almost at the apex behind was a small cheesy nodule, which had not softened. The base of this lung was in the first stage of pneumonia. Scattered throughout the lower lobe were a number of yellowish-white tubercles, of about the size of millet-seeds. At some points these bodies were aggregated together. They were chiefly situated immediately beneath the pleura. The right lung was universally adherent in front, and also to the diaphragm. The lung was considerably increased in bulk, owing to its being throughout infiltrated with yellowish-white tubercles. The whole of the lung seemed marbled by iron-grey induration. A cavity the size of a Tangierine orange was found in the apex of this lung; and another about the size of an egg in the upper part of the lower lobe, on its outer aspect. A number of bronchial tubes, filled with muco-purulent fluid, were visible on section of the lung. No true grey tubercles were found in either lung. A few atheromatous patches were found at the commencement of the aorta. The valves were healthy. The liver was in the first stage of cirrhosis.

Remarks.—There is strong reason to believe that in this case no pulmonary disease existed before the first attack of hæmoptysis, beyond a morbid delicacy of the walls of the bloodvessels of the lungs, probably induced by the patient's intemperate habits. The man's health, he several times assured me, was perfect up to the time of the first attack of bleeding. I have been unable to ascertain what was the state of his chest during the period he was in the hospital under the care of another physician; but at the time of his discharge the only evidence of pulmonary mischief consisted in some slight consolidation of a very limited portion of the upper lobe of the left lung, probably corresponding to the small cavities found after death, which had resulted from the softening of cheesy nodules. These nodules were in all probability the results of changes set up by the hæmoptysis. The further progress of the case is interesting. Every subsequent attack of hæmoptysis was followed by inflammatory changes in the lungs, especially the right. There was also a decided tendency for the first series of these changes to terminate in resolution, but another attack of bleeding unfortunately prevented this. There was a peculiarity in this case which is worth noticing—viz., the rise of temperature for a day or two before each series of attacks of hæmorrhage occurred. If this temperature elevation should be found to occur in other cases it will be an important guide to treatment, for as soon as the increase is noticed remedies should be administered with a view of preventing the supervention of an attack of bleeding. There are a few other points connected with the treatment of hæmopytysis in which this case is instructive. Acetate of lead checked the first attack of hæmoptysis the man had after coming under my care, when gallic acid, which has been recently so highly extolled, failed. In my experience, acetate of lead is the best remedy in pulmonary hæmorrhage, except in those cases in which the hæmorrhage is very persistent, when an emetic of ipecacuanha, as recommended by Trousseau, has no equal. The Dover's powder in the above case produced vomiting, and thereby checked the hæmorrhage, which had proved rebellious to acetate of lead and a mixture containing gallic and sulphuric acids. From the record of the case there can be no doubt of the advisability of checking the bleeding at once in cases of pulmonary hæmorrhage. I am inclined to doubt whether this symptom is ever a salutary one in the progress of phthisis. In a great many instances no ill effects may arise from an attack of bleeding, but in others it may have a most injurious influence; and in my opinion the temperature of a patient should be carefully observed for some days after an attack of hæmoptysis, with a view of ascertaining whether such attack has been followed by inflammatory action in the lungs or not.

Cambridge, September, 1871.

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ON EXTRACTION OF CATARACT BY A PERIPHERAL SECTION OF THE IRIS WITHOUT INJURING THE PUPIL.

By Charles Bell Taylor, M.D., F.R.C.S.E.,

Surgeon to the Nottingham and Midland Eye Infirmary.

I have on former occasions, in the pages of The Lancet, the "Ophthalmic Hospital Reports" and the "Ophthalmic Review," called the attention of the profession to a mode of extracting, in cases or cataract, which I have been led to prefer to all others, and which, so far as facility of performance and percentage of recoveries are concerned, has appeared to me to leave almost nothing to be desired.

I now wish to point out certain advantages I have derived from a further elaboration of the method. In the operation I have already described, the wound is subconjunctival, prolapse of the iris is prevented by excision of a small portion of that membrane, and the curvature of the line of incision is too slight to permit of reflexion of the flap. There is no tendency for the wound to gape; no gushing of the aqueous, and consequent displacement of parts; manipulation of the eyeball is both safe and easy. Every portion of the cataract may therefore be removed at the time of the operation, and a clear black pupil obtained, through which the patient may count fingers before the eye is bound up. Moreover, although it is not possible that all the cases should be successful, still entire loss of the eyeball from shrinking or suppuration does not occur more than four or five times in a hundred operations. What more can be desired? To this I reply that every modern improved method of extraction, whether it be Graefe's, Mooren's, Pagenstecher's, or the one I have described, involves the sacrifice of a portion of the iris extending from the periphery to the pupillary margin, and that the one thing more to be desired is, that with the same safety and facility of performance we should be able to secure for our patients that highest indication of success, a central and movable pupil.

Prolapse of the iris has always been the bugbear of extraction wherever iridectomy has not formed a part of that operation; and yet the extreme beauty and superexcellence of the results, when no accident has occurred and when all has gone well, after one of the old flap operations, is such that we constantly find ophthalmic surgeons abandoning the modern methods to revert to the old and necessarily much more dangerous flap extraction. When I was last in Utrecht I found that Professor Snellen had, in spite of the good results to be obtained by Von Graefe's and other modern methods, returned to the old flap operation; and the same is true of many other continental surgeons as well as of some of the most eminent in this country. Those, for instance, who have had the good fortune to study Mr. Bader's practice at Guy's Hospital must have noticed how frequently and with what excellent results he resorts to the old flap operation. I have myself on several occasions, actuated by a similar desire to save the iris and attain the maximum of success, extracted by my own method without excising any portion of the iris, merely enlarging the wound in a lateral direction sufficiently to permit of the exit of the lens. Dr. Macnamara, of the Calcutta Hospital, has also endeavored to effect the same object by the use of the spoon. In my own experience, however, prolapse has occurred from time to time, and whenever I have left the iris untouched I could by no means say, as I was in the constant habit of doing when every step of my process had been fully carried out, that my anxiety had ceased with the operation, and that scarcely any subsequent treatment was necessary.

Latterly I have been endeavoring to ascertain whether it were not possible to combine the prevention of prolapse and the other advantage attendant upon an associated iridectomy with the central and movable pupil so much to be desired in all operations of extraction for cataract, and having attained that desirable consummation, I am anxious to lay before the profession the steps of the process, which are briefly as follows.

As a rule I administer a mild aperient the day before the operation, and extract the following morning before breakfast. Chloroform is much more pleasantly given under these circumstances. The risk of vomiting is to a great extent obviated, and that of all other accidents dependent upon anæsthesia diminished.

The instruments I employ are a pair of sharp forceps that pierce the sclerotic; a very light speculum (a modification of Von Graefe's); and two knives, a line in width, and bent at an angle similar to the ordinary iridectomy knife—one with a sharp point, the other with a blunt or bulbous extremity.

FIG. 1.

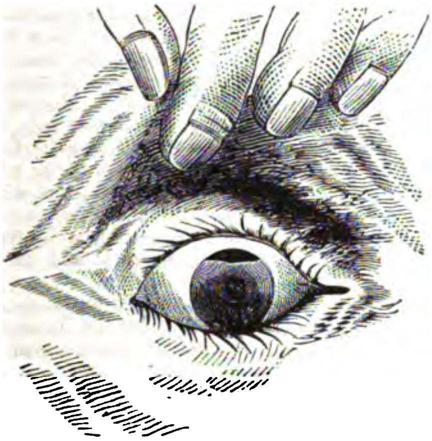


FIG. 2.

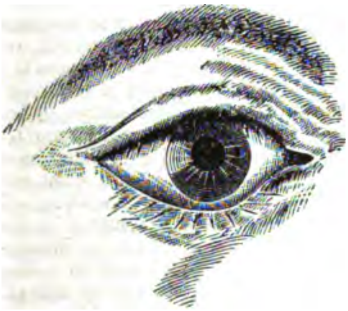
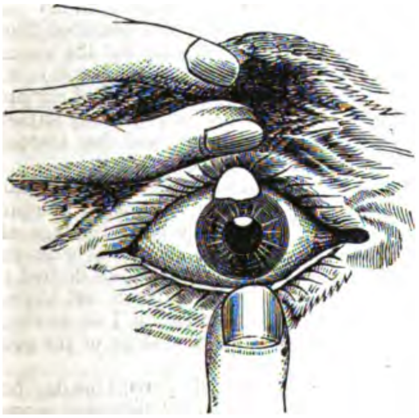


FIG. 3.



Having separated the lids with the speculum, the eye should be gently turned downwards with a pair of ordinary forceps in the operator's right hand. Having got the globe into a favorable position, it should be fixed by the sharp forceps at about the junction of the upper with the middle third of the cornea; the pointed knife is then entered in the cornea-sclerotic junction one or two lines from the forceps at the summit of the cornea, pushed well into the anterior chamber, and then, with a gentle sawing motion, carried along the summit until about one-third of the cornea has been incised. The capsule is then carefully divided with Von Graefe's cystitome, having been previously rendered tense, and the eyeball fixed with a pair of ordinary forceps. (It is better to open the capsule at this stage, because bleeding from the wounded iris—and conjunctiva also—at a later period is apt to fill the chamber and render this part of the operation obscure and difficult.) The upper segment of the iris is then seized, and a small piece of the periphery only excised, the pupillary margin and portion of iris attached to it being left untouched and free in the anterior chamber; the lens is then extruded through the gap in the ordinary way, gliding behind the pupil, so that there is no stretching of the sphincter.

In this way I believe that I have secured all the advantages, in the way of safety and certainty, of an associated iridectomy (which I have already detailed), and at the same time attained that grand desideratum—a central and movable pupil.

The woodcut No. 1 shows the slight gap in the iris when the lid is forcibly raised in order to expose it; No. 2, the same eye with the lid in its ordinary position; while No. 3 exhibits the lens gliding behind the untouched pupil in the third stage of the operation.

The drawings Nos. 1 and 2 have been taken from enlarged photographs of patients operated on by this method; and, although they took like ideal sketches, I must say that, so far from exaggerating they really fail to convey how thoroughly the end is attained, how very slight is the defect in the iris even when the lid is raised in order to expose it, or how natural is the appearance of the eye under all ordinary circumstances; in fact, without very careful scrutiny, no one would guess that the patient had been operated upon at all.

The appearance is quite equal to excellent flap results, and, so far as vision is concerned, I expect much better averages then with that time-honored operation, since the position of the wound is such as to obviate all risk of interference with the natural curves of the cornea and consequent astigmatism, which so frequently tended to depreciate the results, and disappoint both patient and operator, in Daviell's method.

Nottingham. October, 1871.

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PSYCHIC FORCE.

By SAMUEL WILKS, M.D., F.R.S.,

Physician to Guy's Hospital.

UNDER the name of "spiritualism" is included apparently every conceivable occult influence which can produce phenomena of an extraordinary character, or feats beyond the range of human knowledge. It seems remarkable that clear-headed men, in their investigations of the subject, have not perceived how spiritualists have been maintaining doctrines of most opposite tendencies: as, for instance, when a person holds his hand over a table, and the latter moves, an influence is supposed to pass from his body, to which such a name as "animal magnetism" may be given; but when a woman's hand is guided to draw those remarkable pictures which were lately exhibited in London (like twisted rainbows or balls of colored worsted), it is said that "a spirit" is at work; and the same, I suppose, would be asserted when a lady residing at Highbury desires a lobster for supper and immediately it flies through the air from her friend at Liverpool (see *Spirit. Mag.*). What is said to be the exact influence which operates on the clairvoyant when he reads a newspaper by sitting upon it, I do not know.

It seems, I say, remarkable that anything in the shape of what is called "animal magnetism" should be included in the subject of "spiritualism": for the two powers seem to be diametrically opposed. For my part, I know nothing of any influences exerted by man upon man except those which are bound up with his material body. Whether if I considered vital force distinct from the properties of matter I might indulge in spiritual vagaries, I need not inquire. At present I am in accord with those who know of no forces in nature except such as are intimately associated with matter; and, more than this, that in all living objects the latent energies may be expected to become manifest in one form or another. A belief, therefore, in what may be called "animal magnetism" or "mesmerism," as the product of certain operations going on in the body, has nothing in common with the idea of a spiritual influence outside us, and which we cannot grasp. In fact, the two ideas seem of a very contrary nature. It is, indeed, because I disbelieve in spiritualism that I feel inclined to seek further information about real animal forces. When a man says that his hand placed near an object can cause it to move, he is speaking of an influence arising from a material machine—a statement not irrational; but when he says his hand is moved by a spirit, he is asserting something of a totally different nature, and of which we can know nothing. While, then, it seems to me a waste of time to investigate the follies of spiritualism, I can see no *a priori* reason for disbelieving that the forces of the human body may be liberated in a manner which may be felt or measured; and to such forces, if discovered, I would much prefer the old name "animal magnetism" than "psychic force," which carries us into the realms of spirits.

I may be allowed to remind those who have not thought much of the subject, that there is every reason to believe that the brain and nerve-centres are force-producing organs, and that "vis nervosa" does not stand for an imaginary essence, but is a real power. Can it be believed that the other organs of the body are chemical and heat-forming apparatuses, and that the large mass of brain is not elaborating its own forces? Let us think of the simple act of the movement of a limb. In this case it is said, I am aware, by some physiologists, that the forces in operation are in the muscle, and that the nerve does no more than set them free, as one might let loose the spring of a watch. This is no doubt true to a certain extent; for if the power came from the nerve, a horse's brain should be larger than a man's. Nevertheless there remains the fact of muscular tension (which others again deny) seen in the drawing of the face in facial paralysis independent of volition, in the flabbiness of palsied muscles, and in such a fact as the head nodding when we fall asleep or in any way lose our consciousness. Then also in the case of the viscera whose operations are regulated by the sympathetic nerves we must believe in some potential agency arising in the ganglia. That some kind of force is generated in the nervous system is proved by the existence of galvanic currents, and by the fact of the electric organs of the torpedo and other animals being but expansions of ordinary nerves.

That the vis nervosa does not stand for unknown quantity, but is a real force, I think we must conclude from the influence of the mind (cerebral action) on the body. For example, a want of will, associated with an inability to move a limb, may mean no more than an inactive brain; a determined or strong will which would enable a person to rouse himself from a bed of sickness may mean no more than an abundant supply of necessary nerve force to counteract the morbid state. The effects of passion or rage seen in fighting, using abusive language, or pacing up and down a room, may be nothing more than the overcharged cerebral apparatus letting off its steam. Then, again, if these forces are pent up, the effect on the body is not an imaginary but a real one. Page after page might be quoted from writers showing the influence of the mind on the body, as the production of inflammation in a joint by concentrating attention upon it, as John Hunter asserted might be the case; but every medical man can recall examples at his will. The poets could never be exhausted for instances where the passions of the mind have wrought evils on the body most deadly and real.

Seeing these phenomena, and looking at the large cerebral, spinal, and ganglionic masses, no surprise can be felt that all kinds of conjectures have been made as to the nature of the force they produce; and thus, whilst some have surmised it might be no more than electricity, others have said it has been allied to it and styled it animal magnetism. Many have been content to speak of a vis nervosa as a convenient phrase, and have not attached any definite meaning to it. It seems remarkable that such conjectures should have been left to loose-minded men, whilst physiologists and philosophers have for the most part ignored the subject. It may be asked—Is not our body a wonderfully active machine, a furnace, and a chemical laboratory?—does it not require a constant supply of fuel to preserve a heat forty or fifty degrees above surrounding objects? The question is scarcely necessary, for we are daily witness of all the correlated forces occurring in the animal body—heat, free electricity in many persons, light in some animals, motion in all. We may ask are any of these forces produced in the brain? Sir B. Brodie, it is well known, believed that the mere chemical changes in the body were not sufficient to produce heat, and that this was derived direct from the nervous system. Now, if there be any truth in this, it follows that when we are measuring the temperature of the body we are taking note direct of the vis nervosa. Of course, if this be so, we must go still further back to chemistry for the cause of the changes in the brain which are productive of force; but this is of no consequence in the argument. If the hand be placed over a delicate thermometer, and a movement occurs, we are witness to an influence produced by a force originating in the body; we call this heat, but there appears no reason why a similar phenomenon should not occur from another force related to it.

When a mesmerist says that an influence passes from the manipulator to the patient—that in passing the hands over a paralysed limb the latter is receiving strength, whilst the former is losing it,—that man is not a spiritualist, but a rank materialist; and to investigate the existence of such power appears to me not idle, but most important, especially if the vis nervosa be a real force, as I believe it to be. That any power is passing out of the body under ordinary circumstances I do not believe, for I consider that it is expended on the body itself, especially on the muscles, or to cause some ill working of the viscera within, if it be produced in superfluity. And I think so also for another reason: if any such force as psychic force were constantly free and operative around us, it would interfere with the investigations of scientific observers; but for centuries past there have been men at work with their weights and measures, who have all agreed about the specific gravities of substances, about the meaning of foot-pounds, and such like; all of which could not have occurred had there been any disturbing influence to interfere, for I am not aware that any inexplicable results have troubled philosophers which have made them look around for some undiscovered influences to account for them. The question, therefore, of the existence of such force as the so-called psychic force being always in operation around us has been answered in the negative as much as if it had been under special investigation, for philosophers have not found themselves troubled by any other powers than those which all have recognised as the usual properties of matter.

It by no means follows, however, that if the vis nervosa be a reality it might not occasionally escape its bounds; and, if so, it might be tested by peculiar reactions of its own, or by its being found to be another mode of light, heat, galvanism, or motion. These forces, which are constantly observed, may be intimately correlated with some new or undiscovered one which is peculiar to the living animal.

It is, then, because I am not a believer in spiritualism, and have not the slightest leaning towards it, that I see no impossibility in seeing vital forces, or the vis nervosa, subjected to the same scientific researches as any other forces in nature. I see no reason why Mr. Crookes should not investigate them, nor do I see why others should stand aloof. The subject is not one of spiritualism; it is rather eminently materialistic.

Grosvenor-street, October, 1871.

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A COMPLETE ANTERIOR LUXATION OF THE SEMILUNAR BONE FROM THE RIGHT WRIST WITHOUT TEARING THE SKIN.

By J. J. Chisolm, M.D.,

Professor of Operative Surgery in the University of Maryland, U.S.A.

The following case, which has recently come under my professional care, I believe to be one of unusual rarity. In looking up authority I can find no duplicate case of anterior luxation of the semilunar bone in which the skin has not been extensively torn, the bone usually hanging quite from the hand by some shreds of ligament. The case, I think, will be of interest to surgeons.

Alexander S——, aged twenty-five, a sailor by occupation, whilst taking in sail on March 24, 1871, fell from the yardarm upon the deck of the ship, a distance of about twenty-five feet. He fell upon his right side, with his right hand doubled under him, so as, in a measure, to break the fall. He was badly bruised about the right hip and side, but as these were not serious nor extensive injuries, he rapidly recovered from them. Ten days after the accident he reached port, and came under my treatment in the Baltimore Infirmary for a stiff wrist and painful arm, supposed to be fracture of the radius near the carpus.

There was comparatively little swelling of the arm and hand. The axis of the hand was perfectly in a line with that of both radius and ulna. My finger, firmly pressed upon the outer surface of the radius and ulna from the elbow to the tip of each respective styloid process, caused no sharp pain, so constant a symptom when a broken bone is pressed upon at its point of fracture. A hard, large, rounded subcutaneous prominence existed on the front of the wrist beneath the upper wrist fold, and directly over the site of the semilunar bone. This irregularly outlined incompressible knob stretched the skin, and was so firmly fixed as to exhibit not the slightest motion when manipulated with force. From the median position of this large firm mass, which had been detected after the fall, and which had not previously existed, luxation of the semilunar bone was diagnosed. This opinion was confirmed by thrusting a couching needle to the depth of an inch into the back of the wrist, and showing, by the free movements of the free end of the needle in every direction, that a vacant cavity existed, where normally the compact semilunar bone is found. The constant pressure kept up by the luxated bone caused forced and painful flexion of the fingers and wrist. The immobility of the bone in its new position rendered futile all attempts at reduction. Three weeks after the receipt of injury, finding that the presence of the bone would permanently destroy all the movements of the wrist, and that the slightest motion of the fingers continued to give severe pain, under the influence of chloroform I removed the semilunar bone through a single incision two inches in length, in the median line. The operation I found much more tedious than I had anticipated. The force applied at the time of the accident had not only squeezed the bone from its cavity, but had, at the same time, imparted to it a rotary movement of so extensive a character, that the concavity for the reception of the rounded head of the os magnum looked directly up the arm, whilst the convex surface belonging to the radio-carpal joint was facing the fingers. During this forcible rotation *in situ* of this semilunar bone, all of its connecting ligaments had not been torn through. Some of them had been so stretched over, and others under, the luxated bone as to tie it down firmly in its new position, and render it very difficult to enter a knife between it and its closely hugged neighbors to permit of its isolation and removal. Although these tense ligaments kept the semilunar bone immediately over its proper area, its rotated position brought together such reversed, and therefore irregular, surfaces of bone and socket as to render reduction quite impossible. Upon examining the semilunar bone when removed, a small fragment of the scaphoid was found attached to it, showing that the dislocating force had torn off a small piece of the contiguous bone.

Baltimore, May, 1871.

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TREATMENT OF GRANULAR OPHTHALMIA BY THE LOCAL USE OF QUININE (QUINIÆ BISULPHAS).

By C. Bader,

Ophthalmic Assistant-Surgeon, Guy's Hospital.

The unsatisfactory results of the treatment of granular ophthalmia, especially if combined with pannus (vascular cornea), will, I hope, justify the publication of the following remarks. Experiments were made, with various substances, on patients suffering from granular ophthalmia with pannus. The idea which led to the selection of the substances experimented with was, that granular ophthalmia is the result of some extraneous substance becoming lodged in the conjunctiva, and giving rise to what have been called granulations. Experiments made elsewhere with antiseptics on organic matter have shown quinine to be one of the most effective antiseptics.

The substances experimented with on granular ophthalmia were the scrapings and juice of the root of bryonia nigra, the nitric oxide of mercury ointment (hydrarg. nitrico-oxidi, gr. iii., adipis ʒ i.) and the bisulphate of quinine in powder. The remedies were applied morning and evening; of the quinine about as much as would go on the point of a penknife was placed, with a dry camel's hair brush, on the inner surface of each lower eyelid. No other treatment, such as exclusion from light, use of lotions, &c., was made use of. The hypothesis that quinine acts specifically upon granulations, as mercury, for instance, does on lice, may be erroneous; the effect may merely be that of an irritant, causing moderate suppuration, and, with it, removal of the granulations.

The effects on granular conjunctiva of bryonia nigra and of the nitric oxide of mercury ointment, though curious in other respects, are of little interest here. Suffice it to state those of quinine. In some cases its application was followed by severe smarting, which continued for ten or fifteen minutes; in other cases no pain whatever was felt; in all cases appeared increased purulent discharge from the conjunctiva, with shrinking of the granulations, and clearing of the surface of the cornea. The intolerance of light ceased rapidly in all cases; the dilatation of the pupils appeared in from twelve to twenty-four hours after the first application of quinine. The pupils, though dilated in ordinary light, contracted well on exposure to strong light.

Case 1.—A. B., aged fourteen. Granular ophthalmia for six years; at present vascular cornea (pannus) in both eyes; pupils barely visible; granular conjunctiva; extreme intolerance of light. The application of bryonia nigra was followed by symptoms of purulent ophthalmia (chemosis, swollen lids, &c.), which, with slight improvement of sight, subsided within ten days; the intolerance of light persisted. Quinine was used and the bryonia discontinued. The eyes improved rapidly; the granulations became smaller, and the intolerance of light disappeared completely within four days.

Case 2.—C. D., aged ten. Granular ophthalmia with pannus for two years, with intolerance of light. The use of the nitric oxide of merçury ointment (for fourteen days) was followed by slight improvement. It was discontinued and quinine substituted. Within ten days all intolerance of light had ceased, the cornea became much clearer, &c.

Case 3.—E. F., aged ten. Granular ophthalmia for one year. The intolerance of light persisted for three weeks while the nitric oxide of mercury ointment was used; it disappeared on the second day after the quinine had been applied.

Case 4.—G. H., aged thirty-four. Granular ophthalmia for one year, with pannus and intolerance of light. Quinine was used for ten days; on the third day the pupils became dilated, and on the fourth all intolerance of light had ceased.

Case 5.—I. J., aged eleven. Granular ophthalmia for four months, with slight pannus and intolerance of light. Quinine treatment for three weeks. All intolerance ceased; cornea much clearer; granulations red, much smaller; slight purulent discharge.

Case 6, aged fourteen, bad for three months; Case 7, aged fifteen, bad for four years; Case 8, aged ten, bad for two years, improved equally rapidly under the quinine treatment; in all did the intolerance of light disappear within from five to ten days, with marked improvement in the state of conjunctiva and cornea.

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SLOUGHING OF THE CORNEA IN CONNEXION WITH DISEASE OF THE FIFTH NERVE.

By C. S. Jeafferson,

Assistant-Surgeon to the Newcastle-on-Tyne Infirmary.

The influence of the fifth nerve upon the nutrition of the eye has been investigated by many physiologists; and Magendie, Longet, and others, have shown that destruction of this organ follows its division. It is rare, however, in practice to meet with cases which illustrate the pathological bearing of these experiments; and in few of our text-books on ophthalmic surgery do I find disease of the fifth nerve assigned as a cause of ulceration of the cornea. It is my belief that this neurotic origin of corneal disease is not unfrequently overlooked; and yet, when once the attention is directed to it, its symptoms are found to be very striking and peculiar. The extent of the disease—the curious, dry, leathery appearance of the sloughing cornea—the purple color of the ocular tunics—are quite different from what is seen in ordinary inflammatory affections. The absence of secretions—the almost entire absence of pain and the tolerance of light, however intense—are very characteristic; and when to these symptoms are added the complete anæsthesia of the cornea and an absence of sensation in the whole of the conjunctival surface of the globe and lids, a picture is drawn which it is not difficult to recognise.

I am afraid it is useless to speculate as to the exact nature and locality of the changes which, in the case I have reported below, gave rise to the lesion of the eye. Many physiologists assert that it is through the medium of the sympathetic that the fifth nerve influences the processes of nutrition; and experimental physiology has shown that removal of the superior cervical ganglion of the sympathetic is followed by the same train of symptoms, as far as the eye is concerned, as division of the fifth nerve. In my case the patient's chief complaint was of a pain deep in the neck in the region of the ganglion above-mentioned; and its value as a symptom was much enhanced by his having made this statement quite spontaneously.

G. B——, aged forty-three, a foreman in a large boot-making establishment, came to me under the following circumstances:—Three weeks ago, on awakening one morning, he observed the sight of the right eye was dim. Two days after this the ocular tunics became slightly vascular. On the third day a speck made its appearance on the centre of the cornea; and upon the ninth day the whole of the cornea had become quite white. I saw him on the fifteenth day, and found the eye in the following condition:—The conjunctival epithelium and anterior elastic lamina of the cornea had sloughed, with the exception of a small portion round the extreme edge; its fibrous laminæ had become converted into a dead-white leathery substance, which could be stripped off in fine layers. The conjunctiva was of a muddy purplish color; there was slight chemosis, but a remarkable dryness of its surface and of that of the cornea. The whole of the globe and the inner surface of the lids was *completely anæsthetic*—so much so that he was unconscious when they were touched, or an irritant solution was brought in contact with them. There was partial anæsthesia of all parts supplied by the cutaneous nerves of the ophthalmic division of the fifth; and the anæsthesia was complete over a small space covering the right malar bone. He has no pain in the eye, but says it feels numb. There is no intolerance of light, and he can just distinguish the position of the window. There are few subjective symptoms. Occasionally he says he is attacked by a kind of trembling sensation, which commences in the pit of the stomach, and gradually extends to the limbs; and he has at times a rather severe pain at a spot deep in the neck, between the angle of the jaw and the sterno-mastoid muscle. His tongue is furred; and I observed that he had lost all the molar teeth upon the right side. The man has always had plenty to eat and drink, has never suffered from any constitutional disorder, and is free from cardiac, pulmonary, or any other disease that I can ascertain. The right eye is good, and the fundus normal.

Newcastle-upon-Tyne, Oct. 1871.

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SOURCE OF FALLACY IN ESTIMATING THE MORTALITY FROM CHLOROFORM.

By GEORGE BUCHANAN, A.M., M.D.,

Surgeon and Lecturer on Clinical Surgery,
Glasgow Royal Infirmary.

DEATH from chloroform inhalation is a subject worthy the attention of all practical surgeons, and the particulars of each case deserve the strictest investigation. Yet the most accurate attention to details very often leaves us wholly ignorant of the conditions which accompany or cause the fatal result. The object of this communication is to show that one of the sources of fallacy which have been suggested is *not* an ideal one, but must be taken into account in any statistical enumeration which may at any time be drawn up. It has often been stated by writers on this subject that a certain number of deaths which have occurred while persons have been under the influence of chloroform must be set down to natural causes, wholly independent of the anæsthetic. To some, this has seemed so unlikely as not to be worthy of consideration; but when we compare the number of sudden deaths which happen in any community with the very rare occurrence of death under chloroform, it will not seem so unlikely that the coincidence may occasionally occur. A remarkable case of sudden death bearing on this question happened lately in my practice, which has made such an impression on my mind as to induce me to record it.

A patient from the country, recommended to my care by Professor Allen Thomson, came to town in July last to have a small epithelial tumour removed from his lower lip. He and his wife took a lodging in the neighborhood of my house, and a day was fixed for the operation. A more urgent case requiring my services happening on the morning appointed, I sent a message that I would postpone the operation till the following morning. Next morning I got a message from the lodging-house landlady, that my patient had gone out the previous day to execute some commissions, and had not returned. Shortly after this I received the startling announcement that, soon after my patient had left his lodgings, he had dropped down dead in the street! There is little doubt that if I had proceeded with the operation on the morning appointed, my patient would have died during its performance; and I should, for the first time, have been called upon to record a death under chloroform. In this case the death occurred from "natural causes," just about the time appointed for the operation.

The coincidence is sufficiently striking to show that many a patient may die *under* chloroform though not *from* choloform, and that such a consideration should modify our judgment in estimating the value of tables of "mortality from chloroform."

Bath-street, Glasgow, Oct. 1871.

OBTURATOR HERNIÆ.—Dr. Edward Cruveilhier recently exhibited to the members of the Surgical Society of Paris a preparation obtained from a patient eighty-one years of age, who had all the symptoms of strangulated hernia without offering the slightest sign of a protrusion about the rings, &c. When alarming symptoms set in M. Cruveilhier opened the colon. The evacuations were very abundant and offensive, and the woman was much relieved. She could then take a little broth. But the former symptoms soon recurred, and the patient died. On an inspection it was found that a knuckle of bowel had been passing through the obturator foramen, but very partially, being, in fact, only pinched. The very small size of the strangulated portion explains the non-discovery of the actual nature of the case.

A Mirror
OF THE PRACTICE OF
MEDICINE AND SURGERY
IN THE
HOSPITALS OF LONDON.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

HOSPITAL FOR SICK CHILDREN.

CASES OF CHOREA TREATED WITH SULPHATE OF ZINC.

(*Communicated by* Mr. H. T. BUTLIN, Medical Registrar.)

WE conclude this month the report of the series of cases of which the first three were given in our last number.

(Under the care of Dr. WEST.)

CASE 4.—William G——, aged seven years, a pale, delicate-looking boy, was admitted with slight general choreic movements which had existed for some three or four weeks before admission. No cause was assigned for them. He had suffered no fright nor attack of rheumatism, but he had always exhibited an irritable and excitable disposition. His father was said to be subject to fits, probably epileptic.

For the first few days after admission his morning temperature was about 100°, the evening temperature being slightly lower. He was kept in bed, with no other treatment than an occasional dose of aperient medicine, for seventeen days, when the course of sulphate of zinc was commenced with single-grain doses every six hours. In twenty-four days the dose had reached twenty grains, and vomiting occurred several times. The choreic movements, which had diminished at first under the zinc treatment, became again more noticeable. He was therefore ordered to take once more to his bed, which he had left for some days. This measure was followed by a rapid decrease in the movements. On the thirty-fifth day of the course of zinc he again became sick, and a little strengthening medicine was substituted.

As the patient remained weak and poorly, he was not sent to the Highgate Convalescent Hospital until the fifty-fifth day, whence he returned in perfect health after a stay of three weeks.

(Under the care of Dr. DICKINSON.)

CASE 5.—Isabella J——, a tall girl, eleven years of age, had ultra-rough skin, and cracked lips, and presented well-marked choreic movements of face, arms, and legs. Accompanying the first sound of the heart was a slight murmur, most distinct over the base. She appeared to have had chorea for the three weeks before admission. Two years previously she had had an attack of scarlet fever, which was followed by acute rheumatism. Her father was subject to fits (epileptic?). Except that an occasional dose of castor oil was given to correct obstinate constipation, no particular line of treatment was adopted for a week, when, as she appeared to become worse rather than better, she was put on the sulphate of zinc. Twenty-six grains, three times a day, was the maximum reached in thirty days. It was then observed that her skin was much softer, and that her lips were no longer cracked. The choreic movements had become decidedly less, but were still well marked. In two days more, the taking of the medicine having been several times followed by sickness, its use was countermanded. After a week's interval, as there remained some irregular movement, two minims of the liquor strychnine were ordered to be taken three times a day; in a week more the dose was increased to three minims, and, in yet another week, to four minims. Ten days after the increase of the dose of strychnia she was sent to Highgate quite free from choreic movement.

CASE 6.—Louisa W——, eleven years of age, was taken ill in church about a fortnight before admission, and on the following day choreic movements were noticed, which so rapidly increased that when she was brought to the hospital she was unable to stand, and it was found necessary to carry her up stairs. When she reached the ward she was deposited on a couch. She threw her limbs in all directions, and her face was disfigured by very marked contortions. There were lines at the corners of the mouth, and her lips were very dry, but not broken. There was an occasional but distinct prolongation of the first sound of the heart, most plainly heard to the right of the left nipple.

The treatment by sulphate of zinc was commenced as soon as the bowels had been well acted upon by calomel and jalap, and ten grains of chloral were ordered to be taken each night. Ten grains of the sulphate on the eighth day made her sick; the dose was therefore not again increased until the twelfth day. With fifteen grains, on the fifteenth day, she again vomited, therefore sixteen grains were not given till the eighteenth day. By this time, however, she was very much quieter, and was able to talk tolerably distinctly, although, on admission, she had scarcely been able to speak. A maximum of twenty-five grains was reached on the thirty-third day, and in the course of the following seven days the dose was rapidly diminished to two grains. She left her bed on the thirty-seventh day. By the forty-fifth day she was quite well of her chorea, and in a few days more she was sent to Highgate.

(Under the care of Dr. WEST.)

CASE 7.—Sarah W——, aged twelve years, had been an in-patient two years previously with a first attack of chorea which had detained her in the hospital about three months. Her second attack had commenced some six weeks before admission. No cause could be assigned for either. She herself had never had rheumatism, but her father was said to have died of rheumatic fever.

She was found to be affected with general movements. The action of the heart was irregular, and a systolic murmur was audible at the base and on the right side of the sternum. The progress of the case was rapid. At the end of a week she was ordered two grains of sulphate of zinc in half an ounce of water. In twenty days she was taking twenty-four grains, the quantity having been increased by one or two grains each day. After twenty-three days of zinc treatment she was sent to Highgate, almost entirely free from irregular movements.

Case 8.—Frank S——, aged seven years, was a pale, delicate-looking boy, suffering from choreic movements of the trunk and limbs generally, but more particularly of the right side, consequent on a second attack of rheumatic fever which had occurred about six weeks before admission. On examination, he was found to have a loud mitral regurgitant murmur. His bowels were opened with calomel and jalap, and a grain of zinc was ordered on the fourth day. When, on the seventeenth day, twenty-five grains had been reached, he had very much improved; but he then became sick for the first time, and had a slight amount of diarrhœa. On the thirty-second day the zinc was stopped, on account of persistence of diarrhœa and occasional sickness. He was at the time taking fifteen grains. From that time until he was sent to Highgate, about eight weeks after admission, he only took a little tonic medicine. The choreic movements had not even then entirely left him, and it was not until he returned from the Convalescent Hospital that he could be considered thoroughly cured.

Case 9.—Minnie S——, aged eight years, had four years previously had scarlet fever and an attack of rheumatism. These were immediately followed by St. Vitus' dance, from which she had never since been quite free. Her general health was said to be good, although she occasionally had pains of a rheumatic character in her limbs.

On admission, she had well-marked chorea affecting both sides of the body. There was no cardiac murmur, but the heart's action was irregular.

The zinc treatment was commenced on the third day, and in twenty days the dose had risen to twenty five grains. A grain of sulphate of iron and four minims of aromatic sulphuric acid were then added to each dose. But she was already comparatively free from irregular movement, except on a few occasions when she was allowed to dress and sit upon the couch for a short time in the afternoon, when she became very decidedly worse. She was therefore kept entirely in bed. On the thirty-first day of active treatment the zinc was stopped, and some nux vomica was substituted, but this did not agree with her; shortly afterwards, therefore, a grain of quinine three times a day was ordered, and she was again allowed to sit on the couch for a little while in the afternoon—this time without any unfavorable consequence.

She went to Highgate ten weeks after admission, scarcely any choreic movement being then noticeble, and on her return three weeks later not a twitch remained.

(Under the care of Dr. Dickinson.)

Case 10.—Elizabeth G——, twelve years of age, was admitted with a first attack of chorea, dating from a fortnight previously. She had twice had rheumatic fever; the last attack had occurred about a year before. So violent were her movements that it was found necessary at once to carry her upstairs, and put her to bed. Her speech could not be understood without great difficulty. The heart's action was irregular, and a slight murmur, occurring with the first sound, was audible just below the nipple.

She was at first ordered five minims of the tincture of Calabar bean in half an ounce of water every six hours. At intervals of two or three days the dose of the tincture was increased by an addition of five minims, until forty-five minims were taken every six hours. But as no benefit had resulted, the treatment was discontinued, and two grains of sulphate of zinc ordered. At eight grains the choreic movements were already much diminished, and the patient was allowed to go into the garden. The dose had not reached more than eighteen grains when she vomited several times. In the course of a few days it was therefore again dropped to five grains, and she was sent to Highgate quite free from all irregular movements, after twenty-eight days of zinc treatment.

(Under the care of Dr. West.)

Case 11.—Maria W——, aged twelve years, was admitted with general choreic movements of some ten days' standing. Both her parents suffered with rheumatism, but no cause could be assigned for her complaint. The heart's action was irregular, and was accompanied by a slight occasional murmur at the base.

Two grains of sulphate of zinc were at first ordered, but at four grains the treatment was suspended in consequence of an attack of diarrhœa. The movements soon became so aggravated that she kicked holes in the blankets, and her toes appeared even through the strong counterpane. She cried uproariously at night, and in spite of ten grains of chloral, or the same number of minims of laudanum, she got little or no sleep. Therefore, on the twelfth day, the use of the zinc was recommenced. A fortnight later she was undoubtedly better, but the movements were still very violent, and the treatment was again changed to three grains of quinine to be taken three times a day, whilst a cold bath, which had been ordered four or five days previously, was persevered with.

Not the slightest improvement being manifested in the course of the following week or ten days, the treatment was a third time altered; three grains of the zinc were ordered three times a day, the quantity to be increased by the addition of three grains per diem. When twenty-three grains had been reached, the dose was lessened at the same rate; and, a month after the making of the third change in the mode of treatment, the patient was sent to Highgate, free from all irregular movement, though still suffering slightly from a state of general paralysis which had followed the gradual cessation of the chorea.

Case 12.—Ada J——, a well-nourished, comely child, was admitted with well-marked choreic movements, following a febrile attack of an indefinite character, which had occurred a week or two previously. The action of the heart was very irregular, and there was discovered a loud apex murmur, which was not audible at the back.

On the third day the sulphate was first administered. The dose was rapidly raised to twenty-four grains, and then as quickly lowered; and, a month after admission, she was sent to the Convalescent Hospital.

There had been no sickness throughout. The choreic movements had been absent since the twenty-second day of treatment; and, for some days before she left for Highgate, no murmur could be heard over the heart.

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ST. THOMAS'S HOSPITAL.

TWO CASES OF TETANUS TREATED WITH HYDRATE OF CHLORAL; RECOVERY.

(Under the care of Mr. Croft.)

It will be observed that neither of the following cases belonged to the most acute and commonly fatal kind. In their treatment, hydrate of chloral was used to the exclusion of such drugs as opium, atropia, aconite, Indian hemp, &c. Purgatives were administered as occasion required. There is not any uncertainty, therefore, as to which drug the relief was due. The action of the chloral was such as to induce physiological rest. In this way the reparative powers of the sufferers were supported and increased, while the incidental pathological condition underwent those favorable changes which in the end were curative.

Case 1.—William S——, aged fourteen, basket-maker, was admitted with the following history. Thirteen days previously, while he was at work, his knife slipped and cut across the outer border of the left forearm near the wrist. The wound was about an inch and a half long, and bled freely. He was advised to go to the hospital on the fourth day, and attended as an out-patient up to the day of his admission.

On admission, at midday, he presented the risus sardonicus and stiffening of the muscles of the jaw and neck. He was ordered to be cupped over the lower cervical and upper dorsal spines to six ounces, to take a dose of castor oil, and to have a retired, quiet bed.—5 p.m.: Has had slight twitchings of face and limbs; risus marked; the bowels have acted; he has taken beef-tea and milk.—8 p.m.: Frequent slight spasms occur; he is restless, and moves with difficulty. Ordered ten grains of chloral with ten of bromide of potassium.

Second day.—Slept for an hour after his draught. On awaking the spasms became frequent and he was restless.—2 a.m.: The draught was repeated and he slept until 8 a.m., but not soundly. The bowels acted, and he passed water during the time. Tongue coated yellow.—10 a.m.: Temperature 99°.—12 noon: Has had less twitching of limbs and face.—4 p.m.: Has bitten his tongue in spasms; opisthotonos and pains in back; bowels have acted freely.—8 p.m.: Temperature 100·1°; skin warm and moist; tongue coated brown. Ordered chloral every four hours and poppy fomentations to the spine.

Third day.—Chloral repeated at 12, 4, and 8 a.m. He slept at intervals, but was restless. Had spasms with opisthotonos. Took plenty of fluid and light nourishment.—11.15 a.m.: Temperature 100·1°.; pulse 74. He is more rigid; the muscles of the back very stiff; the head drawn back on to pillow; skin warm and moist.—3 p.m.: Severe fit affecting whole body; head and feet dug into bed; arms extended; face flushed; features distorted; profuse perspiration.—5 p.m.: No return of spasm.—8 p.m.: Has been asleep for two hours, but was disturbed by twitchings and flexions of legs; tongue brown. Has taken chloral every four hours.—8.40 p.m.: Temperature 106·7°; pulse 100.

Fourth day.—After the draught at 8 p.m. he slept until 12 a.m., but had frequent spasms. Draught repeated at 12 a.m. and 4 a.m., but he did not sleep until 5 a.m., when he fell asleep and slept soundly until 8 a.m.

Fifth day.—Has passed a bad night; has had several spasms, with painful opisthotonos and extended arms.—4 p.m.: Has not taken any chloral since 8 p.m. yesterday. To take twenty grains every five or six hours.

So matters went on till the tenth day, with daily decreasing spasms, the chloral being given in frequently repeated doses.

Eleventh day.—He slept well during the night, requiring to be roused to take nourishment. No severe spasm occurred during the night or day. He took nourishment well. Chloral at 2 a.m., 8 a.m., and 10 p.m. At 10 p.m. temperature 98°; pulse 98.

From this time he steadily improved. The spasmodic seizures slowly diminished in force and frequency. He took nourishment well. Chloral was given as occasion seemed to require. The bowels were costive and demanded purgatives. The temperature and pulse remained about the same.

The chloral was continued about every six hours until the thirtieth day, when it was abandoned. No spasms were noticed after the twenty-sixth day. He remained in the hospital until the sixty-second day, when he went out strong and well.

Case 2.—S. A. A——, aged nine years, was admitted half an hour after having scalded herself with water from a tea-kettle. Both thighs and knees were injured, but chiefly the left. Superficial sloughs of integument separated and left granulating sores.

On Dec. 2nd (the fourteenth day after admission) the sister reported that the child had not slept well. The sores bled on being dressed, and the child screamed from the pain of dressing. Warm water dressing was used. She was restless and irritable, and said she had a sore throat, difficulty in swallowing, and pain at the back of the neck. On the next day she complained still of dysphagia. She had not slept well. Had taken her food much as usual.

On Dec. 4th, after a sleepless night, she had a general tetanic seizure. The legs were rigid, the back was arched, and she suffered pain at the epigastrum. The muscles of the abdomen were tense. The "risus" was plainly marked; all muscles of expression being in a state of tension. Skin dry; tongue dry; pulse quick; sores ceased discharging. At 11 a.m. fifteen grains of chloral hydrate were administered, and the dose was repeated at 2 p.m. At 3 p.m. she was asleep; at 5 p.m. she had one very slight spasm; she took beef-tea and wine; at 9 p.m. she took two grains of calomel; at 10 p.m. twenty grains of chloral hydrate.

She had slight attacks during the next week, but these were kept under by the chloral, given, in from ten- to twenty-grain doses, three or four times a day. From Dec. 12th she gradually improved. On the 21st the report states that at 6 a.m. the sister of the ward found the child drowsy, not rousable, with pupils alternately contracted and dilated, with a natural expression of face, but convulsions of the trunk and extremities. She then had a severe attack of tetanic spasm, which was relieved by chloral injected into the rectum. In the end she was discharged quite well, and with useful limbs. The chloral was repeated night and morning until the thirty-seventh day of the attack, when the morning dose was discontinued. On the thirty-eighth day the evening dose was given up, and she was pronounced free from all traces of tetanus.

Remarks.—In the first case a temperature of 106·7° F. was noted on the evening of the third day. This sudden elevation and rapid decline seem to be characteristic of tetanus, though it cannot be relied upon as a sort of index to the probable future course of the disease. In both cases constipation, as is common in tetanus, was a marked symptom. Each patient manifested decided relief after free evacuation of the bowels. A remarkable blush of erythema occurred several times in the first case. It was traced to the ingestion of port wine quickly after a dose of chloral. Mr. Croft has never observed it to occur after chloral only. Of the sudden attack which occurred to the little girl on the twentieth day he professes himself unable to offer any explanation, except that it would appear to have been a sudden relapse of tetanus attended by unconsciousness. The administration of chloral by the rectum seemed to be followed by the happiest results.

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ST. MARY'S HOSPITAL.

SUMMARY OF FIVE CASES OF ACUTE RHEUMATISM; WITH CLINICAL REMARKS ON THEIR DURATION AND TREATMENT.

(By Dr. Handfield Jones.)

The abatement of the symptoms of acute rheumatism is so gradual that it is difficult to date accurately the cessation of all morbid action; but in the following cases the subsidence of pain, and the decline of the temperature and pulse to the normal standard, have been taken as the guide. If any inaccuracy remains to be allowed for, it is that the duration of the disease has been overestimated rather than the reverse.

In the case of J. E——, aged forty-nine, whose attack lasted about forty-five days, alkalies were resorted to for two days, then quinine in full doses, and lastly strychnia. His urine remained alkaline for sixteen days or more after the alkali had been omitted. A. B——, aged thirty-three, recovered completely in eleven days. Full alkaline treatment was commenced on the fourth day, and gradually slackened. He took no other kind of medicine. In the case of C. G——, aged twenty, the disease lasted only nine days. The treatment commenced on the third day, and consisted exclusively of five-grain doses of quinine, four times a day. His urine was found to be alkaline on the fourth day. J. B——, aged twenty-seven, was admitted on the fifth day of illness, and remained subject to rheumatism about twenty-nine days. Five-grain doses of quinine, repeated four times a day for three days, produced no improvement. He was therefore ordered one grain of iodide of potassium and five grains of the bromide, in two ounces of citrate-of-potash mixture, every two hours. In four days he exhibited a decided improvement, and in a week he was comparatively well. The fifth case was that of a man of good constitution, thirty years of age, who was admitted on the fourth day of his illness. He had full alkaline treatment and none other, and recovered in thirteen days.

Dr. Handfield Jones's remarks on these cases were to the following effect:—It is clear that acute rheumatism has not a fixed period of continuance, but is, on the contrary, subject to very considerable variations. The question then arises, on what conditions do these variations depend? To some extent, possibly, on the time which elapses before a case comes under treatment, but to a very much greater extent on the *quality* of the individual system. Some systems present a much greater resisting power to morbid agencies than others, succumb when they are overborne only for a short time, and then rapidly spring back again to health; others when once depressed are very slow to regain their former status. Advanced life and debility, however induced, favor the latter condition. Predisposition to a particular disease must also be taken into consideration: some are prone to gout, some to rheumatism, some to Bright's disease, and some to fatty degeneration; and whatever that predisposition may consist in, the man who is predisposed to rheumatism will succumb more readily to acute rheumatism, and recover more slowly than a man who is not. Therefore, as it is difficult in any given case to form a definite estimate of the strength of a man's constitution, or of his degree of debility, as well as of his predisposition to disease, it is a very difficult matter to détermine beforehand the duration of an attack of rheumatic fever.

The next question is, what are the circumstances which should direct the physician to a choice of *remedies?* These also admit of only vague definition. In some measure they depend on the same circumstances which influence the duration of the disease. It is almost certain that as soon as the *urine* becomes alkaline (and it is so sometimes from the commencement), the use of alkalies will be not only useless, but injurious; still more certainly will it be so if the perspiration also be alkaline or neutral. If, on the contrary, the urine be highly acid and rather scanty, alkalies are very likely to be serviceable. When there is great thirst, with a dry, clammy mouth, alkalies in an effervescing form are very grateful. The state of the *pulse* is important. If it be very weak and soft, alkalies are contra-indicated; this is especially the case if, at the same time, the skin yield a copious flow of neutral sweat. In such cases quinine in full doses is very often of great service. Not unfrequently a mixed treatment of alternately given quinine and alkalies, as practised by Dr. Garrod, is productive of much good. In a recent case of a woman who was very weakly, anæmic, menorrhagic, much distressed, restless and half delirious at night, with a high temperature, that treatment was followed in four days by almost entire freedom from rheumatic pain, and by greatly improved sleep, although the temperature remained high for many days afterwards.

The condition of the *nervous system* should be noted. When it is irritable bromide of potassium and repeated subcutaneous injection of opiates afford more relief than any other measure.

Blisters are allowed by Dr. Gull and Dr. Sutton as valuable agents in relieving suffering, though they do not curtail the course of the disease. But they are not suited to cases in which there is not much inflammatory swelling, although the pain and intolerance of movement be very great; they add to the nervous irritability, and may advantageously be made to give place to continuous poultices, medicated, perhaps, with laudanum or an alkaline solution.

Purgatives, containing calomel, or podophyllin, or extract of colchicum, should always be given at the outset, and repeated according to need if the tongue be foul, the taste perverted, and the patient loathe food.

For the *pains* which are apt to linger *about the shoulders and arms* after all febrile action has ceased, the subcutaneous injection of atropia will generally be found to be the best remedy, but sedative liniments are often useful.

As to *food*, the leading of the appetite should be followed. It can be of no use to intrude on an unwilling stomach food which it cannot digest; but as soon as the patient desires solids he should have them. Until then, soups and milk and such like food are essential.

A high temperature appears to be rather an indication for giving than withholding *stimulants*, and wine should always be given during the febrile stage if the patient be low.

In conclusion, Dr. Handfield Jones said that remedies administered carefully and with discrimination may, and often do, effect much good; and that, although the intimate pathology of acute rheumatism is so little understood that a rational treatment is impossible, no justification can be advanced for the neglect of treatment by drugs.

UNIVERSITY COLLEGE HOSPITAL.

[Clinique for Skin Diseases.]

REMARKS ON THE TREATMENT OF ITCH.

(By Dr. Tilbury Fox.)

"We talk of papular, vesicular, and pustular scabies; whereas the scabies itself is only the acarus in its burrow—the cuniculus with the vesicle at one end (the result of effusion set up by the entrance of the acarus), and the embedded acarus, showing itself as a white opaque speck, at the other end. All else is merely secondary to the irritation set up and the scratching practised for its relief. The papules are erected and congested follicles, the pustules suppurating follicles; and these papules and pustules occur as a part of many other diseases in which the skin is subjected to severe irritation. Kill the acari, and the secondary eruption disappears of course. But what do we do usually? We treat not only the essential disease, the real scabies—namely, the acarian furrow and its embedded ova and acari,—but also the secondary results in the same manner, applying to them the same parasiticide. Yet we should treat the former by parasiticides and the latter by soothing remedies; the more so as we know that the acari are generally to be found in certain localities. In recent cases in adults the localisation of the acari to the interdigits and the region of the wrists is complete; and it is easy to do harm by intensifying the secondary irritation, though the original cause (the acari) may be destroyed by our remedies. Therefore I say, in recent scabies use the parasiticides, sulphur or storax, petroleum, benzine, or the like, to the wrists and interdigits, and simple unguents to other parts. In chronic scabies the case is different, for here the acari may be more or less ubiquitous as regards the body. But even here a distinction is to be made: the parasiticide should be applied to the small and fine rash, and not to the ecthymatous pustules.

"Error number one, then, in the treatment of scabies, which is often made, is the application of parasiticides to 'the wrong place.' Error number two is the use of too powerful parasiticides. We need only use half a drachm of sulphur to the ounce of lard; there is no occasion for a stronger ointment nor for hellebore ointment. Gentle friction for a long time with a milder preparation is all that is required. Error number three is the use of parasiticides for too long a time. The use of a parasiticide for two or three days should be followed by a good washing and the discontinuance of the remedies for a night. If the patient be not troubled with itching during the night he may conclude that the acari are killed, and all we need do is to guard against the hatching out of fresh acari by the light application of our parasiticide once a day to any 'pimply' places for a few days longer, taking care that the foul clothes are well heated or scalded. It often happens that the remedy used to destroy the acari is continuously used until it sets up on its own account severe irritation, which is mistaken for an increase or spread of the scabies. 'Not too strong and not too long' is my rule for the use of remedies in scabies. The occurrence of red, rough, erythematous patches is a sign that the remedy itself is creating a disease.

"I do not like sulphur baths. Don't use them in recent cases; you need only treat the hand with parasiticides. In our new baths we shall cure our patients at a sitting, and disinfect their clothes at the same time. But in private practice you must follow the old-fashioned plan. I prefer, for ordinary use, an ointment made of half a drachm of sulphur, five grains of white precipitate, ten drops of oil of chamomile, five drops of creasote, to one ounce of lard. For chronic scabies sulphuret of potassium baths or an iodide of potassium lotion, one drachm to six ounces of water, are good; but I like the first-named remedy as well as any. Should we disinfect the clothes? Yes, by strong heat. I cannot *prove* to you that the clothes convey infection; but I believe it better to be on the safe side."

ST. BARTHOLOMEW'S HOSPITAL.

A CASE OF TYPHOID FEVER COMPLICATED BY HÆMATURIA AND PROSTATIC ABSCESS, LEADING TO EXTRAVASATION OF URINE.

(Under the care of Dr. Andrew.)

For the following notes we are indebted to Mr. P. Butler Stoney, house-physician.

A sailor, thirty-five years of age, a man of intemperate habits, who had had a slight attack of dysentery twenty years previously in India, was admitted with the following symptoms. He had a confused, stupid appearance; the tongue was tremulous, glazed at the tip, and elsewhere furred; the hands also were tremulous; the abdomen was distended, tympanitic, marbled with enlarged superficial veins, and presented special tenderness in the left iliac region. The patient was made aware of something being wrong with him when, a week before admission, while carrying a load, he was suddenly seized with a sharp pain, which darted from the left iliac region to the shoulder. This pain had not left him on the day of admission, and, from the day when he first felt it his health and strength had rapidly declined. He was placed upon two and a half grains of the soap-and-opium pill every six hours; and for the first two days, the temperature ranged between 100° and 101° F., the pulse from 80 to 84. On the third night he manifested violent delirium, but his temperature and pulse on the following morning were 98·8° F. and 68 respectively. He was ordered half a drachm of sal volatile in decoction of bark four times daily, and ten ounces of wine. On the fifth day the delirium remained unabated, the pulse was 100, the temperature 104·2° F., and the character of the evacuations left no question as to the nature of the disease. Ten drops of dilute hydrochloric acid were ordered to be given in water every six hours. In the night the temperature rose to 106° F., the pulse to 108. After an hour and a half of wet-sheet packing, the former had fallen to 103·6°, the latter had risen to 120. On the following day both the urine and evacuations were darkened by the presence of blood. It was some days before the blood entirely disappeared from the urine, which presented the characters usual in renal hæmorrhage. On the twentieth day, the pulse and temperature having fallen four days previously, a swollen, red, and tender spot was found over the pubes, and the scrotum and perineum were observed to be similarly affected. Suitable incisions were made by Mr. Langton, and the patient expressed much relief. He, however, died twenty-eight hours later.

At the post-mortem examination, cheesy nodules were found at the apices of the lungs. The convolutions of rather more than the last foot of small intestine were adherent to each other and to the pelvis, and the last three feet of its inner surface were studded with numerous ulcers. The remainder of the small, and the whole of the large, intestine were healthy. In the kidneys, and confined almost entirely to the cortical portion, were found numerous small white nodules, none of which were wedge-shaped, but some of which were elongated, and arranged perpendicularly to the cortex, and others were irregularly formed. An abscess cavity, through which the urine had escaped, was found in the prostate gland.

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KING'S COLLEGE HOSPITAL.

OPERATIONS ON THE 4TH NOVEMBER.

(By Sir Wm. Fergusson.)

Removal of a Fibrous Tumour from the Roof of the Mouth.—The patient was a woman of about twenty years of age. From the surface of the hard palate, to the left of the median line, there sprang a tumour of the size of a large filbert, somewhat lobulated, and covered with healthy mucous membrane. As the roof of the mouth was rather low, this tumour sufficed to fill the entire cavity of the closed mouth. An incision having been made in the mucous membrane, the tumour was readily turned out by means of an elevator, and proved, as had been suspected, to be of the fibrous variety.

Operation for Hare-lip, with Removal of the Intermaxillary Bone.—One operation had already been performed on this patient, who was about one year of age; but no union had been effected. This result Sir Wm. Fergusson attributed to the omission to remove a projecting intermaxillary bone. He observed that the operation could not be said to have been really completed without the removal of the projection in all cases where it exists; because, as the teeth which it evolves never grow well, no good reason can be adduced for preserving it. The cleft was in the right side, and extended to the hard and soft palates.

Sir W. Fergusson operated on two other patients, adults, for cleft in the hard and soft palates. He directed his attention to the latter only, reserving the anterior portions of the fissure for a future occasion.

(By Mr. Henry Smith.)

Operation for Fistula in Ano.—Mr. Henry Smith operated on a young woman twenty-three years old, for severe fistula in ano. About two inches within the rectum was a firm stricture, through which the finger could only be passed with difficulty. There were several complete fistulous passages. One of these, of which the external orifice was situated at least three inches from the anus, communicated with the bowel so far beyond the stricture that it was only with the greatest difficulty that the finger could be made to reach the end of a probe passed through it, and the operator seemed to have misgivings as to the amount of hæmorrhage which would follow the division of the extensive triangle included between the finger, the probe, and the skin of the buttock. The bleed-which followed the use of the knife was not, however, alarming, and, the stricture having been freely divided and the opened sinuses plugged with oiled lint, the patient was removed to the ward.

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GREAT NORTHERN HOSPITAL.

PTOSIS, MYDRIASIS, AND HYPERÆMIA, WITH ANÆSTHESIA OF THE LEFT SIDE OF THE FACE, IN A SYPHILITIC PATIENT.

(Under the care of Mr. Spencer Watson.)

On the occasion of a recent visit to the above hospital, we had an opportunity of observing some peculiar nerve-symptoms in an out-patient under the care of Mr. Spencer Watson.

She is twenty-two years of age, and since April last has been under treatment for severe syphilitic periostitis of the tibiæ, which is only now yielding to large doses of iodide of potassium and cod-liver oil, varied, as occasion has seemed to demand, with tonic treatment. The pain is even now occasionally so severe as to require the exhibition, three times a day, of grain doses of opium. The cachexia has been extreme, and in the early part of her attendence led to a series of abscesses in the cervical lymphatic glands of the left side, of which the red and deeply puckered cicatrices now mark the neck. One of these is situated over the anterior edge of the sterno-mastoid, immediately below the angle of the jaw; and another lies in the anterior triangle, just in front of that muscle.

About four months ago she presented herself with the left side of the face affected in the following way: There was partial ptosis, and the pupil was widely dilated; the cheek wore a deep and permanent blush, and there was complete anæsthesia of the left side of the face and nose. The numbness first declared itself in the forehead, and gradually spread to the lower part of the face; and sensation is now returning in the order in which it faded, while the other symptoms are also abating. When we saw the patient the ptosis was scarcely perceptible, the mydriasis and blush were becoming less observable, and sensation was restored to the skin covering the lower jaw.

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CHARING-CROSS HOSPITAL.

EXCISION OF THE FIRST INTERPHALANGEAL JOINT OF THE RING-FINGER; RECOVERY, WITH FLEXION AND EXTENSION OF FINGER.

(Under the care of Mr. Bellamy.)

Resection is an operation rarely performed on any other digit than the thumb; in the present instance it not only saved the finger, but enabled it to recover a useful range of motion.

The patient was an ostler, twenty-nine years of age. The first interphalangeal joint of the ring-finger of one hand had become disorganised by inflammation set up by the bite of a horse. Bare bone was detected on introducing a probe into a sinus which led to the joint. The neighboring tissues, especially those on the palmar aspect, were healthy. The patient was in fair health, and very unwilling to undergo amputation. The ends of the phalanges were exposed by means of a straight incision, and removed, not without injury to the extensor tendon. A week after the operation, the wound having nearly healed, a curved splint of gutta-percha was adjusted to the palmar aspect of the finger. Every time that the dressings were renewed, the curve of the splint was increased, and the finger, having been first extended, was fixed into a position of increased flexion. On the occasion of the patient's last visit to the hospital, he possessed full power of flexion over the finger and considerable power of extension; according to his account, he could make very fair use of it.

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Provincial Hospital Reports.

BRISTOL GENERAL HOSPITAL.

(Cases under the care of Mr. Lansdowne.)

In reference to supposed cases of hydatid cyst in the prostate gland, Sir Henry Thompson, in Holme's System of Surgery, calls in question the fact of any such instance having been well ascertained. He surmises that they have been cases in which prostatic enlargement was closely simulated, though that gland was not affected otherwise than by pressure. In the following instance no evidence of hydatid formation could be traced, but it tends to confirm Sir Henry Thompson's opinion, as Mr. Lansdowne entertains no doubt that the prostate was not involved. He observes that, apart from the evidence gained by examination, it is *a priori* very improbable that the tough capsule of that gland would be distended to so great an extent as is here described.

Case 1. *Cyst between the Bladder and the Rectum causing Retention of Urine; Examination of the Contents; Recovery.*—G. W——, aged twenty-eight, was admitted for retention of urine, the cause of which could not be ascertained until the rectum was examined. In the situation of the prostate there was then found a large mass, of a bulging, conical shape, yielding a tense, elastic, and fluctuating feel. The finger could not be made to reach its upper limit, but as much of it as could be felt was larger than a closed fist, and in shape exactly corresponded to the prostate itself.

The introduction of a catheter was accomplished with considerable difficulty, arising chiefly from the existence of a large false passage. About five pints of urine were drawn off, and a second examination by the rectum discovered the swelling to be *in statu quo.* The man stated that he had had more or less constipation and difficulty of micturition and occasional retention for the last two months. On the day after admission it was noticed that the urine was thick and smoky from the presence of pus and blood. The use of the catheter was required night and morning. On the fourth day, the bowels having been previously acted on by medicine, the swelling was punctured from the rectum with a fine trocar and canula, and twenty-three ounces of a colorless, limpid fluid were drawn off. It was of a specific gravity of 1006, and contained a very small quantity of albumen. A number of squamous epithelium cells were the only formed products to be seen under the microscope; there was no trace of echinococci. A body having the shape, size, and situation of the prostate, obscured by wrinkled folds of mucous membrane, could still be felt. The bladder was evacuated naturally shortly after the operation.

In nineteen days the cyst had become refilled, and again gave rise to retention of urine, as well as to an aching pain in the sacral region—probably due to pressure on the sacral nerves—which extended down the thighs. He had a slight rigor; his pulse was 100, and his temperature 100·5°. The cyst was, in consequence, again tapped, and about a pint of most fetid flaky pus, which escaped with intermittent jets, was on this occasion drawn off. Some diluted Condy's fluid was injected into the cyst through the canula. In nine days more it had again refilled, and, at the most depending part, gave indication of a disposition to point. He had a hectic, anxious appearance, and a temperature above the normal standard. While at stool in the evening the cyst gave way, and emitted, according to the patient's statement, about a pint of most offensive pus. On the following day the pulse and temperature were normal. During the succeeding eight days a discharge took place from the rectum on each occasion of a motion being passed. The day after the cessation of this discharge, on the patient attempting to pass water, a quantity of fetid pus was discharged through the urethra; at first it came unmixed and in advance of the stream of urine, then mixed with it. It was presumed that a channel of communication had been formed between the cyst and the prostatic portion of the urethra. For more than a week longer the patient continued to discharge matter from the urethra, but his health improved so much that he soon was made an out-patient. To the last a bulky swelling, much smaller and harder than that at first felt, continued to obscure the prostate.

Case 2. *Excision of the Superior Maxilla for Epithelioma of the Cheek and Hard Palate.* Epitheliomatous growths had been removed from the patient's cheek on two separate previous occasions, twelve and eight months back respectively. The third recurrence was estimated to have been in existence about sixteen days before the operation in question was undertaken. In the middle of the cheek, and in the cicatrix of the former incisions, was an indurated swelling of about the size of a filbert, surmounted by a small sore, which presented the usual epitheliomatous characters. The greater part of the left hard palate was also involved; there was no apparent affection of the adjacent glands. The patient's general health was very good. For the operation, an incision carried from the external angular process of the frontal bone and round the tumour, skirting widely the diseased tissues, and prolonged to the angle of the mouth, was found amply sufficient. As the patient's breathing gave rise to serious apprehension towards the close of the operation, the floor of the orbit was removed for the sake of expedition; the whole of the left soft palate was also removed. The hæmorrhage was readily controlled by torsion. The wound was brought together by means of carbolised gut sutures and two hare-lip pins at the points of greatest tension. The patient was at first troubled with monocular diplopia, probably from loss of the attachment of the inferior oblique muscle. He made an excellent recovery.

(Under the care of Mr. Atchley.)

Extravasation of Urine in a Child following Rupture of the Urethra from a blow.—The patient was six years old, and, two days before admission, had been observed to suffer from irritation and slight œdema about the prepuce. On admission the scrotum, penis, and perineum were of a bright-red color, and very œdematous. A clear serous fluid exuded on puncture. The boy had passed no water for twelve hours. A catheter was passed without difficulty, but, at the membranous part of the urethra, a slight roughness was felt. There was no blood in the urine. Free incisions were made into the affected parts, and poultices were applied. On the second day the child seemed better, and passed water naturally. On the third day retention again occurred; a sensation of grating was felt as the catheter passed the neck of the bladder. In the evening he passed water in great agony. On the fourth day the integuments of the penis were found to be gangrenous; the incisions were sloughing, and a red and very tender swelling occupied the right pubic region. Further free incisions were made, and a charcoal poultice was applied. Quinine and opium were internally administered. Some urine was withdrawn through an elastic catheter. A few hours later it was decided to make a free opening into the bladder through the perineum. On the finger being introduced into the rectum, the elastic catheter, which had been tied in, could be felt immediately above the mucous membrane, moving freely in a large pouch. A straight bistoury was passed, in the middle line, into the catheter, and onwards into the bladder. Very trifling hæmorrhage occurred, and a large quantity of urine was liberated. The operation gave much relief. On the following day urine passed through the urethra, and through an irregular opening close to the perineum, but chiefly by the perineal opening. Two days later, on a slough being removed from one of the incisions in the pubes, an opening was exposed from which urine immediately welled up. There were thus four separate orifices of discharge. Rather more than three weeks later the child was in good health, and all the adventitious openings, with the exception of that near the perineum, which was rapidly contracting, had completely closed. Some time after he had been in the hospital it was discovered that, on the day on which he had first been brought to the hospital, he had fallen astride a crutch.

Medical Societies.

JANUARY—FEBRUARY.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Mr. Curling, F.R.S., President, in the Chair.

A CASE OF LARGE BILIARY CONCRETION IN THE ILEUM.

BY F. LE GROS CLARK, F.R.C.S.,

Surgeon to, and Lecturer on Surgery at, St. Thomas's Hospital.

The author gave the details of a case occurring in a patient aged fifty-eight, who was seized with abdominal pain and bilious vomiting, accompanied by constipation. A hard tumour was to be felt in the right hypochondrium. There was no abdominal tenderness or distension. On the eleventh day the vomiting became stercoraceous. Two days later the bowels were open and the vomiting ceased until ten days later, when it recurred, and continued at intervals during a week. For three weeks after this time the bowels acted daily, and there was no sickness. The patient was then seized with severe abdominal pain and vomiting; the abdomen was tender, especially over the region of the cæcum, where a hard tumour could be felt. Death took place two months from the commencement of the first attack. At no period of her life had the patient suffered from jaundice.—The post-mortem examination revealed the existence of extensive peritonitis. Two large gall-stones occupied the ileum close to the valve. An ulcerated opening in the small intestines had permitted the escape of several small gall-stones into the peritoneum. The gall-bladder was healthy; there was adhesion between it and any portion of the intestines. There was no trace of any ulceration either in the gall-bladder or in the neighboring intestines.—The concretions measured 1 in. in length and 4 in. in circumference. They seemed moulded to the shape of the ileum. On examination, it was found that the stones were composed of 95 per cent. of cholesterine, and that nothing had been added to them in the intestines. The gall-ducts were dilated and thickened. The author drew attention to the singular absence of any proof that these concretions had passed by ulceration from the gall-bladder to the duodenum, though this is the only way in which such large bodies could have entered the intestines.

Mr. De Méric asked what were the grounds of the diagnosis with regard to the cause of the obstruction. He had been consulted in a somewhat similar case, where, however, there was no post-mortem, so that he was left in the dark as to its exact causation.

Dr. A. P. Stewart remembered a patient in the Middlesex Hospital with obstinate stercoraceous vomiting. No relief could be obtained, and after death a gall-stone as large as any of those exhibited was found impacted in the ileum. It was smooth and rounded, and constituted a complete barrier to the passage. There were distinct marks of ulceration in the gall-bladder and duodenum. The year before last he had seen a lady with intense pain; suddenly she obtained rest, but again the pain came on, and again relief followed. Next day she passed a calculus the size of a pigeon's egg. It was almost certain this had passed by the ductus communis.

Dr. Habershon said such cases were interesting for their rarity and the difficulties of diagnosis. Some years ago he had a somewhat similar case in a lady over fifty years of age. She had pain and bilious vomiting, and died in ten days. They found a large gall-stone in her jejunum. There had been no peritonitis, and no stercoraceous vomiting. There were adhesions between the gall-bladder and the duodenum, but no regular cicatrix.

Mr. Le Gros Clark said there had been great difficulty in coming to any diagnosis, and that it was only reached by way of exclusion. There were no inflammatory symptoms, and no signs of malignancy, so he considered the obstruction due to scybala, and directed his treatment accordingly.

ON ETHER AND CHLOROFORM AS ANÆSTHETICS.

BY J. WARRINGTON HAWARD,

Assistant-Surgeon to the Hospital for Sick Children.

The paper commenced by stating that, it having been suggested to the author that the statements of Dr. Bigelow and other American surgeons showed that ether as an anæsthetic had been to our detriment neglected, he had, during the past year, practically investigated the subject, and had arrived at the conclusion that ether was, for several reasons, to be preferred to chloroform. Of these reasons, the strongest was the greater safety of ether, for by using it the chief, and in skilled hands probably the only, cause of fatal cases of chloroform inhalation was excluded—i.e., paralysis of the heart; ether being a stimulant to the heart's action, and uniformly improving the pulse. The second was that ether, from its stimulant quality, was antagonistic to the effects of the shock of an operation, which the author maintained, and quoted cases to show, was not abolished by rendering the patient insensible. A third was the greater liability of chloroform than ether to produce after-sickness. The principles and mode of administering ether were then described, and it was shown that if these were attended to, the production of anæsthesia by ether was as easy and as certain as by chloroform and required but little more expenditure of time or the drug. The only cases to which ether was not so applicable were operations upon the mouth, in which an inhaler could not be used, and where it was necessary to re-administer the anæsthetic as rapidly as possible without an inhaler. There were two appendices to the paper: the first consisting of a table of fatal cases of chloroform; the second, of a table of ninety-seven cases in which the author had administered ether, including amputations, excisions, perineal section, lithotomy, lithotrity, staphyloraphy, vesico-vaginal fistulæ, ligature of piles, and other operations. Especial note was taken of the occurrence of after-sickness, and the only approach to it was that in one case, after an operation for recto-vesical fistula, the patient vomited once, an hour after the operation.

The President regretted that no notice had been taken of the proposal of the Society's committee to use the mixed vapors of alcohol, ether, and chloroform.

Mr. Spencer Wells said the mixed vapors of ether and chloroform had been employed at Vienna, but it was found that the ether evaporated first, and that after it had gone the patient was drenched with the chloroform. He had long felt objections to chloroform, especially as regards the vomiting it produced. This was particularly objectionable in many of the operations he had to undertake, especially in the abdomen and vagina. He had tried ether on the advice of Dr. Keith of Edinburgh, but it was so troublesome that he was glad to take to bichloride of methylene. This he thought the best anæsthetic; sickness after it was rare. Some said it was only a mixture of chloroform and hydrochloric ether; that might be so, but as long as it suited his purpose he did not care what it was chemically. It was best given by Jünker's apparatus.

Dr. Day said he had frequently given bichloride of methylene for Mr. S. Wells, and was much satisfied with it. He entered at length into its chemical characters, and exhibited the apparatus referred to.

Dr. Sansom said the data available did not admit of an accurate estimate of the relative danger of chloroform and ether. He differed from the author alike in his estimate of ether and chloroform. He could not call the former safe. Relatively, chloroform was more manageable and less nauseous than ether. He considered the great danger of chloroform lay in its depressing action on the heart; but this could be avoided. It was true ether and chloroform did not go off together, but alcohol acted by restraining them both. By injecting morphia C. Bernard found that much less chloroform was required to produce anæsthesia. It was best to give morphia subcutaneously first, and chloroform afterwards, or to administer chloroform and alcohol together, or chloroform first and ether next, as he had first proposed some time ago.

Mr. Holmes said he had tried ether some years ago. It required twice the time chloroform did to produce anæsthesia; but this was an insignificant trouble if it were safer. He tried to favor the escape of the vapor by using a hot sponge, and it blistered the face of the patient. Violent convulsive movements were also induced by it in certain patients. Its relative safety was doubtful, for the statistics were more than suspicious. It was really impossible to avoid danger in the production of anæsthesia.

Mr. Brudenell Carter had tried ether on himself, and the taste of it hung about him a long time. This was an objection to its use.

Dr. Hunter said if they used other narcotics they must look to the lungs as well as to the heart. In one of these compound cases he had nearly met with a fatal result. It was best, he thought, to use morphia when the effects of the chloroform were passing away.

Mr. Clover remembered that sickness followed the use of ether when that anæsthetic was first used in this country. He had been in the habit of giving nitrous oxide first, and then ether, as the great difficulty was to get patients to inhale it freely. As to the giving of chloroform, the modes of giving it were of the utmost importance. He was convinced that a mixture of 4 per cent. of chloroform with air had a totally different effect from a mixture of 6 per cent.

Mr. Haward, in reply, pointed out that he was no special advocate for ether. He had tried it, and he had been struck with the results as being different from those commonly reported. The evidence was rather in favor of its safety. If, therefore, it could be easily employed, it would be well to use it; and he had shown that it could. The pulse really improved under its use. He had seen the combined fluids used; they produced excitement. No peculiar unpleasantness was left behind by ether.

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PATHOLOGICAL SOCIETY OF LONDON.

Mr. John Hilton, President, in the Chair.

Dr. Peacock exhibited a Heart which showed a very marked amount of disease of both the aortic and mitral orifices. The patient was a lad who before his death had had four or five attacks of acute rheumatism, and three of chorea. During life his aspect was so exceedingly cyanotic as to give rise to the strongest suspicion of congenital malformation of the heart. In addition to the valvular disease there was a considerable amount of general endocarditis, the endocardium being in places fully a line in thickness.

Dr. T. H. Green showed a case of Acute Interstitial Hepatitis. The patient was a boy aged nine, who was admitted into Charing-cross Hospital in July last. On admission he was suffering from jaundice. A few days after his admission his pupils were noticed to be remarkably dilated. This was followed in a short time by violent delirium, which continued without appreciable abatement till his death, which occurred ten days after his admission. At the post-mortem examination, a couple of enlarged lymphatic glands were found pressing upon the common bile-duct, and the connective tissue throughout the liver presented every appearance of recent inflammation. The stomach and intestines contained a considerable amount of blood, of which there had been no indication during life.

Dr. Murchison inquired if there was any elevation of temperature. Supposing the jaundice had not been present the cerebral symptoms would have made the diagnosis a difficult one. In cases of acute atrophy of the liver with cerebral symptoms the temperature is not raised.

Dr. Green replied that at no time was there any appreciable pyrexia; and, in answer to Dr. Bäumler, stated that no tyrosin had been found in the liver substance.

Dr. Murchison next called the attention of the Society to two cases of Hydatids of the Peritoneum, and exhibited a number of hydatid cysts which had been removed from one of these patients during life by Mr. Spencer Wells. The first case was that of a married woman aged twenty-nine. She was married at the age of nineteen, and continued in good health till three months after the birth of her first child, when she was awakened one night by severe pain just below the margin of the right ribs, near the ensiform cartilage. The pain was accompanied by tenderness, faintness, giddiness, and vomiting, and a swelling (which has remained ever since) appeared in the situation of the pain, about the size of a small egg. Her health now began to decline, and after the lapse of three or four years she noticed a swelling in the right iliac region, which has been gradually increasing, and about nine months ago noticed other lumps scattered over her abdomen. She had been liable to occasional severe paroxysms of general (not local) pain in the abdomen. She was supposed to be suffering from an ovarian tumour, and was sent to the Samaritan Hospital for treatment. The case being evidently not one of ovarian disease, Mr. Spencer Wells transferred her to the Middlesex Hospital, to be under the care of Dr. Murchison. At this time the pain in the abdomen was constant and severe, though there was nothing of the lancinating pain of cancer. The surface of the abdomen (which was greatly distended) presented to the eye a distinctly nodulated appearance at every part, which became still more distinct on palpation, and several round, movable, very elastic tumours, varying in size from a cherry to an orange, and giving distinct "hydatid vibration," could be felt. The hepatic dulness reached to the upper border of the fourth rib, but the lower border could not be defined, owing to the dulness given by the nodular mass. In the left flank (patient lying on back) there was tympanitic resonance. No jaundice; slight dyspnœa due to pressure, but no other pulmonary symptoms. The patient was suddenly, and without notice, removed from the Middlesex Hospital by her husband. She again went to the Samaritan Hospital, and Mr. Spencer Wells, finding no cyst large enough to make any difference by tapping, made a small incision, and removed three or four pounds of the hydatids, leaving at least as many more behind. The patient did well, and obtained by the operation considerable amelioration of her condition. The second case was that of a man aged forty-five, who for twelve years had suffered from the condition for which he was admitted into the Middlesex Hospital. His abdomen was much distended and nodular, the nodules being less numerous and bigger individually than in the case of the woman. The nodules presented all the physical signs of hydatid cysts. One small cyst was successfully tapped, and he was discharged at his own request.

Dr. Wilks mentioned a similar case as having occurred at Guy's Hospital, in which Mr. Bryant made an incision in the abdomen and successfully removed the hydatids. During the discussion it appeared that in Mr. Bryant's case the tumour consisted of one large cyst containing numerous smaller ones, and the operation was undertaken in the belief that the tumour was an ordinary ovarian cyst. In Mr. Well's case, however, the cysts were not enclosed in a parent cyst, and a correct diagnosis of their nature was made before the operation was undertaken. This, Dr. Murchison thought, constituted a distinct difference between Mr. Bryant's case and Mr. Spencer Wells's, which, he contended, was unique.

Dr. Wilks asked if Dr. Murchison considered that the hydatids had originated in the liver or the peritoneum.

Dr. Murchison replied that in the second case they probably primarily originated in the liver, but in the case of the woman their point of origin was doubtful. Mr. Spencer Wells did not see or feel the liver during his operation.

Mr. Hulke mentioned the fact that peritoneal hydatids were not uncommon in the pig.

Dr. Payne read a report of the Microscopical Examination of some Lymphomata taken from a patient who had died of Hodgkin's disease, with enlargement of the lymphatic glands throughout the body. Dr. Payne had made out that in the early stages of the disease there was an increase of the lymph elements of the gland, but that in the later stages the fibrous elements of the gland predominated, the true lymph elements being no longer in excess.

Dr. Goodfellow exhibited a Heart with an Aneurismal Pouch-like Dilatation of the wall of the Left Ventricle, close to the aortic valves.

Dr. Murchison also exhibited a similar specimen; in which the aneurismal pouch "grew" from the ventricular wall just behind the posterior flap of the mitral valve. The pouch itself was as big as a small orange, the orifice of communication with the ventricle being of about the size of a pea. The heart was otherwise healthy.

Dr. Moxon raised a question as to whether that form of suppuration of the kidney which was so often found post mortem as an accompaniment of stricture of the urethra, could, in cases where the stricture was successfully treated, be recovered from.

Dr. Pye-Smith exhibited a specimen of Cystic Dilatation of the Kidney. The patient from whom it was taken was a young man, a farrier by trade, who in the course of last summer was suddenly seized with a sharp attack of diarrhœa, and on the following day noticed for the first time an abdominal tumour, for the relief of which he went to Guy's Hospital. On admission, the left hypochondriac region of the abdomen was found to be filled with a solid mass, which was dull on percussion and gave a doubtful sensation of deep fluctuation. About two years previously the patient had been kicked in the left side by a horse, and had suffered in consequence from a slight attack of hæmaturia. The sudden appearance of the tumour seemed to point to hæmorrhage as the most probable cause of it. The patient died during the past month, exhausted by hectic and diarrhœa. At the post-mortem examination, the left kidney was found to be composed of a number of cysts communicating with each other, around which the true kidney substance was expanded. An old stricture of the left ureter was found an inch and a half below the pelvis of the kidney. Dr. Pye-Smith suggested that the ureter had probably been torn by the kick which the man had received two years previously, and, a stricture resulting, had thus given rise to the cystic dilatation of the kidney.

Dr. Dickinson could hardly conceive a blow on the side rupturing the ureter without causing serious and evident damage to the superficial structures. Calculus was by far the most common cause of this condition of kidney, and a calculus might very readily, and as a matter of fact often did, form and escape without the patient being in the least aware of it.

Dr. Hare objected to the use of the word "cystic" in this case. It was not a cystic kidney at all, but merely a dilated kidney. He did not agree with Dr. Dickinson that a calculus had been the probable cause of the condition in the case in question. This form of kidney was very common without calculus. He had seen it result from a simple twist of the ureter.

Dr. Thorogood also thought that these kidneys were of common occurrence without calculi.

Dr. Murchison remarked that Mr. Stanley had recorded cases of sacculated kidney resulting from injury. He thought that it was very unlikely that in this case the lesion of the ureter was due to a calculus, because the ureter was nowhere dilated.

Dr. Pye-Smith replied to the various observations, and consented to strike out the word "cystic" in his description of the kidney.

Mr. Squire exhibited a specimen of Tuberculous Ulceration of the Intestine, from a patient who had had also some tuberculous consolidation of the lung, for which he had been treated and apparently cured by the use of quinine.

Dr. Powell wished to know if any of the tuberculous deposit in the lungs was of recent origin. This point was important as bearing upon the curative or non-curative powers of quinine in such a case.

Mr. Squire thought that the lung mischief was none of it recent, but expressed his willingness to have the matter submitted to the judgment of experienced microscopists.

Mr. Hulke remarked that when a deposit of tubercle occurred at two distinct points, as, for example, in the lung and in a bone, it was very common, if one deposit was removed by surgical means, to get the deposit which remained increased and aggravated.

Dr. King exhibited a Heart the seat of considerable endocarditis, and a Spleen in which was an unusually large infarct. The patient from whom these organs were taken was admitted into the Middlesex Hospital in August last, suffering from weakness and dyspnœa. He had had an attack of acute rheumatism six years previously. On the 29th of August he was suddenly seized with violent pain in the left hypochondrium, and on the 29th of October he had an attack of apoplexy of which he died on November 4th. At the post-mortem, besides the diseased conditions exhibited, there was found extensive softening of the anterior lobe of the right hemisphere of the brain. No embolus was anywhere detected in the middle cerebral artery.

Dr. Leared quoted a similar case, in which the arteries of the circle of Willis were found very small and contracted, but no plug could be detected.

Dr. King, in reply to Dr. Bäumler, stated that no embolus was found in the splenic artery.

Dr. Murchison had seen many such masses occurring in the spleen and other organs, without any indications of embolism. The tendency to coagulation in these cases must be due to the blood state.

Mr. Hulke exhibited four specimens of Epithelioma. The first case was that of a large cauliflower mass growing from the arm of a man, aged sixty-four, a farm laborer. In early boyhood his arm had been crushed by a waggon, and, in conconsequence, there was a large scar on the arm, causing flexion and deformity. One point of the scar was always tender, and from it there grew a "wart," which he had been in the habit of occasionally cutting away with his pocket-knife. The epithelioma gradually grew. There were no enlarged glands in the axilla. The arm was amputated, and the patient did well. The second case was that of an epithelioma growing from an old scar on the back. The patient was a middle-aged woman. The scar was the result of a burn when seven years old. There were no enlarged glands, and no cachexia. The tumour was excised, and the patient did well. The third case—one of ulcerated epithelioma of the tongue—came under Mr. Hulke's care in 1867. The tongue was removed, but the cancer returned in the floor of the mouth, and the patient died. This patient's condition was of twenty years' standing. At first his tongue was covered with patches like wet washleather, and it was regarded as a case of ichthyosis linguæ. Three years later he was troubled with little buttony masses over the tongue, and these, when they got inconveniently big, he was in the habit of cutting with a razor. Ultimately the condition drifted into epithelioma. The patient was a healthy fireman. There was no history of syphilis. The fourth case was also one of epithelioma of the tongue, and resembled the former in beginning in the form of leathery patches, which the patient was in the habit of cutting with a razor. The tongue was removed by the écraseur; one of the old-fashioned many-stranded wire-cords was used, and absolutely not a drop of blood was lost during the operation. The patient made a good recovery. Mr. Hulke had brought these cases before the Society because he considered they tended to show that epithelioma was a local and not a constitutional disease.

Dr. Edis, from observations on epithelioma of the uterus, had come to the conclusion that it was strictly a local disorder.

CLINICAL SOCIETY OF LONDON.

Dr. W. W. Gull, F.R.S., President, in the Chair.

Dr. Baumler read a paper on Partial and General Idiopathic Pericarditis, in which he endeavored to prove that the white or milky spot on the surface of the heart frequently met with at post-mortem examinations has a clinical history of very transient acute pericarditis. He adduced in support of this proposition two cases, in which an acute illness, coming on with dyspnœa, with pain behind the sternum, radiating upwards to the larnyx, the left shoulder, and towards the left ear, and with slight febrile disturbance, was accompanied by a characteristic pericardial friction-sound, lasting, like the other symptoms, only for two or three days. In a third case, where the onset had been more gradual, the friction-sound was heard over a larger area; and there was also some distension of the pericardium by fluid; yet the whole attack was mild and lasted only a fortnight. Such intermediate forms link the very slight cases to the more serious ones, which more generally come under observation. Cases of idiopathic pericarditis being of rare occurrence, Dr. Bäumler appended the history of three other cases of this kind which had come under his observation. The three patients had been little girls from eight to ten years of age, and the pericarditis had come on in so insidious a manner that they had walked about with the pericardium full of effusion. One of them died; the two others recovered, one entirely, the other with valvular disease remaining. With regard to treatment, Dr. Bäumler particularly recommended the application of ice to the cardiac region, especially for its influence in reducing the number of the heart's contractions and in relieving pain.

The President remarked that, among other points of interest in connexion with this subject, it has been a matter of doubt as to whether genuine pericarditis, *per se*, ever causes pain, and that some so-called cases of the disease had proved to be cases of pleurisy, the friction-sound being due to a pleuritic rubbing immediately over the pericardium.

Dr. Douglas Powell asked Dr. Bäumler how the treatment of ice-bags could be reconciled with the intermittent application of linseed and mustard poultices.

Dr. C. T. Williams asked for particulars as to the influence of ice-bags on the pulse and heart.

Dr. Bäumler replied that, according to his knowledge and belief, the application of ice-bags acts antiphlogistically, reducing both pulse and temperature, and that the poultices were used, in the absence of other remedial agents, because in an empirical way they relieved pain.

Mr. Nunn read a paper on Lupus Erythematosus. This disease, known also as superficial lupus, was believed by Mr. Nunn to be essentially an inflammatory atrophy of the cutis, limiting itself to that structure, and thus distinguished from lupus exedens, which was capable apparently of destroying indiscriminately every structure. Two cases of lupus erythematosus were reported, in which the family history afforded no clue to the nature of the disease; and, in contrast, one case of lupus exedens, in which an hereditary syphilitic taint was, with almost complete certainty, to be traced. The first two cases had been treated for years before coming under Mr. Nunn's care with mercury, iodine, arsenic, &c. The first patient, a male, aged thirty-four, had (October, 1870) suffered during thirty-two years, the second during twenty-one years, with lupus erythematosus of the cheek. The bromo-iodine waters of the Woodhall Spa, in doses of a wineglassful three times a day, were given, and a tablespoonful of lemon-juice in a tumblerful of milk every morning. In the first case, the gums being spongy, a solution of chloride of zinc (one grain to the ounce of water) was ordered to be applied to them. This case was, to all appearances, cured at the end of six months. The second patient was still continuing the treatment with advantage, having only commenced it in May last. The case of lupus exedens had been in the Middlesex Hospital under the care of the late Mr. Moore, and was now an inmate of the Hospital for Incurables, at Putney.

Dr. Althaus thought the plan of giving large doses of these salts was too commonly practised, and that smaller quantities would, when used therapeutically, be quite as beneficial; and Dr. Gull remarked that this disease was so much an opprobrium to the physician that all hints as to its treatment were welcome and valuable.

Mr. G. Lawson related the particulars of a case of large Melanotic Tumour of the Eye, which had burst through the sclerotic and had extended into the orbit. He first excised the globe and then freely applied the chloride of zinc paste for the purpose of destroying all the tissues within the orbital cavity, and thus effectually to get rid of all the cancer-germs with which those structures are in such cases generally infiltrated. The operation was performed in July of this year, and the patient was now progressing favorably towards recovery. All the tissues within the orbit sloughed, and large portions of the bony cavity have exfoliated. Mr. Lawson remarked, that when the diagnosis of melanotic tumour within the eye is made at a very early stage of the disease, the simple removal of the eye is frequently sufficient. He quoted the case of a patient in whom he had been able to recognise the tumour by the ophthalmoscope when it was scarcely the size of a pea. He removed the eye, and now nearly three years has lapsed and there has been no recurrence of the disease in the orbit.

Mr. Campbell de Morgan, after referring to the case under discussion, said that the occurrence of epileptiform seizures after operation in which these caustics were applied was not uncommon. The fits usually come on a few hours after the operation, and rècur every twenty minutes, with marked diminution in the power of the pulse. The most severe case of the kind that had come under his care eventually did well; and in reply to Dr. Buzzard it was elicited that no recurrence of the epilepsy took place after convalescence from the operation.

The President queried whether cancer was a constitutional malady, and, indeed, hardly knew what constitutional maladies were. He believed cancer to be a local disease, but thought the question ripe for discussion.

Mr. Lawson, completing his original remarks, said that the tumour, when growing within the eye, is black, but becomes colorless after it has burst through the sclerotic coat.

Mr. Nunn was disposed to question the trustworthiness of statistics of operation in affording very positive evidence as to prolongation of life.

Mr. Henry Lee, remarking that he did not operate to cure, but to remove a hideous deformity, took occasion to demur to what had fallen from the President as to the constitutional nature of the disease.

A general discussion followed on this subject, in which Mr. De Morgan, Mr. Arnott, and Mr. Lawson contended that cancer was a local malady.

Mr. Hulke exhibited a blacksmith, from whom, in August, 1870, he had removed in the Middlesex Hospital, by excision and cauterisation, a very large Rodent Cancer of the left side of the face. It had eaten away the whole lower eyelid, and part of the upper one; it had also invaded the orbit and destroyed the eyeball, and opened the frontal sinus and nasal passages. The patient returned in August last with two small growths of the size of a threepennypiece. These were destroyed with zinc paste, and the whole surface now seemed perfectly healthy. There was now a large hollow, lined with healthy mucous membrane, showing the lower and middle turbinated bones, the openings into the frontal sinuses and nasal passages, and the antrum. This chasm could be hidden by a mask. Mr. Hulke said that the danger of operating in these advanced stages was over-estimated, and that with care the actual cautery and a chloride-of-zinc paste could be safely used, even to the roof of the orbit.

Mr. G. Lawson exhibited a patient from the Middlesex Hospital on whom he had operated successfully for a Rodent Cancerous Ulceration, involving the upper eyelid and extending into the orbit and on to the side of the nose. On account of the extent of the disease the eye was first excised, and then the whole of the diseased structure was removed with a scalpel. The bleeding having been arrested by the actual cautery, the chloride-of-zinc paste, spread on pieces of lint, was freely applied to the cut surface; a layer of cotton-wool was then laid over the parts, and the whole was kept *in situ* by a turn of a bandage round the head. The patient suffered comparatively little from the operation. The pain which she had was relieved by a subcutaneous injection of morphia. Large sloughs soon came away, and portions of the bony walls of the orbit exfoliated, and ultimately the granulating surface of the wound cicatrised. There was now a large gap, showing the upper portion of the nasal cavity and some of the ethmoidal and frontal cells; but the parts were all healthy and cicatrised, and there was reason to hope there would be no recurrence of the cancerous ulceration.

The President suggested that the faces of the patients should be photographed.

Dr. Moxon asked what was the structure of the indurated edge of the ulcer?

Mr. Hulke referred to specimens shown by him last winter at the Pathological Society, saying that it agreed with them, being essentially a cell-growth without a true intercellular substance, the cell masses lying in the interstices of a stroma, and pushing out beads into the surrounding parts.

Dr. Moxon also asked for the clinical and anatomical grounds for classing the ulcer with cancer.

Mr. Hulke replied that the clinical definition of cancer had different scopes; that, according to Virchow, cancer, anatomically considered, was a limited group of tumours, having essentially a structure similar to the edge of this ulcer; that in these ulcers cell-tissue was found graduating from a tissue, like the rete mucosum, of small, soft, round cells to the flat scales of typical epithelioma. He observed that, clinically speaking, it was customary to regard all tumours as cancers which infiltrate themselves at their advancing borders into the surrounding normal tissues, invading every tissue indifferently. In attacking all tissues alike, the local manifestations of rodent cancer are the same as those of other cancers, while in this case the absence of mischief in the glands, &c., showed only a minor degree of malignancy, and afforded no proof that the disease was non-malignant.

A conversational discussion ensued as to the composition of escharotic pastes, in which Mr. Hulke, Mr. De Morgan, Dr. Sedgwick, and Mr. Berkeley Hill took part, and united evidence went to prove that the pastes in common use were very manageable, and that the escharotic effects did not spread beyond the parts intended to be influenced.

Mr. De Morgan also remarked that the age of the patient seemed to have little influence over the success of the operation, and cited the case of a patient eighty-four years of age, in whom an entirely successful operation had recently been performed.

Dr. C. Theodore Williams related three cases of Phthisis, in which contraction and rapid obliteration of cavities had taken place. The patients were two females and one male, and their respective ages were fifteen, fifty-three, and twenty-seven. They had symptoms of phthisis for periods varying from six to twelve months; the disease being for the most part limited to the upper lobe of one lung, where unequivocal signs of a cavity had appeared. The first was a case of caseous pneumonia, where the cavity became obliterated in two months, and the patient has since remained free from cough for more than one year. In the second case, which, from the great prostration, excessive night-sweats, and aphthous state of the mouth, was regarded as unfavorable, the disease followed pleuro-pneumonia, and closure of the cavity was complete in three months. In the third case the patient had fistula, followed by scrofulous pneumonia; and a large tinkling cavity, involving the whole upper lobe of the right lung, formed, which became obliterated in two months, cavernous sounds being no longer detected. All three patients were free from family predisposition. They took cod-liver oil, with tonics, and enjoyed a liberal diet. Dr. Williams remarked that contraction had taken place with unusual rapidity in these cases; and the remarkable feature was, that it gave rise to little or no displacement of the neighboring organs, and to no marked collapse of the chest. He therefore concluded that the vacuum created by the shrinking of the cavities must have been supplied by an expansion of the lung-tissue round the cicatrix. The obliteration in two months of a cavity sufficiently large to give tinkling sounds was an exceedingly rare occurrence.

Dr. Habershon opined that the diagnosis of a cavity in the lungs was by no means an easy matter, and quoted Dr. Addison in proof of the assertion that no ordinary physical signs can determine the existence of a cavity satisfactorily. He was sure that vomicæ were much more rare than is generally supposed.

Dr. C. J. B. Williams made some general comments on the cases adduced, and said that in the third case quoted amphoric resonance or metallic tinkling existed in a very marked degree, and that these sounds can only be produced by a cavity; that cavernous sounds were quite distinct from those of bronchophony, and that the latter were much more diffused.

Dr. Moxon criticised the description and the mode in which Dr. C. T. Williams arrived at his diagnosis, and asked for a distinct explanation of the so-called "cavernous croak." He believed also that a single examination of the chest was not sufficient to diagnose the existence of a cavity, because, among other reasons, the tubes may be occupied by mucus for a considerable time. He also doubted if displacement of the heart could have occurred under the circumstances described by Dr. Williams.

Dr. Douglas Powell had no doubt as to the existence of cavities in these cases, and he had often seen marked enlargement of the opposite lung as a a consequence; and in post-mortem examinations had frequently found cavities in the lungs empty and flattened, though no adhesion of the walls had occurred.

The President commented upon the great importance of being able to diagnose accurately the existence of a cavity, and gave examples of errors committed by himself in this particular.

Dr. C. J. B. Williams, in reiterating his firm belief that cavities can be easily and accurately diagnosed, remarked that intensity of sound was deceptive, that vocal resonance in the trachea was not a trustworthy symptom, that loudness of voice did not of itself constitute pectoriloquy, and that the general history of the case should always be considered. He believed the "cavernous croak" to be caused by the sudden entrance of air into a hollow.

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MEDICAL SOCIETY OF LONDON.

Dr. Andrew Clark, President, in the Chair.

Mr. Spencer Watson showed a case of Traumatic Dislocation of the Crystalline Lens into the anterior chamber. Glaucomatous symptoms were set up, with very severe pain and intra-ocular tension. The lens was extracted through an incision made in the ciliary region of the sclerotic at the lower and outer side. The result was good.—Mr. Watson also exhibited a case of Congenital Displacement of both Lenses, in a patient aged fourteen. When the pupils are natural in size the irides are tremulous, and when the eyeball is moved inwards the pupils become oval, and the plane of the iris near the pupil is obliquely inclined on the inner side as if from the lens pushing it forwards; at that part there is a slight divergent squint, and the child's aspect peculiar. The father's sight is defective, but that of the mother and of a younger child is good.

A very ingenious method for applying dry cold to the eye was shown by Mr. Watson. Ordinary india-rubber air-balls were filled with water, placed in a basin containing iced water, and applied to the eye, being changed at intervals. Different sizes could be used, and they adapted themselves well to the part.

Mr. Victor de Méric narrated a case of Fibroid Phthisis depending on Traumatism. The patient, aged forty-five, was forcibly raised from the ground by an assailant, who attempted to throw him into a wine-vat. In this act some of the lower ribs on the right side suffered fracture near their angles. In a few days the symptoms became alarming: hurried breathing, severe pain in the left side of the chest, restlessness, anxious countenance, and abundant expectoration. Shortly afterwards the patient was in a typhoid state, the whole of the left side of the chest being dull on percussion, and offering no respiratory sound whatever. It was concluded that the left pleura had been injured either from pressure or shock, that effusion had taken place, and that the case was one of acute pleuro-pneumonia. Signs of phthisis ultimately showed themselves, and the patient died. At the post-mortem examination the evidences of old-standing pleurisy, with effusion and also recent phthisis, were found. It was believed that the pleuro-pneumonia did the mischief, and originated the phthisis.

Dr. Douglas Powell was unable to understand why this was called a case of phthisis. He would consider it, from an examination of the specimen and the clinical history, a case of pneumonia in the grey stage.

The President remarked that it was not a case of what he called fibroid phthisis, which would present ulceration and suppuration of a more or less circumscribed non-malignant deposit in the lung. If there was no cavity, there was no phthisis. The specimen was an excellent example of the hard form of grey pneumonic consolidation. Some bronchi becoming blocked by exudation, fluid collected in them and gave rise to gurgling.

Dr. Lichtenberg communicated two cases of Rhinoplastic operations. Mrs. K——, the first of the two patients, had been for some time under the treatment of Mr. Bäumler, at the German Hospital, as an out-patient for tertiary ulceration of the nose, and, being discharged cured, was sent to him for surgical improvement. On examination he found that the septum, the entire left ala, and also almost the entire right ala were destroyed. She was admitted into the Tottenham Training Hospital, and operated upon on 23rd March, 1871. He took two flaps from the forehead, but unfortunately he was under the impression that he could not obtain a sufficiently large flap without encroaching considerably on the scalp, and he merely formed the two alæ with the intention of providing the septum from the upper lip at a future time; but he found that the nostrils, or rather the nostril, contracted to such a degree that he did not consider it warrantable to produce a new deformity with the doubtful prospect of a tolerably good septum. The second case came under his treatment in the Tottenham Hospital, and was useful in showing what large doses of iodide of potassium could do when given in these particular cases. The patient had been at different hospitals without deriving any benefit, but directly this remedy in large doses was given the improvement was well marked, and the ulceration rapidly healed. He waited some months to see whether the disease was thoroughly subdued, and operated on the 6th April, 1871. The whole nose, with the exception of some rudiments of nasal bones, was destroyed, as was shown by a drawing made before the operation. He therefore had to form a very large flap from the forehead, but the skin contracted more than he anticipated, and he thought that in these cases the flap can scarcely ever be too large provided it can be made with safety to the patient, and without creating a larger deformity in order to improve a smaller.

Mr. Francis Mason remarked upon the interest of the cases; none could help admiring them. When the pedicle was taken from the centre of the nose, the supply of vessels was scanty. Was the flap applied to a raw surface or to an ulcerating one? In a case at the Westminster Hospital he had made three small flaps, one above the aperture and the others from each side of the aperture, turned these over the opening, and laid the flap forming the nose, and taken from the forehead, over them. After his discharge some slight ulceration had taken place at the tip of the nose. Now the nose was broader, and not so symmetrical as those shown.

Mr. Thomas Bond then read a paper "On Urethral Rheumatism." His deduction was that urethral rheumatism is a slow form of pyæmic poisoning, due to the local disease of the urethra, and that when the discharge is thoroughly and permanently stopped, the rheumatism may soon be cured, and has no tendency to return, except by access of a fresh urethral discharge.

Editorial.

TREATMENT OF HYPERPYREXIA.

Most medical men, we feel sure, have studied with intense interest the important cases lately detailed by Dr. Wilson Fox in the columns of this journal; and to the few who have not read them we recommend their immediate perusal. These papers form the record of a genuine success in positive therapeutics—a success by which a hitherto most intractable and fatal phase of disease has been shown to be essentially curable. During the last few years we have been informed (by Wünderlich, Sidney Ringer, Murchison, Wolff, Herman Weber, and others) of a mode of termination to which several acute pyrexial diseases occasionally tend, and in which the occurrence of a certain excessively high degree of temperature, rapidly produced, appeared to be an infallible death-warrant of the patient; in fact, Wünderlich concluded that it was a part of the process of death, for not only did the fatal termination always ensue with great rapidity, but the temperature was observed to go on rising for some time after life had ceased. The phenomenon had been observed to occur especially in acute rheumatism, but also in various other inflammations and fevers, in delirium tremens, &c. Dr. Wilson Fox thinks the balance of evidence from recorded cases shows that any temperature above 108° Fahr., especially if suddenly reached and still increasing, might be said to indicate a surely fatal result, in the absence of the one form of treatment which has now proved successful—the continuous cold-water treatment. Dr. Wilson Fox's cases now recorded are two of rheumatic fever: in one the temperature reached 110°, yet a week later the patient was sitting up in bed and eating fish, and was discharged convalescent three weeks afterwards; in the other, the maximum temperature reached was 107·3°, but it was rapidly rising at the time when it was stopped by the commencement of the cold-water treatment, and would probably otherwise have gone fully as high as in the first case, the general symptoms being exceedingly bad, and the complications most numerous and dangerous. Convalescence was fully established on the sixty-first day.

It happened, moreover, that by a coincidence, of which Dr. Wilson Fox was not at first aware, a foreigner, Dr. Meding, had (in 1870) also tried the cold-water treatment successfully in a case of sudden hyperpyrexia, where the temperature had gone up to 108·6°. Additional strength is thus given to the facts brought forward by Dr. Wilson Fox, and there can no longer be any doubt that it is the duty of any medical man who sees a patient reach a temperature at all considerably over 107° Fahr., at once to entertain the cold-water treatment as giving a very hopeful chance for the patient's life.

It would be impossible here to indicate, even ever so briefly, the importance and suggestiveness of this modern confirmation (under the sanction of the most accurate scientific observation) of the old and too culpably neglected doctrines and practice of Curric. Since attention was first drawn to these phenomena of rapidly fatal hyperpyrexia in acute disease, much controversy and speculation have taken place respecting the significance of the fact. Was the excessive temperature itself the cause of the fatal result (super-heated blood being poisonous to the life of the nervous centres), or the mere expression of a sudden increase in the violence of the disintegrating processes going on in the nervous centres, which was itself the cause of death? or were both these factors at work? Under these circumstances, the practical answer given by the success of the cold-water treatment is of great pathological as well as therapeutical interest, since it is plain that the mere abstraction of heat was sufficient to change the whole course of the symptoms, and especially to subdue the coma or delirium, which are of the worst possible prognosis. It therefore seems probable that a certain superheating of blood, producible from causes acting within the body, may of itself cause death. Another thing that is very striking is the total powerlessness of bleeding (as tried in Dr. Ringer's cases) and of the most powerful drugs to effect the reduction of temperature, which was at once accomplished by the cool plunge-path, and kept up by other minor means of removing external heat. And even more remarkable is the crucial evidence afforded by the histories of these cases of Dr. Wilson Fox's that the tendency to grave internal complications—pneumonia, pleurisy, pericarditis—is no whit increased by so seemingly formidable a treatment as that by pailfuls of ice-water repeatedly poured over the body, or keeping the patient immersed for three hours at a time in the cold bath or in the iced pack, such measures being frequently repeated.

Now that we know, not only (as Liebermeister and others have abundantly proved) that many febrile diseases may be treated, at least as successfully as in any other way, by the cool bath, but that cool baths, cold affusion, &c., are the only treatment for an otherwise inevitably fatal phase of acute diseases, there is one very plain duty before us. Should the results of the treatment employed by Dr. Wilson Fox be repeated in the hands of other physicians, no hospital or infirmary will have the right to remain any longer without the amplest machinery for applying every variety of the cold-water treatment. We may say, emphatically, that this is one of the greatest questions of the day, and it must be immediately followed out in this country by experiments on the largest scale.

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THE NATURE OF VIRUS.

That a considerable number of diseases—embracing amongst them some of the most important and of the most frequent occurrence—result from the action of a virus or poison, few or none will be disposed to deny; but if the question be asked, what is the nature of a virus or the material that enables the disease to be communicated from one animal to another? the answer given by the best instructed members of the profession would, we apprehend be far from unanimous. It is well known that in all virulent fluids a clear fluid or serum may be discerned in which certain morphological elements, molecules, nuclei, cells, and débris of cells, are discoverable. The question is, whether the poisonous agent is disseminated uniformly through the fluid, or is a special quality belonging to and characteristic of the corpuscles. This question, interesting in many points of view to the practical surgeon, M. Chauveau has undertaken to investigate, and has given the results of his inquiries in a series of lectures delivered before the Société des Sciences Médicales de Lyon, and reported in the *Revue Scientifique*. He states in the most unequivocal manner that the virulent property is associated, not with the fluid, but with the corpuscles held in suspension. This opinion he bases on the following facts: first, that, if virulent fluids are diluted with water or other inert material, and their activity be then tested by inoculation, it will be found that they act, not as if they were uniformly diffused through the fluid—that is, not with diminished virulence only, as though weakened by dilution,—but irregularly, and as though the poisonous quality were limited to particular molecules dispersed here and there through the fluid, and the more distant from each other in proportion as the fluid is more diluted, some of the inoculations failing altogether, whilst others are as energetic as usual; and secondly, that, if the plasma or serum be carefully separated from the molecules and cells, the former proves innocuous when introduced into the system, whilst the latter retain in a high degree the poisonous properties. The difficulty of isolating the corpuscles and cells from the plasma is much greater than might at first sight be imagined. It is impossible to effect it satisfactorily by merely straining the fluid, for in this operation it may always be objected that a part of the plasma still remains adherent to the cells, and accounts for their active properties. M. Chauveau advances the various reasons against the employment of dialysis, and, in order to obtain the cells as pure as possible, proceeds in the following manner. The purulent fluid (and he prefers the fluid obtained from the pulmonary abscesses of horses affected with glanders, as possessing an extraordinary degree of activity and virulence) is first washed with ten times its bulk of water, and, after standing for two hours, the superjacent fluid, containing cells and molecules free from coarser fragments, is strained through filtering paper. The material left in the filter is washed over and over again with many hundred times its bulk of water, and allowed on each occasion to precipitate to the bottom of the vessel. By these means, which occupy nearly forty hours, he considers he has withdrawn or washed away all the adherent plasma. But if a small portion of the mass of the corpuscles, leucocytes, mucous globules, proliferating cells, columnar epithelium, and granulations, all more or less swollen with water, be introduced with a lancet into the cheek of a perfectly healthy horse or ass, the initial symptoms speedily become apparent, and demonstrate in the clearest manner that they constitute the true poisonous agents. The means adopted by M. Chauveau to discover which of the morphological elements are to be regarded as the active agents we reserve for consideration.

Medical Annotations.

"Ne quid nimis."

FISSION OF RED BLOOD-CORPUSCLES.

Dr. Laschkewitsch of Charkow places on record, in the last part (Heft iii.) of Stricker's *Medizinische Jahrbücher*, a case of Addison's disease in which he obtained the remarkable phenomenon of fission of red blood-corpuscles. The patient was forty-five years of age, very anæmic, and suffered from considerable debility, with headache, palpitation of the heart, and dyspnœa. The size of the spleen, as ascertained by percussion, was somewhat increased, and there was a little sibilant rhonchus in different parts of the chest, but few other indications of disease. The patient had been perfectly well up to a year previously, when he suddenly fell ill with vomiting and diarrhœa. He became very feeble, and several similar attacks occurred in the course of the year, his memory at the same time failing. On examining the blood-corpuscles under the microscope, they appeared paler and larger (flatter) than usual; and he observed that they changed their form whilst under examination. They presented biscuit, club, and vermiform shapes; thrust out one or more processes and again withdrew them; constrictions occurred, and ultimately portions separated off entirely. The addition of a little weak acetic acid considerably accelerated the rapidity with which these changes took place. Finding these relations interesting, he experimented on the corpuscles with heat and electricity; but neither these nor the addition of quinia produced any alteration in the phenomena witnessed. He was able to obtain Teichmann's crystals from the blood, but they were few in number, small, and pale. In the course of three months the bronzing of the skin of the face, neck, and hands became well marked.

Fission of the red corpuscles has been noted by Friedrich as occurring in a case of leucæmia.

INSTRUMENTS FOR DISTINGUISHING LEAD FROM BONE.

In the accounts of the Dublin trial much stress has been laid upon the doubts that rested on the minds of the surgeons as to the presence of the ball, notwithstanding that Nélaton's porcelain probe and other instruments were used to distinguish between bone and lead. Our readers are probably aware that apparatus for utilising galvanism have been recommended, such as those of M. de Wilde and of Messrs. Krohne and Sessemann. We have heard from some army medical officers of a very simple and ingenious method of attaining this end, the invention of Mr. John Taylor, of Kensington. The instrument consists of a pair of bullet forceps, with blades electrically insulated from each other at the joint, and connected by means of wires with a minature pair of galvanic plates, one of the wires being coiled round the case of a compass needle, thus constituting a small galvanometer. On the pocket instrument case to which this is attached being opened, and so arranged that the needle stands parallel to the wires, the moment any metallic body is touched by the blades of the forceps, indication is given by a deflection of the needle, enabling the operator to know with certainty whether it is the proper object or a piece of bone he is touching. We may further state that this apparatus is inexpensive, and that it is contained in a common pocket case. The voltaic plates are only two inches square, and when wrapped in gutta-percha tissue their activity is preserved for weeks, and is easily renewed at any time by remoistening the woollen cloth between them with a few drops of acidulated water.

A NEW FORM OF SENSITIVE FLAME.

Mr. Philip Barry, of Cork, sends the following account of a new and very beautiful sensitive flame to Professor Tyndall, by whom it has been published in *Nature*. It is, in Mr. Barry's experience, the most sensitive of all sensitive flames, though, from its smaller size, not so striking as Professor Tyndall's vowel flame. It possesses the advantage that the ordinary pressure in the gas mains is quite sufficient to develop it. The method of producing it consists in igniting the gas (ordinary coal gas) not at the burner, but some inches above it, by interposing between the burner and the flame a piece of wire gauze. With a pressure of 7–10 lb. at the burner, a piece of gauze seven inches square may be placed on the ring of a retort stand, at a height of about two inches above the burner. The gauze may be ordinary window-blind wire gauze, with about thirty-two meshes in the lineal inch. The burner should be Sugg's steatite pin-hole burner, the same as used for vowel flame. The flame is a slender cone about four inches high, the upper portion giving a bright-yellow light, the base being a none-luminous blue flame. At the least noise this flame roars, sinking down to the surface of the gauze, becoming at the same time almost invisible. It is very active in its responses, and, being rather a noisy flame, its sympathy is apparent to the ear as well as to the eye. To the vowel sounds it does not appear to answer so discriminatively as the vowel flame. It is extremely sensitive to A, very slightly to E, more so to I, entirely insensitive to O, but slightly to U. It dances in the most perfect manner to a small musical snuff-box, and is highly sensitive to most of the sonorous vibrations which affect the vowel flame, though it possesses some points of difference.

CONDURANGO.

We have received from Mr. Davidson, the house-surgeon of the Middlesex Hospital, a short paper on the treatment of cancer by the internal administration of the decoction of condurango. The results plainly show that condurango is as futile as any of the cancer cures which have preceded it. It was used in four cases—(1) in an ulcerated epithelioma of the roof of the mouth; (2) in primary cancer of the penis, and secondary injection of the lymphatic glands in both groins; (3) in an ulcerated epithelioma of the scrotum; (4) in an ulcerated scirrhus of the female breast. In all of these cases the condurango had positively *no effect* upon the progress of the disease.

Correspondence.

"Audi alteram partem."

COLD-WATER TREATMENT OF FEVERS.

To the Editor of The Lancet.

Sir,—Will you kindly give space in your columns for the following letter, which may, I hope, be the means of eliciting from our leading physicians their views relative to the advisability or otherwise of the cold-water treatment of fevers.

I have had under my care, during the last few years, a great number of cases of typhoid and rheumatic fever, all of which I have treated in the general way, and have been fortunate enough not to lose any.

Some few weeks ago I had a very severe case of typhoid fever in a young lady, who, by-the-by, was also suffering from severe mental shock, having lost her sister in Wales from the same fever. My patient's symptoms were, it is true, very severe and unfavorable from the first, her temperature being, from the first day I saw her, 104·5°, and sometimes in the morning 105°. She had violent delirium. Pulse varying from 125 to 145 in the minute.

At a consultation, it was decided to adopt the cold-water sponging, give stimulants, quinine, &c. After the first twenty-four hours the temperature had decidedly decreased, and the pulse was quieter. The sponging was continued four or five times a day, the temperature still falling till it got to 101·5°, and I held out hopes to the friends that the symptoms were more favorable. Two days after the sponging was commenced, pneumonia set in, and the following day, the fourteenth of the disease, she died.

At the same time I had another case of typhoid fever, in a man; temperature 104·5°; pulse 130–140 per minute. I ordered him the cold-water sponging. Two days afterwards, on examining his urine, I found it contained large quantities of blood. I immediately left off the cold sponging and substituted hot hip-baths, and gave antimonial wine with acetate of ammonia; the hæmaturia disappeared, and the patient rapidly recovered.

In both the above cases it is but fair to say that the patients themselves liked the cold sponging.

I think, Sir, you will agree with me that the above cases must give rise to grave doubts as to the advisability, at any rate in private practice, of adopting the cold-water system. It suggests to me the following questions, which I shall be glad if anyone who is acquainted with facts will answer—viz., Is the cold-water system adopted at the fever hospitals? if so, is the rate of mortality less now than before it was commenced? Is the rate of mortality lower in Germany—where, I believe, the cold-water treatment has been in use for some years past—than it has been in England? or is the fever appreciably lessened in intensity or duration?

In the two cases I have roughly sketched, the first I admit to have been as bad as it possibly could be, the patient having from the first made up her mind to die, and the disease making rapid strides from the very onset. In this case I do not mean to say that the cold water hastened death, but, although it lowered the temperature, it certainly did not prolong life. In the second, the urine was perfectly free from albumen or blood until the cold water was commenced, and although, as in the first case, the temperature was lowered, the general symptoms were aggravated, and hæmaturia supervened. This may have been a coincidence perfectly independent of the cold water, but it was, nevertheless, the fact.

Having put this subject fairly before the public, I shall be perfectly contented if it gives rise to a fair and impartial discussion in your columns; and let me, in conclusion, beg any of your readers who may have had cases similar to the two I have recorded, not to hesitate to publish them, as it is only by looking at both sides of the question that one can arrive at a fair estimate of the value of any method of treatment.—Believe me, Sir, yours truly,

F. P. Jessett.

Pier-road, Erith, October, 1871.

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WELL-OILED URETHRA *vice* WELL-OILED CATHETER.

To the Editor of The Lancet.

Sir,—The plan which I am about to describe is so simple that possibly it may have occurred to many others besides myself; but as I have never heard of it being tried, I venture to ask you to spare me a small space to record it.

In cases of retention of urine in the male, we are advised by some authorities to use a well-oiled catheter, by others to anoint the catheter with cold cream, &c.; yet it has seemed to me that, however carefully the instrument may be greased, the dry irritable urethra wipes off the greater part, if not the whole, of the oily material from the most important part—viz., the point—long before it reaches the stricture or other impediment, and that dry point, not passing easily over the equally dry surface of the urethra, is apt to be impeded in its transit, even before it reaches the stricture, by folds of mucous membrane, &c. It struck me some time since that the injection of oil into the urethra would meet the difficulty better than oiling the catheter, and on trying it I was very much pleased with the easy way in which the catheter glided down the canal as far as, and after a short time through, the stricture, which had previously given me considerable trouble, and my patient a corresponding amount of suffering.

My plan is to fill an ordinary glass syringe with oil, and inject as much as possible into the urethra, being careful to compress the end of the penis firmly between the finger and thumb of the left hand while injecting, and after withdrawing the syringe, until the point of the catheter is inserted; then to proceed in the usual way. If careful compression be neglected, the elasticity of the urethra drives out a great part of the oil, and one great advantage, that of distension of the canal, is lost.

The advantages which I claim for my method are: that the urethra is cooled and rendered less irritable by the oil; that, by being distended, any folds of mucous membrane which might otherwise catch the point of the catheter are kept out of the way; and that the interior of the stricture itself, which by the ordinary plan must receive a very homœopathic dose of oil (and one is naturally desirous of excluding homœopathy from surgery), is treated to as free a supply of the lubricating fluid as its calibre will allow. I have found some of the oil in the urine in a case which was impermeable to the catheter, and in which it became necessary to relieve the bladder by the trocar.

The comfort expressed by patients and the saving of time to one's self are not the least among the reasons which have induced me to trespass so far on your valuable space.

Yours faithfully,
A. E. Barrett.

Grimston, Lynn, Norfolk, Oct. 30th, 1871.

ALBERT AND STRICKER'S RESEARCHES ON TRAUMATIC FEVER.

In the *Aerzltiches Literaturblatt* (No. 6, 871) is a short article on Pathology, of which the following is a translation:—

The last phase in the development of the theory of fever has been an essentially humoro-pathological one. Proceeding from the results of injuries (septicæmia), which strongly support this view, Billroth has taught that simple traumatic fever originates by resorption of the products of inflammation; whilst Bergmann has still more recently advanced the statement that the fluid products of the disintegration of the tissues will produce fever if they are suddenly introduced into the blood in considerable quantities, as after copious blood-letting. The experimental evidence in favor of Billroth's view is the fact that pus of good quality introduced into the blood induces fever. Stricker and Albert corroborate this statement, and find by their method (they measure the temperature in the rectum of the animal at liberty) that the elevation of temperature undergoes a determinate, typical, or periodic change. They adduce a series of control experiments that may here be briefly recorded, the temperature being given in centigrade degrees.

Experiment.	A.	B.	C.	D.
Injection of	2 c. c. of pus	20 c. c., ½ per cent. of cina. solution	20 c. c. of water	Nothing done beyond ligature of the vein.
Temperature prior to injection	39·6°	39·3°	39·0°	39·0°
Rise in Temperature after so many minutes	45′ + 1·5°	32′ + 1·09°	40′ + 1·1°	During the operation, + 1·3°
Alteration of temperature after so many more minutes.	33′ — 0·6°	48′ — 0·49°	2 hs. & 50′ + 0·6°	1 hour — 0·6°
From thence onwards rose		After 60′ + 1·2° ...	... ...	24′ — 0·4° 1 hour 46′ + 0·7° 52′ + 0·6°

To this it may be added that the phenomena observed in experiment D are not constant, since in four animals in which the vein was merely tied, the temperature did not rise in two during the first hour. This control experiment, however, is very valuable, since it shows what may happen when nothing whatever has been introduced into the circulation. Nevertheless the great variation in temperature in A and B during the first two hours is not to be referred to the external injury, but must be considered as the effect of the substance injected (pus). As evidence that the injected fluid produces directly an elevation of temperature the authors refer to the regular elevation of temperature occurring in the course of the second half-hour, previously to which it rises slowly, but then rapidly—one-tenth of a degree Cent. in two minutes, or even in one minute—and then again becomes more gradually elevated. In animals kept firmly bound down this order of events, however, is not observed. During the rise in temperature the animals tremble violently, but when the highest temperature is attained the animals remain perfectly quiet. The rise in temperature, after the lapse of an hour and a half, cannot be taken into consideration, since it occurs in animals that are simply tightly bound down. In one experiment the injection of four-fifths of a cubic centimetre of fresh pus caused an elevation of 2° C. But since so small a quantity of an indifferent fluid would not occasion anything like such an elevation, the question arises whether the intensity of the action is referable to any specific quality of the pus, or depends on the embolism which infiltrated pus always produces. To determine this, one cubic centimetre of water rendered milky by starch was injected. Within forty-three minutes an elevation of 1·6° C. occurred. The form of the curves in the two experiments was similar, and we may, therefore, ascribe the rise of temperature which occurs after the injection of small quantities of pus to the embolia produced. In order to obviate the objection that the injection of large quantities of indifferent fluids causes the breaking down of the blood-corpuscles, and that these operate like the products of the disintegration of the body generally as pyrogenic agents, or that the loss of blood owing to the injection occasions increased absorption of parenchymatous juices into the vascular system, Messrs. Albert and Stricker combined experiments in which the mechanics of the injection process were followed without any considerable quantity of foreign fluid gaining entrance into the circulation, or blood being lost. They ligatured the arteria and vena cruralis with canulæ, and a short piece of india-rubber tubing, which was filled with solution of carbonate of soda, and allowed the blood to pass from the vein directly into the artery only for twenty or thirty seconds. In two such experiments made with all precaution the temperature rose in the second half-hour after the self-injection 0·9° C., and 1·35° C. Here the mass of the blood was unchanged whilst the blood-pressure in the veins augmented, so that there was no reason for supposing there was an increased absorption of parenchymatous fluids; nevertheless fever followed quickly after the transfusion.

MM. Albert and Stricker have also made control experiments in reference to subcutaneous injections, and have found that, for example, the injection of four cubic centimeters of spring water increased the temperature of a young dog two hours after the injection to 40·3° C., or 0·5° above its normal maximum, and six cubic centimeters of healthy rabbit's blood caused an elevation, after two hours, of 0·4°, and an abscess formed. In rabbits, which the authors consider to be available for such experiments, the action of spring water is still more plainly shown. The elevation of temperature is therefore plainly not due to any special action of the pus. So also with the febrile symptoms which Weber excited by injection of pus and other fluids, as blood, into a serous sac, it is clear that they can be produced in the same manner, and are not in any way due to pus as such. Weber has stated that the injection of the blood of dogs in a state of fever excites fever when injected into the veins of a healthy dog. Albert and Stricker repeated the experiment, so far modifying it, however, that they withdrew blood from the sound animal to fainting, and then refilled the vessels by direct injection from the feverish animal. By this means a much larger proportion of blood from the feverish animal was introduced. In this experiment the temperature first fell considerably, and then, after six or eight hours, rose to a great height —that is, the symptoms observed in the above-mentioned simple experiment were augmented in intensity, but MM. Albert and Stricker do not think that on this account there are any grounds for believing that the blood of feverish animals, when injected into a healthy animal, will excite fever.

Foreign Gleanings.

PROFESSOR SCHIFF'S EXPERIMENTS.

In looking over Italian periodical medical literature one is struck by the occurrence of the German names of several professors attached to universities, as Moleschott, Oehl, and Schiff. In fact, these are all eminent German physiologists whose labors had rendered them so conspicuous that they were offered chairs in the Italian seats of learning. Among these accomplished men Schiff is particularly conspicuous for the skill and patience with which he performs experiments tending to solve physiological problems. The Scientific Institute of Florence is specially adapted for such investigations, the latest of which were undertaken to test the following opinion recently published at Berlin. The anterior part of the brain, it is contended, possesses motor irritability; when stimulated muscular movements are excited on the opposite side of the body. Hence it was concluded that the anterior portion of the brain possesses motor properties. Professor Schiff's experiments have just confirmed this; but he has found that the muscular movements do not arise from an actual motor property of the brain, but are reflex movements excited by the irritation of certain parts of the cerebrum which regulate tactile sensibility (not sensitiveness to pain). The removal of such parts does not *directly* abolish the movements, but disturbs them *indirectly*, through the destruction of tactile sensibility. *L'Imparziale* of Sept. 1st, 1871, promises to publish a detailed account of Professor Schiff's experiments.

PROFESSOR CROCQ OF BRUSSELS ON CHOLERA CONTAGION.

The question of cholera is at present one of so much importance that all documents on the subject coming from recognised authorities, and bearing a practical character, must be noted with great care. Professor Crocq of Belgium, vice-president of the Brussels Academy of Medicine, has just communicated to the Paris Academy of Medicine the results of a series of experiments which he has carried on upon animals with the object of testing whether the alvine evacuations constitute the true vehicle of the choleraic virus. In all of the animals he has succeeded in producing most of the symptoms of cholera, and he finds that the alvine evacuations indeed constitute the vehicle of the virus, not, however, as was suggested by Pettenkofer, through a kind of fermentation, but because of the immediate presence of the virus in the evacuations. M. Crocq draws the following inferences from the results of his researches :—

1. Cholera is contagious, and is transmitted by a virus, the vehicle of which is the alvine evacuations.
2. The virus may manifest its effects even less than twelve hours before any evacuation of matter.
3. The period of incubation may be only of two hours; it may continue over one or two days, and even more.
4. All the subjects do not show a like predisposition to undergo the effects of the choleraic poison, receptivity may even be altogether absent.

TUMOUR OF THE PINEAL GLAND AND CONSIDERABLE EFFUSION.

Dr. Blanquinque, of Laon (France), has published such a case in the *Gaz. Heb.* of September 15, 1871. The patient was a coachman of thirty-nine, the principal symptoms being epileptic fits, paraplegia, and amaurosis. The effusion, on a post-mortem examination, was very considerable at the base of the brain; it had caused the flattening of the circumvolutions, and widened the space between the crura cerebri and the optic tracts. The lateral ventricles were much dilated, the choroid plexuses very vascular, and the venæ Galeni gorged with blood. The pineal gland was surrounded with a softish mammillated tumour the size of a pigeon's egg, which presented, on being cut across, calcareous points and small hæmorrhagic deposits. The tumour pressed upon the venæ Galeni and the tubercula quadrigemina. By a microscopical examination it was found that the growth consisted of a hypertrophy of the pineal gland. Dr. Blanquinque does not think that this tumour gave rise to special symptoms, and considers that its effects were similar to those which are the consequence of growths in the brain of other kinds.

ARTERIAL TRANSFUSION.

Prof. Hüter recommends the transfusion of the venous blood of a healthy individual into the artery of a patient. He uses the radial artery at the wrist, or the posterior tibial at the ankle. When the vessel is laid bare for about an inch, four threads are passed under it. The upper is to prevent the escape of the blood coming from the heart, and another is loose, to be used in case of hæmorrhage by the collaterals. The vessel is now incised, and the canula introduced and fixed by the third thread, the fourth being merely a precautionary measure. The venous blood from the healthy man is now beaten and filtered, to prevent the clogging of the fibrine, and injected. The author says that about a pound of blood can thus be introduced into the hand or foot, and he quotes eight such operations of his own. Prof. Albanese, of Palermo, has used this kind of transfusion in three cases of anæmia and four of pyæmia, the radial artery being opened in all the cases. Four patients recovered—three anæmic, and one suffering from poisoning of the blood.—*Riv. Med. Chir.*, *Buenos Ayres*, Jan. 23rd, 1871.

TEMPERATURE IN ERYSIPELAS.

Prof. Verneuil mentioned, at a late meeting of the Surgical Society of Paris, several cases of erysipelas which had been under his care, and in which he was able to predict the advent of erysipelas by watching the sudden rise of temperature. In all these the rise of temperature preceded the rash at least twenty-four hours. The cases certainly have some weight, but it was objected to M. Verneuil that other signs, noticed in the same patients, have also great value—viz., shivering, &c. As, however, the temperature in the cases alluded to rose fully three degrees, it is worth while, in appropriate instances when erysipelas is apprehended, to pay attention to the variations of temperature. But we all know that a thermometrical rise may be the forerunner of several pathological phenomena besides erysipelas.

PURIFYING OF HOSPITAL WARDS.

M. Rabot states in *L'Union Méd.*, August 20th, 1871, that since 1868 he has succeeded in purifying hospital wards, without disturbing the patients, by the following means:—An oxygen-generating apparatus is placed outside the building, and, by means of a long indiarubber tube, a volume of oxygen is introduced measuring about the thousandth part of the cubic feet of the room, after the ward had, as usual, been well ventilated. To feed the patients' curiosity a few fumigations with cascarilla were made. In both extremities of the ward large basins containing one pound of peroxide of manganese and ten pounds of the solution of hyperchloride of lime were placed and changed every day. The method has been conscientiously applied by M. Rabot in a hospital at Versailles with the best results.

ON ALBUMINURIA IN SMALL-POX.

In the *Lyon Méd.* of Sept. 3rd M. Cartaz has published an account of several cases of variola in which the above complication was noted. The deductions are as follows:—1. Albuminuria has been noticed in confluent small-pox, the frequency being about one in five. 2. Albuminuria is most frequently only temporary, and does not interfere with the progress of small-pox. When permanent, it may bring on the mischief wrought by Bright's disease. 3. Even when transitory, albuminuria may give rise to severe symptoms, as eclampsia, &c. 4. In hæmorrhagic small-pox abuminuria is persistent, whether it be connected with a lesion of the kidney, whether it depend on contamination of the blood, or it be owing to the combination of both phenomena.

OSTEO-MYELITIS AND PYÆMIA.

There are, unfortunately, many ways in which pyæmia may occur, and we are not surprised to find M. Demarquay publishing a series of nine cases, all of gunshot wounds, in which osteo-myelitis was followed by pyæmia and death. The author endeavors to show the relationship of both affections, and to prove that the osteo-myelitis was really the cause of the pyæmia. He thinks that henceforth osteo-myelitis should be carefully studied in view of the consequences alluded to in the paper. M. Demarquay has very carefully injected a solution of strychnine into the medullary cavity of the femur of a rabbit, and the animal soon died with all the symptoms of strychnine poisoning.

News Items, Medical Facts, &c.

Torsion of Arteries.—M. Tillaux lately read a paper before the Surgical Society of Paris, wherein he stated that he had given up the ligature for torsion with complete success. The author of course mentioned that herein he had followed Amussat's teachings; but did not say one word of the progress of torsion in England. M. Fort writes a sensible letter to the *Gazette Médicale*, where he pointedly alludes to the omission. M. Fort refers, moreover, as an eye-witness, to Mr. Pollock's successful applications of Reverdin's transplantation at St. George's Hospital.

Collodion Teeth.—It appears, from a review in *L'Union Méd.*, Sept. 27, 1871, of M. Preterre's treatise on the teeth, that an Englishman now makes beautiful teeth with collodion. The latter by evaporation is reduced to thin leaves, which are then dissolved in ether, so as to obtain a kind of paste. This is passed into moulds, subjected to pressure and heat, becomes solid, and soon acquires the hardness of bone and ivory.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 3. NEW YORK, MARCH, 1872.

PRACTICAL PHYSIOLOGY.

Lectures
ON
EXPERIMENTAL PHYSIOLOGY.

By **Wm. Rutherford**, M.D., F.R.S.E.,

Professor of Physiology in King's College.

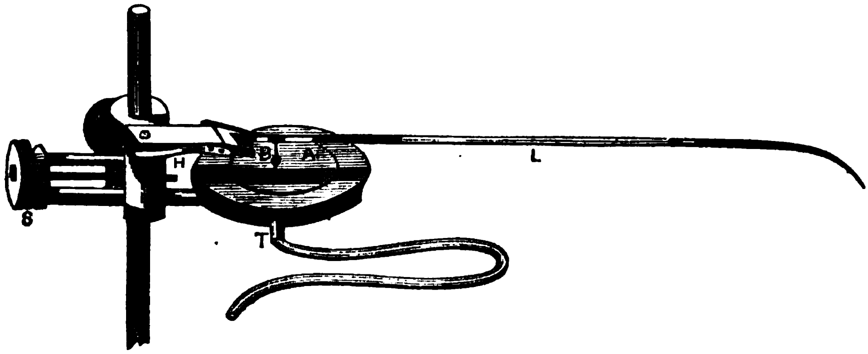
LECTURE IX.

CIRCULATION.

Heart.

Gentlemen,—The motion of the heart has long been a subject eagerly investigated by physiologists; but only since the introduction of the graphic method into physiology have we arrived at accurate information regarding it. The most direct method for recording the heart's motion is that which we owe to Chauveau and Marey. These ingenious experimenters have supplied us with many instruments adapted for the registration of movements, but none more generally useful than the following, which has been well named the "polygraph." (Fig. 37.) This invaluable instrument consists of a light lever (L), bearing at its free end a pen for carrying ink, or a sharp metal point for scratching a line upon a blackened surface. The fulcrum of the lever can be raised or depressed by a little handle (H). The power is applied to the lever by means of a thin brass rod jointed to the lever above and to a thin circular aluminium disc below. This in turn is glued to a thin vulcanised india-rubber membrane stretched over a little tambour or drum containing air communicating with an india-rubber tube (T). When air is forced into the tambour its elastic roof is raised, and with it the lever. I fill the pen on the lever with ink, and place it against the paper of a registering apparatus (Fig. 38.) I blow into the tambour, and you observe the motion which I give to the lever is registered. The excursions of the lever can be

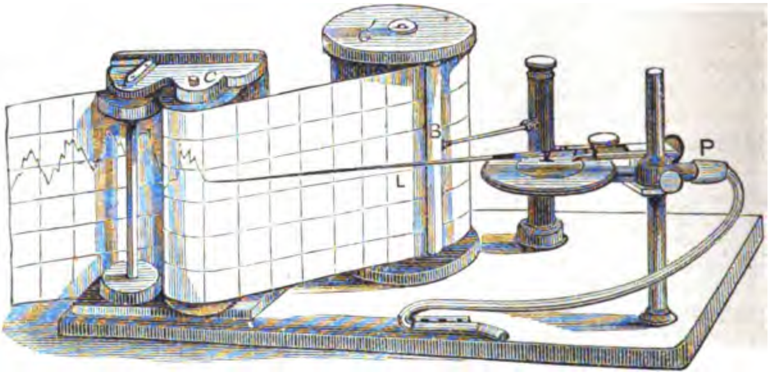
Fig. 37.



Chauveau and Marey's polygraph.—L, Lever. A, Circular plate of aluminium resting upon a thin vulcanised india-rubber membrane. This covers a tambour or drum, the interior of which communicates with a tube (T). B, A hinge connecting the lever with the metal plate. H, Handle by means of which the fulcrum of the lever can be raised or depressed. S, Screw by means of which the tambour can be pushed forwards or drawn backwards when it is desirable to apply the power to the lever at a distance from, or near to, the fulcrum.

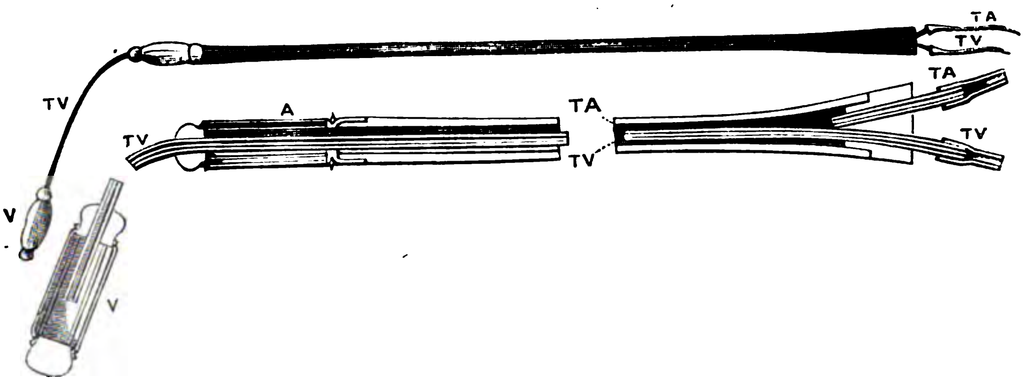
regulated by causing the power to act near to, or at a distance from, the fulcrum. This we readily do by pushing the tambour forwards or drawing it backwards by means of a screw (s, Fig. 37). The movable roof of the tambour and the lever are all made as light as possible—hence the *aluminium*—in order that there may be little momentum. This tambour can be readily employed for registering the movements of the heart, pulse, lungs, and some muscles by means of Marey's myographic forceps. (Fig. 36.) We only require to put at the other end of its tube a compressible air-tight bag or tambour, consisting, e.g., of a glass funnel with its mouth covered with vulcanised rubber sheathing, and its tube inserted into the rubber tube of the polygraph. Every time we compress the bag or indent the membrane over the funnel we force air out of these into the tambour of the polygraph, and so cause its lever to rise.

FIG. 38.



P, Polygraph. L, Its lever inscribing its movements upon Marey's double cylinder register. B, A clamp for keeping the paper tight upon the adjoining cylinder, C′, while it is drawn off this by the other cylinder (C). (After Marey.)

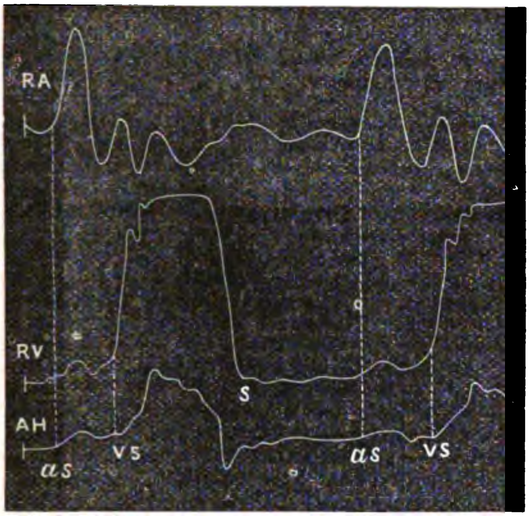
FIG. 39.



Chauveau and Marey's sounds for the right side of the heart. The upper figure represents an entire cardiac sound. T A, Tube for the auricle. T V, Tube for the ventricle. V, Ampulla for the ventricle. The auricular ampulla is seen at the bend of the instrument. The lower figure represents a vertical section of the apparatus enlarged. The letters are the same.

In the celebrated experiment in which Chauveau and Marey registered the movements of the heart of a horse, they used three polygraphs such as the above. The pens of these were all placed at a little distance one above the other, and brought into contact with the same piece of paper, so that they wrote their stories one above another. (See Fig. 40.) With two of the polygraphs there were connected two tubes terminating in elastic ampullæ. (Fig. 39.) These tubes were introduced through the right jugular vein, so that the terminal ampulla (V) was within the right ventricle, while the other ampulla (above T V) was within the right auricle. When the auricle and ventricle contract[illegible] they compressed the little air bladders or ampu[illegible] and squeezed the air through the tubes into the [illegible] polygraphs connected with them. In this way movements of the right auricle and ventricle were recorded. The third polygraph registered the movements communicated to the intercostal space struck by the apex of the heart. In their earlier experiments an ampulla was placed between the ribs in this space, and through it the beat of the heart communicated movement to the polygraph in connexion with it. In this way the movements of the right auricle and right ventricle, together with those produced by the beating of the heart's apex against the wall of the chest, were all simultaneously recorded. You must at once perceive how valuable must be the information obtained from so ingenious and admirable a method of experimentation. I show you a tracing taken in this way by the French experimenters. (Fig. 40.) The writing points of the three levers were placed vertically above each other, so that when in a horizontal position their points would all touch a vertical line. Vertical lines have been drawn in order to aid the eye in comparing the events recorded in the different tracings. Perhaps you will jump to the conclusion, that after having arranged the ends of the three levers so that all touch a vertical line when the levers are in a horizontal position, we have only to draw vertical lines through any parts of these tracings, and to at once come to the conclusion that the parts of the tracings intersected by a vertical line were all produced at the same moment. It is not so. You see that when I move the lever (Fig. 37) its free end describes a part of a circle. Now, if we have the point of one lever, say an inch above the abscissa, and the point of another lever only a quarter of an inch above the abscissa, a vertical line drawn through the two tracings cannot possibly intersect parts which were simultaneously traced by the levers. To arrive at absolute accuracy we must subject the curves to a correction. We must draw under the trace, or through it, a horizontal line such as that which would be inscribed by the lever in a horizontal position (Fig. 41). Near the origin of the curve we produce a vertical line, or ordinate (*o*, *o'*), and then intersect the curve by any number of horizontal lines parallel with the abscissa. We now take a pair of compasses, and separate their points to a distance equal to the length of the graphic lever. We place one point on the junction of the ordinate with the abscissa (*o*), the other limb to the right of this point along the abscissa line, and then describe a curve (*o*, *x*). This is the part of a circle described by the lever; and in order to correct the curve (*o*, *a''*), so as to find out the exact *moment* at which any point of it was produced, and in order to find out the exact intensity of any of its points (see Lecture VIII.), we must measure the distance between any two points on the same level in the lines *o o'* and *o*, *x*, and shift the point of the curve *o*, *a''* which is on the same level towards the left—e. g., the point *a'* must be shifted towards the left for a distance equal to the interval between the ordinate (*o*, *o'*) and the point *a* in the curve *o*, *x*. By carrying out this operation at various points we form a new curve (indicated by the dotted line, Fig. 41), in which the error due to the circular motion of the lever is got rid of. In an instrument such as Helmholtz's myograph (Fig. 26, Lecture VIII.), where the lever in its motion, instead of describing the arc of a circle, simply inscribes a vertical line upon a surface that is motionless, no correction of the curve is necessary.

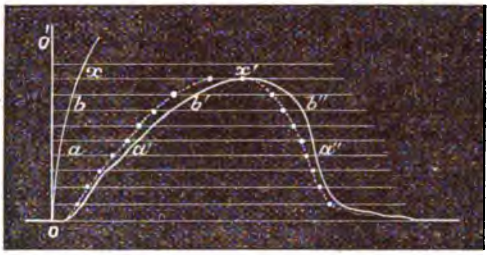
FIG. 40.



A cardiac revolution.

Tracing taken from the heart of a horse by Chauveau and Marey's cardiograph, in the manner indicated in the text. R A, Tracing produced by right auricle. R V, Tracing due to right ventricle. A H, Tracing obtained from the beat of the apex of the heart against the wall of the chest. The dotted lines have been added in order to assist the eye. A S, Auricular systole. V S, Ventricular systole. S, Closure of sigmoid valves. Read the tracings from left to right. (After Marey.)

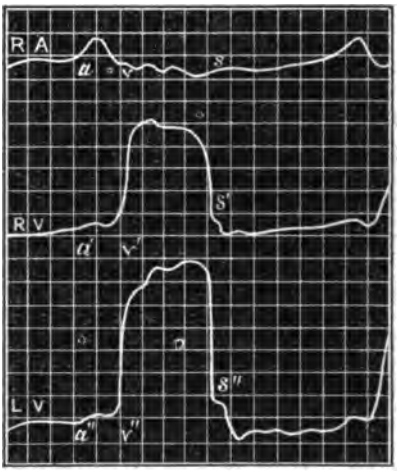
FIG. 41.



o, *o'*, Vertical line or ordinate. The horizontal line, or abscissa, is the lowest of the horizontal lines. *o*, *a''*, Curve produced with Marey's myograph by gastrocnemius of frog—see text. (After Marey.)

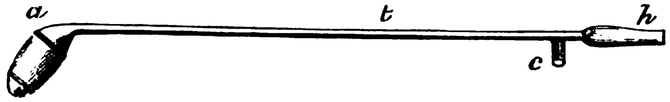
Now, after this very necessary digression, let us return to the cardiac tracings (Fig. 40). After what I have said regarding the correction of the curves, you may feel inclined to ask why vertical lines have been put there at all. But the fact is, that the vertical lines are perfectly permissible, provided that the levers have been parallel when they traced the points intersected by the vertical lines. And just at the points where the dotted lines have been drawn the levers have been so nearly parallel that, practically, we are sufficiently accurate in producing the dotted lines in these situations, and in regarding the points of the tracings intersected by them as simultaneously produced. A rise in the trace line indicates compression of the ampullæ inside the heart and of the ampulla between the ribs. The great rise in the auricular trace, R A, is due to contraction of the auricle. Observe that it produces a slight wave in the ventricle (R V), and in the apex trace (A H). The ventricular contraction is marked by a great rise in the ventricular and in the apex tracings; and simultaneous with the ventricular contraction there is a great vibration in the auricular trace. The cause of this vibration is not clear. Possibly the movement of the base of the heart by the contracting ventricles may have something to do with it. The closure of the auriculo-ventricular valves is indicated by a slight curve in the ventricular and apex tracings (just to the right of the dotted line V S). The closure of the sigmoid valves is indicated by a vibration in all the three tracings (S). If you look at the frogs' hearts which are beating before you, you can have no difficulty in seeing that the auricular contraction comes immediately before that of the ventricle, and that it is much shorter than the latter. The tracing shows that the contraction of the auricle entirely ceases before that of the ventricle begins. It is a most difficult matter to determine what amount of cardiac evolution is occupied by the auricular, and how much by the ventricular contraction. But it is easy to do so when we record the movement. (A straw lever was here laid across the auricle, and another upon the ventricle, of the excised frog's heart; the ends of levers shod with aluminium foil were placed against a smoked cylinder, and the movements recorded.) To measure the portion of the cardiac revolution taken up by the auricular and by the ventricular contractions, we have only to lay a scale horizontally along each tracing, and measure off the relative duration of the periods of rest and movement. By doing this we find that in the horse's heart (Fig. 40) the auricular contraction occupies one-fifth, and the ventricular contraction two-fifths, of a cardiac revolution. When the tracing was taken the heart was beating nearly sixty times in the minute. When the rapidity of cardiac action varies it is the length of the pause which undergoes change; there is rarely any marked change in the duration of the contraction.

FIG. 43.



Tracings taken from the heart of a horse by Chauveau and Marey.—R A, Right auricular trace. R V, Right ventricular trace. L V, Left ventricular trace. *a*, Auricular systole. *a'* and *a''*, The effects of this upon the ventricular tracings. *v'* and *v''*, Ventricular systoles. *v*, Effects of right ventricular systole upon right auricular trace. *s'* and *s''*, Closure of sigmoid valves. These tracings are not so delicate as those in Fig. 40. Probably the friction between the pen and the paper has in this case been greater than in the other. Compare R V, Fig. 40, with R V, Fig. 43.

FIG. 42.

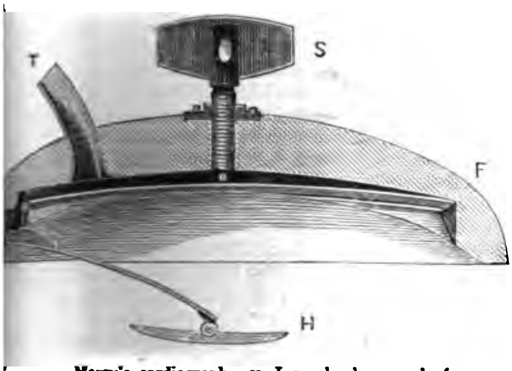


Chauveau's and Marey's sound, for the left ventricle of the heart. *h*, Handle; *t*, tube; *c*, opening by which the tube communicates with an elastic tube of a tambour (Fig. 37); *a*, end of the sound, with its ampulla of vulcanised india-rubber membrane for introduction into the ventricle.

It is instructive to compare the movement of the left with that of the right ventricle. Chauveau and Marey have performed experiments upon the horse, which enable us to institute this comparison. They introduced into the right auricle and ventricle the sound which I have just described, and they passed another sound (Fig. 42) into the left ventricle through an opening in the left carotid artery. I show you the tracings taken in this experiment by the French investigators (Fig. 43). It is sometimes very convenient to take tracings upon paper previously divided into squares, such as is commonly employed by meteorologists. For tracings such as these it is most valuable, for it enables the eye readily to measure the height and duration of each part of the trace. But, as regards the *time*, such squared paper is only valuable when the motion of the cylinder of the recording apparatus is perfectly uniform; for, were it not uniform, of course each space between two vertical lines could not have the same value in point of time. (See Lecture VIII.) On comparing the tracings derived from the two ventricles, it is evident that the ventricular movements are synchronous; but it is also evident that, while the maximum of contraction in the case of the left ventricle is attained near the close of the systole, in the right ventricle it is attained shortly after its commencement. This, though generally obtained, is not, however, a constant feature. The pressure exerted within the left ventricle is stronger than that within the right, as indicated by the higher curve produced by the former. The closure of the left sigmoid valves (S″), too, produces a more marked effect than the closure of those on the right side (S′).

In Chauveau and Marey's earlier experiments the apical cardiac movements were communicated to a polygraph through a little elastic ampulla placed in the intercostal space over the heart. The apex tracing which I have shown you, however, (A H, Fig. 40), was taken by means of another instrument invented by Marey, and termed by him the cardiograph (Fig. 44). This instrument consists of a hollow eliptical wooden capsule (F). Two springs are placed in its interior, one bearing an ivory knob (H), which is placed over the point where the apex of the heart beats. Near its fulcrum it is pressed upon by another spring, to which various degrees of tension may be given by a screw (S). The interior of the cardiograph communicates by a tube (T) to which the elastic tube of a polygraph is connected when the cardiac movements are to be recorded. When the capsule is pressed firmly upon the skin, the interior of the instrument is converted into a closed chamber, and every shock of the heart expels air out of the chamber into the polygraphic tambour. The spring with the knob (H) depresses the skin when it is relieved from the pressure of the heart, and so the air is drawn back again from the polygraph. Admirable tracings have been taken from the human heart by Marey by means of this apparatus. It is, however, inconvenient for long observations, because the edge of the capsule is apt to rise from the skin, and so permit the air to escape from the interior.

Fig. 44.



Marey's cardiograph. H, Ivory knob on end of spring. S, Screw for tightening spring. T, Tube for connecting the interior of instrument with a polygragh. F, Wooden capsule. Full size (after Marey).

Dr. Sanderson has invented a tambour cardiograph which gives good tracings. It is essentially the tambour and spring of Marey's myographic forceps (Fig. 36, Lecture VIII.), mounted upon three screw legs, and rested upon the chest. Marey's sphygmograph may be used as a cardiograph. For this purpose we have only to place the ivory pad of the spring over the intercostal space where the apex beats, and retain it there. This instrument gives better cardiograms than any other form of cardiograph yet invented. The advantage of the tambour cardiograph is that it can be more readily fixed upon the chest, and that it can be readily connected with a polygraph, so that a tracing of any length can be taken. A long tracing could, however, be taken by the sphygmograph by causing the pen to write upon a long piece of paper, moved by a small register with cylinders (similar to that represented in Fig. 38). When we desire to take a cardiogram from the human chest, it is advantageous to make the person sit erect, or incline a little forwards, in order to permit of the heart's apex acting as much as possible upon the thoracic wall. The respiratory movements disturb the cardiac tracing, and therefore, when a short tracing is wanted, it is advantageous to make the person expire, and to hold his breath with his chest collapsed as long as he can. The state of expiration is chosen because, if the lungs be inflated as in inspiration, the heart is more separated from the anterior thoracic wall than during the state of expiration. The pressure exerted by the cardiographic spring over the heart's apex must be constant during the registration of the heart's movements. A difference in the pressure of the spring gives a difference in the character of the tracing. It is difficult to maintain this constant pressure for any length of time. The instrument may be fixed in position by a strong belt passed round the chest; another bar fixed to this at right angles passes across the side of the chest, and to the latter is fixed at right angles a second bar, which projects in front of the chest. This bar is elastic, and made of whalebone or steel; it can slide up or down the lateral bar, and be fixed in position by a screw, so that it can be adapted to a chest of any capacity. The anterior bar, too, can be lengthened or shortened, so that the cardiograph connected with it can be moved to any point upon the anterior surface of the chest. The elastic bar bearing the cardiograph can at pleasure be taken away altogether, and a rigid bar bearing a tambour, similar to that of the myographic forceps (Fig. 36, Lect. VIII.) be substituted when we desire to connect the instrument with a pneumograph or stethograph. (The arrangement of the apparatus is really essentially like that of the myographic forceps just referred to.) The bar bearing the cardiograph should be elastic, in order that during the respiratory movements the cardiographic pressure may remain as constant as possible. [Cardiac tracings were here taken with the tambour cardiograph, and the sphygmograph used as a cardiograph, and it was demonstrated that, in the same individual, tracings differing from one another may be obtained by (1) varying the pressure of the spring over the heart's apex; (2) varying the position of the body; and (3) shifting the point of the thoracic wall to which the cardiographic pad was applied.]

Now you see with what a fickle instrument we have to deal! See how many are the causes of variation in the results which it affords. How easily we might be led into error by coming to hasty conclusions from a consideration of these results. In order to study any cardiac affection from day to day with the aid of this instrument, see what must be done in order to yield results anything like trustworthy. The pad of the cardiograph must always be in relation to the same part of the heart. The tension of the cardiographic apparatus must be the same. You must readily see how difficult it is to fulfil such conditions, and to, at different times, obtain results which can be compared. By and by we hope to possess, regarding this subject, more perfect information, but, for the present, take particular care what you are about. You have seen, too, that if you merely touch for a moment the tambour of the cardiograph you cause the lever of the polygraph in connexion with it to describe, not a single wave, but a number of waves. There is, first, a great wave indicating the touch, and then a number of wavelets of diminishing size. These wavelets are due to the pendulum-like oscillations into which the column of air is thrown. You have also studied the fact that these secondary oscillations can be largely got rid of by diminishing the mobility of the lever—e. g., by causing a little friction between the pen and the paper; but, again, if this friction be too great the lever fails to indicate movements communicated to the cardiographic tambour.

Next day we will study the innervation of the vascular system.

[A cardiographic tracing is given in Fig. 40 (A H). This, though from the horse's heart, is like tracings which may be obtained from the human heart. For beautiful cardiograms taken from the apex of the human heart see an excellent paper on "Cardiographic Tracings from the Human Chest-wall," by Mr. A. H. Garrod, in the *Journal of Anatomy and Physiology*, p. 26, Nov. 1870.]

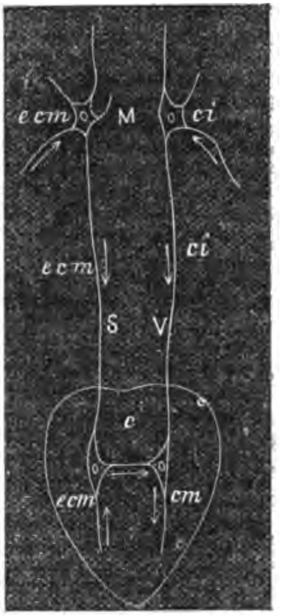
LECTURE X.

CIRCULATION—(CONTINUED.)

Innervation of the Heart—Intracardiac Nerves.

Gentlemen,—From this frog, narcotised with opium, I remove the heart, and lay it upon a plate of glass in a little pool of serum to keep it from getting dry. The heart goes on pulsating rhythmically, just as it did before it was removed from the body. As we shall see, the cardiac movements may be influenced by the central nervous system; but this experiment shows that in the heart itself are to be found the conditions which are essential for its rhythmical motions. Do these conditions exist in every part of the heart? I divide the heart transversely through the middle of the ventricle. Observe that now the apex lies motionless, while the base goes on beating rhythmically as before. Why is the apex motionless? Have I destroyed the contractility of its muscular fibres? Not so; for you see that when I send a faradic shock through the apex it gives a contraction, and then lies motionless as before. This experiment shows that the conditions necessary for the rhythmical motion do not exist everywhere in the heart. They are in the basal but not in the apical portion of the frog's heart which lies divided before you. Ganglia are found in the septum between the auricles, and in the sulcus between the auricles and ventricle. They have not been found in the apex. Hence it is generally believed that Remak was right when he maintained that the intracardiac ganglia are essential for the heart's rhythmical motions.

Fig. 45.



A diagrammatic representation of some of the cardiac nerves.—c, Heart. M, Medulla oblongata. Two intracardiac nerve-cells are represented as forming, with their fibres (the three marked with arrows, and in the heart), a reflex mechanism. *e c m* being an afferent or excito-cardio-motor nerve, which is probably connected with the endocardium. *c m*, An efferent or cardio-motor fibre, ending peripherically in a muscular fibre. S, Sympathetic. V, Inferior cardiac branch of vagus. The sympathetic is excito-cardio-motor, or accelerating in its action. The vagus is cardio-inhibitory in its action. Two cells are represented in the medulla with nerves joined to them. The arrows indicate the direction in which the nerve influences appear to pass. The nerves marked *e c m* are excito-cardio-motor; they accelerate the cardiac contractions. The fibres marked *c i* are cardio-inhibitory; they inhibit or retard the cardiac action. (See text.)

Regarding the nature of the nervous mechanism existing within the heart we know nothing definite. We are, no doubt, safe in supposing that the muscular fibres are presided over by motor nerves connected with cells of the intracardiac ganglia. Bernard showed long ago that cardiac action may be readily called forth by stimulating the lining membrane of the heart. This fact seems to show that there is a reflex nerve mechanism in the heart, and that afferent nerves connected with this terminate in the endocardium. I repeat that we do not know the exact nerve arrangements, but it will facilitate your study of the matter if I draw a diagram (lower half of Fig. 45) showing a reflex nerve mechanism, such as I have drawn in a previous lecture (Lecture VI.) I put two nerve-cells, with a centripetal, a centrifugal, and an intercentral nerve fibre. I beg you particularly to remember that this diagram *is a mere scheme.*

There is, then, every reason for believing that there is an intracardiac reflex nerve mechanism. Does this mechanism come into operation in order to bring about every cardiac contraction? I cannot return a precise answer to such a question. I am inclined to believe with Von Bezold that within the motor nerve-cells nerve force is produced as long as pabulum is supplied to the cell, and that when this force reaches a certain degree of tension it excites the motor nerve-fibre, and thereby throws the muscular fibre into contraction. That seems to me the most feasible hypothesis accounting for the rhythmical motion of the heart; but, mind you, it is *only* a hypothesis.

Cardiac Branches of the Vagus.

Whatever be the cause of the rhythmical action, it is quite certain that it can be influenced by nerves connecting the heart with the central nervous system. These nerves are branches of the vagus and sympathetic. In the rabbit the vagus gives two branches to the heart, a superior and an inferior cardiac branch. The former (*s c*, Fig. 46) leaves the vagus with the superior laryngeal nerve (*s c*); commonly it has two roots, one connecting it with the superior laryngeal nerve, the other with the trunk of the vagus. This nerve (o and o′, Fig. 47) courses down the neck close to the cervical sympathetic (it is the smaller of the two), joins one or two branches of the inferior cervical ganglion (Fig. 47), with which it proceeds to the heart. In dogs the nerve is bound up with the vagus and cervical sympathetic in one common trunk. In cats it forms, throughout the greater part of its extent, a single trunk with the cervical sympathetic. The terminations of the nerve inside the heart are unknown. The inferior cardiac branch of the vagus (*i c*, Fig. 46) leaves the vagus with, or rather below, the inferior laryngeal nerve in the rabbit, and from thence proceeds to the heart. It is believed to end in the cardiac ganglia, but a connexion between the nerve-cells and its fibres has not been satisfactorily shown. These two branches of the vagus have very important functions. That of the inferior cardiac branch was discovered in 1845 by the brothers Weber, while the function of the superior branch was discovered by Ludwig and Cyon in 1866. Both nerves are inhibitory; they diminish, or may indeed suspend, contractions of certain parts of the blood-vascular system. The inferior branch is a cardio-inhibitory, the superior branch is a vaso-inhibitory nerve. That is to say, the one inhibits the contractions of the heart, the other inhibits the contractions of bloodvessels...... [The vagus of a narcotised frog was divided in the middle of the neck, and the lower or cardiac end irritated, at first gently, and then severely. During the gentle stimulation the heart beat slower, and during the powerful stimulation it came to a standstill in a state of *relaxation*. During the arrest of the heart's action it was shown that direct stimulation of the heart produced contraction. It was further shown that, although the vagus irritation was continued, the heart began to beat again after a time.]

Fig. 46.

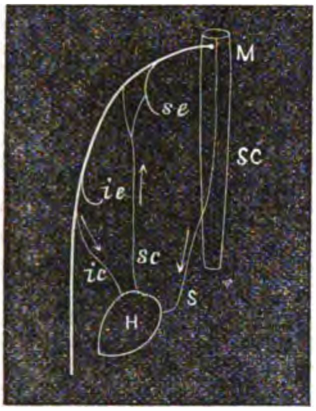
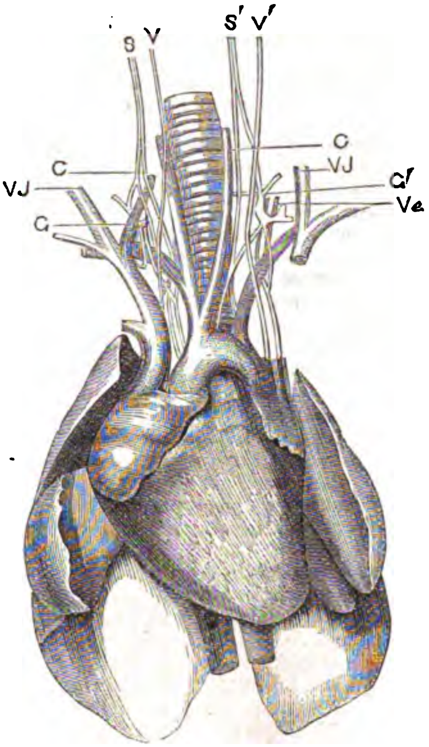


Diagram showing extracardiac nerves of the rabbit. M, Medulla. S C, Spinal cord. V, Vagus. S. Sympathetic. *s e*, Superior laryngeal nerve. *i e*, Inferior laryngeal nerve. *s c*, Superior cardiac, and *i c*, Inferior cardiac branch of vagus. The arrows indicate the direction of the nerve influences.

You have seen the effect of stimulating the lower end of the trunk of the vagus. The heart beats more slowly and may even for a time be brought to a standstill, not in a state of spasm, but in a state of *relaxation*. This is not due to a paralysis of the muscular fibres of the heart, for you have seen that when they are irritated they contract. It was formerly ascribed to spasm of bloodvessels in the heart cutting off the supply of pabulum to the nerves. But, as shown by Hyrtl, the frog's heart has no bloodvessels, therefore it is supposed that the nerve acts upon the cells of the cardiac ganglia which are concerned in the production of the cardiac motions, and this view is further supported by the fact that inhibitory nerves appear always to convey their influences *towards* parts which contain nerve-cells. Schmiedeberg and others suppose that the vagus does not directly end in the motor ganglia of the heart, but that there is between it and the motor ganglia an inhibitory apparatus which they are inclined to regard is ganglionic in its nature. Schmiedeberg has arrived at this conclusion from observing that if a frog be poisoned with nicotine irritation of the trunk of the vagus no longer retards the heart's action, but if the venous sinus be irritated, the cardiac action is inhibited just as if the trunk of an unpoisoned vagus had been stimulated. On the other hand, if a frog be poisoned with atropia it becomes impossible to retard the cardiac action by stimulating either the trunk of the vagus or the venous sinus. The atropia paralyses a part of the inhibitory mechanism which occupies a position peripheral to that paralysed by nicotine. Such experiments are extremely interesting and important. We are not, as yet, however, able to come to a conclusion regarding the way in which the vagus ends in the heart. The existence of a special intracardiac ganglionic inhibitory apparatus, distinct from the motor ganglia, has yet to be proved, and, therefore, I am not inclined to indicate the probable termination of the vagus within the heart by a diagram differing from that which I have used for sever

Fig. 47.



Extracardiac nerves of rabbit. S and S′, Sympathetic nerves with the superior cardiac branches of the vagus, C and C′. The sympathetic joins the inferior cervical ganglion, G and G′. The superior cardiac branch of the vagus joins a branch of the inferior cervical ganglion and with it proceeds to the heart. V, Vagus. V J, Right external jugular vein. V J′, Left external jugular vein. V *e*, Vertebral artery.

years. I therefore represent a cardio-inhibitory fibre of the vagus as ending in a motor ganglionic cell within the heart (see Fig. 45, v). Whether there be an intermediate apparatus or not, the cardio-inhibitory influence of the vagus seems to be conveyed to the motor ganglia. It appears to restrain the action of these. The amount of this restraint or inhibition bears a direct proportion to the extent to which the nerve is stimulated. When the stimulation is powerful the heart is completely arrested for the time; but the promptings to motion seem to become so powerful in the heart that contractions take place after a time, even although the vagus be stimulated as powerfully as before.

A powerful stimulation of the vagus is required in order to inhibit the cardiac action. This fact was brought out in a somewhat striking way by a method of experimentation adopted by me some years ago.* In rabbits and frogs I divided the vagus in the middle of the neck, and irritated the lower end by means of induction currents obtained from Du-Bois Reymond's machine. I began with very feeble currents, and found out the weakest current which sufficed to irritate the inferior laryngeal fibres of the vagus so as to cause motion or the arytenoid cartilages. I then found out the weakest currents which sufficed to retard the cardiac action. From twelve experiments on rabbits I found that the laryngeal muscles were thrown into contraction by a current obtained from the machine when the secondary coil was at an average distance of 660 milimetres from the primary coil; while the heart's action was not inhibited until the secondary coil was pushed up to within an average distance of 225 millimetres from the primary coil. In four experiments on frogs I obtained similar results. The laryngeal muscles contracted when the average distance between the coils was 575 milimetres; while the heart was inhibited when the average distance was 190 milimetres. It is thus evident that a far more powerful irritation of the vagus is needed to retard the heart's action than is required to produce muscular action. The explanation which I would suggest in order to account for this difference is, that it is probably not owing to a difference in the excitability of the inferior cardiac and inferior laryngeal fibres of the vagus, but to a difference in the termination. An ordinary motor nerve may be supposed to encounter little opposition when it acts upon the muscular plasm compared with the opposition which an inhibitory nerve has to encounter, for its action is opposed by counter influences, and therefore it must be thrown into action sufficiently powerful to overcome these.

As I have mentioned, nicotine paralyses the cardio-inhibitory fibres of the vagus. This paralysis is preceded by irritation of these fibres. Atropia and curara produce paralysis without any previous irritation. Calabar bean increases the irritability of the cardio-inhibitory fibres, so that they are more readily thrown into action. As shown by Arnstein and Sustschinsky, the actions of Calabar bean and atropia are antagonistic. The effect of the one may be undone by that of the other. It is easy to convince oneself of this interesting and important fact. [The vagus of a rabbit was divided in the middle of the neck. Its lower end was irritated. The heart was thereby brought to a standstill. Four milligrammes of sulphate of atropia, dissolved in four minims of water, were injected into the jugular vein. Within a minute thereafter the vagus was again stimulated; but no inhibition of the heart resulted. (The inferior cardiac branch of the vagus had been paralysed by the atropia.) Twenty milligrammes of the ordinary extract of Calabar bean, dissolved in eight minims of water, were injected into the vein. Within a minute thereafter the vagus was again irritated, and the heart was brought to a standstill—owing to the atropia paralysis having disappeared.] The remarkable experiment which you have just witnessed is one of extreme importance. It affords a typical example of the antagonistic action of two poisonous substances.

The cardio-inhibitory fibres of the vagus may be thrown into action by stimulating (1) the central end of the superior cardiac branch of the vagus, (2) the central end of the vagus of the other side, (3) sensory nerves in the case of warm-blooded animals, (4) the abdominal viscera in the frog, (5) the splanchnic and cervical sympathetic. In these instances the action of the cardio-inhibitory nerves appears to be induced reflexly. (6) Increased blood-pressure stimulates the cardio-inhibitory fibres. In the rabbit the heart may be inhibited to a remarkable extent by stopping the respiration. The heart comes nearly to a standstill usually within three seconds after the cessation of the respiration. This is due to irritation of the cardio-inhibitory nerves. A similar phenomenon is not observed in man, in the dog, or cat. The cardio-inhibitory fibres of the vagus appear in the case of warm-blooded animals to be usually in a state of action. It is commonly said that they are constantly in action, but I do not think that this is true. They are usually, but not always, in action. When they are in action, section of the vagi in the neck and paralysis of the cardio-inhibitory fibres are followed by an increase in the rate of the heart's motion. The results of section are not altogether to be trusted in the determination of such a point, for I have repeatedly found that section of the vagi in rabbits and dogs is followed by accelerated cardiac action, even when the cardio-inhibitory nerves had been previously paralysed by atropia. The probable cause of the acceleration in this case will be alluded to when we study the blood-pressure. Atropia paralyses the cardio-inhibitory nerves. If it be given after division of the vagi in rabbits and dogs, acceleration of the heart very rarely ensues; but if given before division of the vagi, acceleration generally ensues. This I regard as the best proof that the vagi are generally in action. In the case of the cat and rabbit the amount of action appears to be less than in the dog and man. Bernstein maintains that this action is not due to an influence starting from the roots of the vagus, but that it is induced reflexly. He finds that section of the vagi is not followed by cardiac acceleration if the nerves which can reflexly excite the cardio-inhibitory nerves (see above) have been previously divided. A further investigation is, I think, necessary ere we come to a conclusion on this point. Donders maintains that the action of the cardio-inhibitory fibres increases during inspiration, and diminishes during expiration.

The superior cardiac branch of the vagus (sc, Fig. 46; c, Fig. 47) conveys influences from the heart to the medulla oblongata. It seems to be the sensory nerve of the heart; for when it is divided, and its upper end stimulated, signs of pain are produced. It is also an excito-cardio-inhibitory nerve —that is to say, it conveys influences to the medulla, which excite the cardio-inhibitory fibres of the vagus. But the most remarkable result which follows stimulation of the central end of this nerve is a fall in the arterial blood-pressure. This is to a slight extent due to inhibition of the heart's action; but it is chiefly owing to dilatation of bloodvessels, those which appear to be most influenced being chiefly those of the abdominal viscera. The inferior cardiac branch of the vagus inhibits the heart. It brings the heart to *rest;* it dilates the heart by rendering the organ passive. The superior cardiac branch of the vagus inhibits *bloodvessels*. It brings them into a state of dilatation—that is, a state of rest. The former is cardio-inhibitory; the latter vaso-inhibitory. The former conveys its influence to the cardio-motor ganglia; the latter conveys its influence to the vaso-motor ganglia in the medulla oblongata. In consequence of the remarkable lowering of the blood-pressure effected by the superior cardiac nerves, its discoverers have named it "nervus depressor." Ludwig and Cyon suppose that this nerve is thrown into action when the heart encounters too much opposition to its action. The nerve lowers the arterial blood-pressure, and thereby diminishes the resistance offered to the heart's contraction. The nerve is certainly not in constant action; indeed, the state of action seems to be the exception. The discoverers of the nerve always failed to find it in action—that is to say, they never found the blood-pressure rise on dividing the nerve. I have been more fortunate. In two rabbits and one cat I have found division of both superior cardiac branches of the vagi followed by an increase in the blood-pressure, varying from 15 to 25 per cent. of the pressure previous to the division. These cases occurred in some thirty experiments performed by me. In the case of the cat I had the satisfaction of giving the first demonstration of the action of this nerve to my friend Professor Ludwig himself. What the conditions are which throw the nerve into action my experiments do not as yet enable me to say. It is as yet impossible to say what nerves in the human subject correspond to these nerves in the rabbit. In man one or two small branches leave the vagus in the upper part of the neck, and join the cardiac nerves of the sympathetic. A large cardiac branch leaves the vagus as it enters the chest. One or two small cardiac branches come off the trunk of the right vagus as it lies in the thorax. The corresponding nerves of the left side are branches of the recurrent laryngeal nerve.

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* Influence of the Vagus upon the Vascular System. Trans. Roy. Soc. Edin., vol. xxvi.

The Cardiac Branches of the Sympathetic Nerve.

The cardiac sympathetic nerves appear to have their origin in the medulla oblongata, or brain. In the rabbit they pass to the heart through the cervical portion of the spinal cord (Fig. 46), the inferior cervical and first dorsal sympathetic ganglia, and from thence to the heart. The inferior cervical ganglion of the rabbit is sometimes called ganglion stellatum. It corresponds to the middle cervical ganglion of man, while the first dorsal ganglion of the rabbit corresponds to the lowest cervical ganglion of man. These nerves convey, from the medulla oblongata to the cardiac organ, influences which accelerate its action. Von Bezold thought he had proved this by showing that on irritating the lower end of the cervical spinal cord after its division the heart's action is accelerated. Ludwig and Thiry pointed out, however, that such evidence is fallacious. The cervical spinal cord contains nearly all the vaso-motor nerves of the body, proceeding from the vaso-motor centre in the medulla to various parts of the body. These are stimulated when the lower end of the divided cervical spinal cord is irritated, and, as a result of this, the blood-pressure is greatly increased. They pointed out, and Von Bezold afterwards confirmed the observation, that if the vagi be previously divided, increase in the blood-pressure, which is not excessive, accelerates the heart whether the cardio-motor nerves connecting the heart with the spinal cord be intact or not. If the root of the inferior cervical ganglion connecting the ganglion with the spinal cord be stimulated, the heart's action is accelerated without any noteworthy alteration in the blood-pressure being produced. This root is found alongside the vertebral artery. It is sometimes named "Nervus accelerans."

The nerves, then, which pass from the medulla oblongata through the cervical portion of the spinal cord and the last cervical and first dorsal ganglia of the rabbit to the heart excite the heart's action. We may term them excito-cardio-motor. They antagonise the influence of the cardio-inhibitory nerves. Their termination in the heart, though believed to be in the ganglia, has not been satisfactorily made out. In the diagram (Fig. 45) I have joined them to the cells of ganglia, for it is only by so doing that we can comprehend the action of the nerves. These sympathetic fibres are not in constant action, for division of the cardiac branches of the two ganglia which I have mentioned is rarely followed by a slackened rate of cardiac action. The influences which throw them into action have not been fully ascertained. (1) Emotion probably excites the heart through their agency. (2) According to Ludwig, muscular action stimulates the heart reflexly through them. (3) Diminished arterial blood-pressure, within the capillaries of the brain and medulla oblongata more especially, is believed by Ludwig to stimulate them. Bezold believed that the cervical sympathetic nerve contains fibres which accelerate the heart's action, he having found that if in rabbits this nerve be divided, and its lower end be stimulated, the heart's action is accelerated. I have performed many experiments on this point, and have observed this acceleration in only one instance. It was in a rabbit to which one centigramme of atropia sulphate had been given. In several other cases in which a similar dose was administered no acceleration followed excitation of the nerve. Ludwig had previously arrived at the conclusion that the cervical sympathetic is not an excito-cardio-motor nerve.

In man there are three cardiac nerves connected with the sympathetic on either side. These nerves pass between the cervical sympathetic ganglia and the heart. Each cervical ganglion sends a branch. These nerves have probably an excito-cardio-motor function, but this has not been ascertained. I have mentioned that the middle and inferior cervical ganglia of man correspond to the inferior cervical and first dorsal ganglia of the rabbit.

We have seen, then, that the heart's action is influenced by two nerves connecting it with the medulla oblongata and cervical spinal cord. The inferior cardiac branch of the vagus inhibits, the sympathetic branches accelerate, the heart's action. There is here, therefore, a complex mechanism which doubtless plays a very important *rôle* in cardiac action. What is the precise nature of the influences which bring them into play is still open to conjecture. I have several times spoken of the effect of the blood-pressure on the cardiac action. That the pressure of the blood upon the lining membrane of the heart can influence the heart's motions there can be no doubt. As shown by Ludwig and von Bezold, when the vagi and sympathetic nerves of the heart are divided increased pressure within the heart accelerates cardiac action. This appears to result reflexly by stimulation of the peripheral ends of the intracardiac afferent nerves concerned in the cardiac motions. On the other hand, diminution of the pressure is followed by diminished speed of the heart. But if the cardio-inhibitory fibres of the vagi be intact, increased blood-pressure is followed by diminished cardiac action. This seems to be due to stimulation of the cardio-inhibitory nerves; while if the blood-pressure be lowered Ludwig finds that acceleration of the heart ensues. This he ascribes to irritation of the excito-cardio-motor nerves of the medulla. The influence of alterations in the blood-pressure which are observed when the extracardiac nerves are intact is supposed to proceed from small arteries or capillaries of the brain and medulla, and perhaps other parts. I think that all this requires further investigation ere we come to definite conclusions.

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AN ABSTRACT

OF A

Course of Lectures

ON THE

INTERNAL DISEASES OF THE EYE AS SEEN WITH THE OPHTHALMOSCOPE.

Delivered at the Royal London Ophthalmic Hospital, Moorfields.

By J. Soelberg Wells,

Professor of Ophthalmology in King's College, Ophthalmic Surgeon to King's College Hospital, and Assistant-Surgeon to the Royal London Ophthalmic Hospital.

LECTURES XI. & XII.

SCLERECTASIA POSTERIOR (STAPHYLOMA POSTICUM, SCLEROTICO-CHOROIDITIS POSTERIOR).

Gentlemen,—This disease is almost always present in the higher degrees of myopia, and forms one of the most important affections of the inner tunics of the eyeball, both on account of the extensive tissue changes which it may produce and the dangerous complications—e. g., detachment of the retina, opacities of the vitreous, glaucoma—to which it may give rise. It affords well-marked and often very striking ophthalmoscopic appearances. When it is but slightly developed there is noticed at one part of the margin of the optic disc (generally at its temporal side) a whitish cresent or arc, whose tint varies from a pale greyish-pink to a brilliant white, according to the extent of the choroidal atrophy. This crescent is closely applied to the disc, and varies greatly in size and appearance. At the commencement it really resembles a small arc or crescent, and is but narrow and confined to one portion of the disc; but this white figure may gradually extend more and more in size until it entirely surrounds the optic nerve, and reaches to a considerable distance from it, in time perhaps including even the region of the yellow spot in its expanse. Indeed, it always reaches furthest towards the temporal side. The color of the whitish figure varies with the extent to which the choroid is atrophied; for if the stroma of the choroid is but little thinned the figure will assume a pale greyish-red tint, the outline of the choroidal vessels being easily recognised in its expanse, and small clusters of pigment being perhaps scattered on it and around its margin. But if atrophy of the choroid is far advanced, so that its tissue is greatly attenuated, the sclerotic will be here nearly or even quite denuded of choroid, and shine brightly through, giving a very white, glistening appearance to the figure, the disc perhaps even looking abnormally red by contrast. We have often an opportunity of studying various degrees of atrophy side by side in the same eye: the portion of the arc which is nearest the disc showing the most advanced degree of atrophy, and being consequently the whitest; next to which comes, perhaps, a zone of a pinkish-grey tint, with well-marked choroidal vessels in it; and this, again, passes over at the periphery into a zone of a darker red, in which the structural changes are still less marked; and this may gradually merge into the perfectly healthy choroid. The smaller branches of the retinal vessels are rendered very conspicuous and distinct by the white background formed by the exposed sclerotic, so that their finest twigs can be traced over the figure, whilst they are perhaps quite indistinct in the neighboring healthy fundus. If the white figure surrounds the greater part or the whole of the disc, an inexperienced observer might mistake it for an abnormally enlarged optic disc; but on more careful observation the mistake would at once become apparent, for the distinction between the atrophic zone and the disc is easily recognised, especially by the well-marked pink appearance of the latter, which seems indeed, of an unusually deep tint, from the contrast with the glistening white area surrounding it.

There are certain points in the general appearance of the white figure which should be specially attended to, as they are of importance with regard to the question as to whether the disease is stationary or progressive, a point upon which the nature of the prognosis will very much turn. If the arc is but small and circumscribed, being confined to one portion of the disc, its peripheral margin being sharply defined and regular, and the fundus in its close proximity quite normal and showing no tendency to choroidal atrophy, we may consider the disease as stationary, although it may in time, and under unfavorable circumstances, become progressive. Whereas we must consider the affection to be distinctly progressive if the outline of the figure is irregular and ill-defined, and the choroid at its peripheral margin thinned, and studded perhaps with small circumscribed atrophic patches (which may also appear in the yellow spot), for these will gradually increase in size, coalesce with each other, and extend towards the original crescent, finally becoming merged in it, and thus forming one large white figure. The progress of the disease, more especially if it be at all rapid and the atrophy somewhat advanced, is generally accompanied by inflammatory symptoms in the eye. The patients then complain of frequent pain in and around the eyes, which become readily flushed and watery if it be much used; and they are often troubled by flashes of light, bright sparks, or dark muscæ volitantes.

The external appearance of eyes affected with a high degree of myopia and sclerectasia posterior is often peculiar and striking, for they are very large and prominent, showing a wide opening of the palpebral aperture. The increase in the antero-posterior axis of the eyeball is especially noticeable if the eye is turned inwards towards the nose, for then the posterior segment of the globe appears square and lengthened, and the little hollow which exists between the outer canthus and the globe when the emmetropic eye is turned inwards, has disappeared.

Causes.—It has been often a matter of controversy whether the disease is chiefly due to inflammation of the posterior segment of the sclerotic and choroid (hence the name sclerotico-choroiditis posterior) leading gradually to the extension of these tunics at this point, or whether it is simply caused by a congenital elongation of the eyeball, producing a circumscribed bulge of the sclerotic at the posterior pole (staphyloma posticum) near the optic nerve. Jaeger especially favors the latter view, and considers that posterior staphyloma is almost always congenital, and often hereditary. It appears, however, that the disease is generally due to a combination of these causes. There is no doubt that in the higher degrees of myopia there always exists a tendency to elongation of the eyeball in the antero-posterior axis, which produces stretching and attenuation of the choroid and sclerotic near the posterior pole, where this bulging can occur the easiest, as the globe does not receive any support from the ocular muscles at this point. This tendency to elongation of the eyeball is aggravated by the strong convergence of the visual lines, which a high myopia necessitates when the patient is reading or working at small objects. This great convergence and the strain of accomodation cause congestion of the eye, stasis in the venous circulation, and an increased pressure of the intra-ocular fluids. These conditions not only produce a gradual softening and extension of the choroid, but often also set up more or less acute inflammatory symptoms. The sharp outline of the crescent then becomes irregular and broken, the choroid in its vicinity inflamed, attenuated, and atrophied, which leads to a more or less considerable increase in the size of the white figure. At the same time the eye may become watery, irritable, and painful, and opacities in the vitreous humour appear. But we often meet with cases of a moderate degree of myopia, in which the posterior staphyloma exists for years without increasing in size, or the choroid undergoing any change at its margin or in its vicinity.

The *complications* which are most frequently met with in sclerectasia posterior are: opacities of the vitreous, detachment of the retina, and a glaucomatous condition of the eyeball.

Opacities of the vitreous humour are frequently observed, especially in the progressive and inflammatory forms of the disease. They vary greatly in degree; for sometimes there is only a slight general haze of the vitreous, with a few fixed or floating threads in it, in other cases the cloudiness is very considerable, and there are large membranous shreds pervading it, which are due either to inflammatory changes or to blood effusions. But besides opacities of the vitreous there is another dangerous affection to which Iwanoff has lately called special attention—viz., detachment of the vitreous; which, he states, not unfrequently occurs in sclerectasia posterior, being produced in the following manner:—"The vitreous does not grow in proportion to the gradually increasing size of the eye, and the serous exudation is not at the same time converted into the tissue constituting the vitreous humour, nor does it dissolve it, both remaining indifferent to each other; the connexion between the vitreous and retina becoming, however, loosened, according to the amount of effusion. Now, in the space which is thus formed between the vitreous and retina we find that, in proportion to the development of the staphyloma, more and more serous effusion is collected, detaching the vitreous more and more from the retina."* The peculiar gravity of this condition consists in its being generally followed by detachment of the retina, and it is therefore of great prognostic importance to diagnose it, if possible, with the ophthalmoscope. According to Von Graefe† it is probably chiefly characterised by the following symptoms:—There suddenly appears a circumscribed uniform opacity in the posterior portion of vitreous humour; it is of a grayish tint, oscillates slightly, and is divided by a sharp line of demarcation from the transparent vitreous. The retina is found, on careful examination, to be at first not the least detached.

Detachment of the retina is another dangerous and not very unfrequent complication. The elongation of the sclerotic in sclerectasia posterior is accompanied by a corresponding stretching of the choroid and retina; but the latter, being less elastic and firmly attached than the former, does not follow this extension so readily; its connexion with the choroid becomes somewhat lax, and any serous or hæmorrhagic effusion readily produces its detachment. Or, again, the latter may be caused by the shrivelling up and contraction of membranous shreds of the vitreous which are attached by one extremity to the retina, and, in contracting, drag upon the retina and detach it at this point. At first the detachment of the retina may be but very slight and be only detected by very carefully tracing out the course of some of the retinal vessels, which will then be found, at the detached portion, to be slightly undulating, and to lie somewhat nearer to the observer. In such cases a very careful and accurate examination of the field of vision will prove of much assistance in the diagnosis, as a portion of the field corresponding to the detachment will be found somewhat impaired in its function; indeed, this is not unfrequently even a promonitory symptom of detachment of the retina.

Secondary glaucoma may also supervene upon sclerectasia posterior, and is especially dangerous from the fact that its progress may be so insidious, so free from all pain and inflammation, that it may quite escape detection until the disease has already considerably advanced, and produced great contraction of the field of vision, and perhaps a deep excavation of the optic nerve. This condition is the more apt to be overlooked as the central vision often remains remarkably good, even although the field is greatly contracted. This simple form of glaucoma is most prone to occur in those cases in which the sclerectasia posterior has existed without any inflammatory symptoms. With advancing years the sclerotic loses its elasticity, and becomes more firm and unyielding, and hence resists the tendency to bulging at the posterior pole. This produces stasis of the venous circulation within the eye, and compression and irritation of the ciliary nerves, which pierce the hinder part of the sclerotic, and the glaucomatous tension supervenes, leading gradually to contraction of the visual field, excavation of the optic nerve, and more or less loss of sight. This tendency to glaucoma is sometimes hereditary, and then shows itself early in life, and occasionally in several members of the same family. In other instances the glaucoma assumes from the commencement the character of serous iritis, and this occurs especially in those cases in which the sclerectasia has been accompanied by marked inflammatory symptoms of the choroid, vitreous, &c. But even in such cases the true nature of the complication may sometimes be for a time undetected, as the irritability and redness of the eye, periodic dimness of sight, and the pains in and around the eye, are but too often attributed by the patient, and even by his medical attendant, to simple inflammatory changes accompanying the sclerectasia posterior. Hence special attention should always be paid to the degree of intraocular tension if a patient suffering from sclerectasia posterior complains of increasing dimness of sight or inflammatory exacerbations. The increase of tension may be periodic, showing itself only during the presence of the inflammatory attack, and varying perhaps considerably in the course of a few hours. Such patients should, therefore, be seen at different times of the day, and especially during one of the inflammatory exacerbations, so that we may ascertain whether or not the eye is then in a glaucomatous condition.

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* Archiv fur Ophthalmologie, xv., 2, 57.

† Klinische Monatsblatter fur Augenheilkunde, p. 301. 1868.

If the choroidal atrophy around the margin of the disc is at all considerable, it may greatly mask the glaucomatous excavation of the optic nerve and the bending of the vessels over its edge. In other cases the cup is, however, well marked, and there may even be a double cup—one at the margin of the disc, and the other in the sclerotic at a distance of from a quarter to half a millimetre from the edge of the nerve.

The *prognosis* must depend chiefly upon the fact whether the sclerectasia is progressive or not, and whether the affection is accompanied by inflammatory symptoms. If there is only a slight, stationary crescent, and the degree of myopia but moderate, the prognosis is good; but it must be very guarded if the affection is distinctly progressive, if there are marked symptoms of sclerotico-choroiditis posterior, with opacities of the vitreous.

The *treatment* must principally consist in insisting upon the patient being careful not to use his eyes too much for reading, working, &c., especially by artificial light or in the recumbent position, as this is apt to produce intraocular venous congestion. Moreover, in order to prevent the strain necessitated by having to hold small objects very close, it may be advisable to give such patients concave glasses for reading, sewing, &c., so that they may be able to hold the book or work at a distance of twelve or fourteen inches from the eye; but if even moderate use of the eyes makes them irritable, red, and watery, and causes pain in and around the eyeball, we must strongly urge the patient to abstain from all work at near objects for some length of time. The flashes of light and the appearance of bright stars before the eyes, both of which are dependent upon irritation of the retina and optic nerve, are most relieved by blisters or a leech to the temple. But when the symptoms are more severe, and the patient complains of constant pain and a feeling of heaviness in the eyes, great benefit is experienced from the application of the artificial leech to the temple, which may be repeated for several times at intervals of six or eight days; but if the sclerectasia posterior is very considerable, the antero-posterior diameter of the eyeball much increased, and if there is any fear of detachment of the retina, the use of the instrument may be dangerous, as the sudden relief of the intraocular circulation may be followed by considerable temporary hyperæmia of the choroidal and retinal vessels, which might produce effusion of blood and detachment of the retina.

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Clinical Lecture

ON

HÆMOPTYSIS.

Delivered at St. Thomas's Hospital,

By Thomas B. Peacock, M.D., F.R.C.P.,

Senior Physician to the Hospital.

Gentlemen,—There are few affections which the practitioner is more frequently called upon to treat, and none which offer greater difficulties in the selection of their treatment, or are more alarming to the patients and their friends, than hæmoptysis. I have therefore thought that a few remarks on the subject would be not without use and interest to you, and the more especially as we have, during the summer and recently, had cases in the wards which bear upon several of the points on which I propose to speak.

Under the term of hæmoptysis, however, I must be understood to refer to cases of hæmorrhage proceeding from the substance of the lungs or bronchial mucous membrane, not to cases in which blood is poured into the tubes from the sac of an aortic aneurism.

Hæmoptysis, thus understood, may occur under the following different circumstances:—First: When there is considerable vascular excitement or active inflammatory action, while the loss of blood, instead of being, as in ordinary cases of pneumonia, a mere tinge, is entirely disproportionate to the amount of inflammation which exists. Secondly: When it arises in connexion with extreme congestion, temporary or permanent, of the lungs. Thirdly: When it occurs without having been preceded by any decided exciting cause, or after the operation of only some slight cause, and in persons who, though they may be at the time in good or average health, are predisposed, either hereditarily or otherwise, to consumption. Fourthly: When it arises in persons who are already the subjects of disease in the lungs to a more or less marked extent.

The symptoms of the disease, the course which it is likely to run, and, to a considerable extent, the treatment which it should receive, will vary in each of these different forms.

The first form of hæmoptysis is generally seen in persons who have been much exposed to cold or damp, or to the two combined. I have known it occur after bathing, and in persons who had sustained hardships during stormy weather at sea. In these cases the hæmoptysis is not generally profuse, but the blood which is expectorated is very viscid, probably from containing a large proportion of fibrin, and is mixed with the products of inflammation. The inflammatory action may affect chiefly the cells of the lungs or bronchial mucous membrane, but more frequently the whole substance is more or less involved. The hæmorrhage which occurs in acute plastic bronchitis, on the separation of the exuded false membrane, affords another example of this form. I have not myself seen any case of the kind, but various instances are on record, and I may particularly refer to two cases mentioned by Sir Thomas Watson. In this form of hæmoptysis, in addition to the signs caused by the presence of blood in the lungs, there will also be the usual evidences of bronchitis or pneumonia, together with the furred tongue, the flushed face, the dry skin, and the increased temperature which attend these diseases.

The second form of the affection occurs in connexion with mitral obstruction and regurgitation, and especially with the latter, and is usually combined with pulmonary apoplexy. The blood expectorated in these cases is generally dark-colored and airless. It is not brought up in large quantities, and is usually combined with other secretions, such as are thrown out in cases of chronic bronchitis, broncho-pneumonia, and œdema of the lungs. Blood also is expectorated under similar circumstances in cases of imperfect expansion of the lungs combined with deformity of the spine.

The third and fourth forms of the affection are those which are of most common occurrence. In the first of these the expectoration is often very profuse, and the blood at first brought up is very fluid and contains much air. Subsequently it is often coagulated, dark-colored, and airless. The expectoration rarely occurs to a marked extent at first, but generally there is a little blood brought up, perhaps only a few drops, or a teaspoonful of fluid blood, or a small coagulated mass; and this may be followed, after an interval of a few hours or a day or two, by a similar but larger expectoration, and that again perhaps by a very profuse hæmorrhage, which takes place rapidly, so that the patient will expectorate the blood constantly for some minutes or hours. Generally an expectoration of this kind arises after some failure of health; the patient has been languid or feeble, and his appetite and digestion impaired, suffering from a little cough, with or without expectoration, and has been noticed by his friends to be falling away in bulk for some little time. Under these circumstances, the first expectoration may occur without having been preceded by any obvious exciting cause, or only after some slight exposure to cold. In other cases, the hæmorrhage occurs in persons quite in good health, and then generally follows the operation of some decided exciting cause—playing at cricket, footfall, or rowing, or lifting heavy weights; or it takes place in persons of dissipated habits after a debauch. It is rare that an expectoration of blood occurs suddenly, and equally suddenly ceases. Generally after the bleeding has come on somewhat gradually in the way described, and has continued for hours, or a day or two, it ceases, either entirely or partially, and recurs again, though to a smaller extent, in some hours, or in two or three days, or a week; and generally in the interval, and after the final cessation, blood, evidently of old date, is expectorated. Should a copious hæmorrhage of blood suddenly occur, and equally suddenly cease, it will most probably have proceeded from the sac of an aneurism which has formed a connexion with the trachea or larger bronchial tubes. During the process of cessation the sputum generally continues bloody for some days, either the whole mass being more or less deeply tinged, or the matters expectorated consisting partly of bloody sputum and partly of ordinary bronchitic or pneumonic expectoration, having a more or less dark color. Very often the sputum, especially when the hæmorrage has been profuse, is very offensive to the smell. Occasionally, when there has been large loss of blood, after some time has elapsed the patient will cough up a distinct moulded and partly decolorised coagulum, or, as it was termed by the older pathologists, a "bronchial polypus."

The results of stethoscopic examination vary in these cases. Supposing the lungs to have been previously free from disease, during the active period of the hæmorrhage somewhat small crepitation is heard, though the sound is less fine than that which indicates the first stage of pneumonia, and the resonance on percussion will be little if at all impaired. Supposing the hæmorrhage to go on, and the blood not to be entirely expectorated, a portion of the lung, generally under one clavicle, becomes consolidated; the movement of the parietes is almost entirely abolished; there is entire dulness on percussion; and the respiratory sounds are almost absent, or there is decided bronchial respiration. This dull space may be very extensive, occupying, perhaps, the whole of one upper mammary region, and generally, but not invariably, around the margins of the dull space, crepitation of a more or less fine character is heard. Subsequently, as the blood is expectorated or absorbed, the air again enters into the consolidated portion, and sounds similar to the returning crepitation of pneumonia become audible. I have occasionally known the anterior portions of the chest to yield a peculiarly clear note on percussion—to become, indeed, quite tympanitic, while the chest movement is almost abolished, and the respiratory sounds are nearly inaudible. The explanation of this peculiar condition is, I think, to be found in the obstruction of the posterior part of the lungs by the extravasated blood gravitating to the back as the patient occupies the recumbent position, and so causing the air more fully to inflate the anterior portions, while the obstruction in the tubes prevents the escape of the air which has entered into the distended cells. When, after a time, the removal of the blood from the posterior part of the chest allows the air again to enter into these portions of the lungs, the undue inflation of the other part ceases, and the respiratory movements and sounds return without the occurrence, perhaps, of any crepitation or any other morbid sounds being heard in the inflated portion of the lungs. Of course, after an attack of this kind there is more or less constitutional disturbance, for not only is the respiratory process interfered with by the extravasated blood, but there is also a rent in the substance of the lung, which is necessarily followed by inflammatory action: hence the pulse is quickened; the breathing rapid and short; there is a troublesome cough, often both severe and frequent; and the temperature is raised. As these attacks generally occur in persons of debilitated habit, they are attended by considerable prostration of strength, the fever has a tendency to assume the hectic type, characterised by a low temperature and great feeling of exhaustion in the morning, and by marked increase of heat and aggravation of all the symptoms at night.

It has been thought that hæmorrhage of this kind is necessarily connected with some deposit in the chest, but I am well convinced that it is not always so. I have seen cases in which the hæmorrhage has been fatal, and no appearance of tubercle has been found in the lungs; and, judging from clinical experience, I should regard such an occurrence as not uncommon; certainly cases of hæmoptysis are not unfrequently met with in which the lungs, if not absolutely sound, do not present evidences of material disease.

The last form of hæmoptysis is also of very frequent occurrence. In this the hæmorrhage is not generally profuse; often it only amounts to some streaks or specks in the sputum, or to the expectoration of a few masses of deeply tinged sputum or of blood; but it is generally of this description because the bloodvessels in the neighborhood of cavities in the lungs are ordinarily occluded by adherent coagula. Occasionally, however, a large expectoration of blood takes place where there is decided disease of the lungs. Thus in some cases, in consequence of the formation of a small cavity in the neighborhood of one of the large branches of the pulmonary artery, the coats of the vessels, being deprived of support, become expanded, and so a small aneurism is formed, which giving way allows a large escape of blood to take place. This, I believe, generally occurs in early and rapidly progressing phthisis. In other cases of advanced phthisis copious hæmorrhage may also occur from a vessel, probably of only small size, giving way. The blood oozing from it gradually fills the cavities, and is then expectorated in large quantities, simulating the sudden outpouring of blood by a large vessel. In cases of this description the physical signs will differ from those in the second form, as, in addition to those which may result from the outpouring of blood, there will be evidence of more or less previous disease in the lungs.

The immediate sources of danger in cases of hæmoptysis are—first, shock or syncope from the sudden and rapid occurrence of profuse hæmorrhage in a healthy person, or from the escape of smaller quantities of blood in a person previously in delicate health or the subject of disease; and, secondly, apnœa, from the accumulation of blood in the lungs which the patient is not able to expectorate. It is, however, rare that the quantity of blood discharged at one time is so great as to destroy life by the shock which it occasions, if the attack occur in a person previously healthy; but in those who are the subjects of disease and already much reduced, such a result may readily ensue. So also the engorgement of the lungs from inability to bring up the blood which is thrown out, and consequent death from apnœa, is more likely to occur in a person previously phthisical, Most generally, however, death is the result of a combination of these causes, aggravated by the alarm which the hæmorrhage creates; and thus patients often die after comparatively small losses of blood, and in whom there has occurred but little bleeding into the lungs.

The more remote sources of danger are—first, gradual sinking from repeated small losses of blood; secondly, the development of low inflammation affecting the mucous membrane of the bronchi, or the substance of the lungs, and more generally both; and, thirdly, and at a still more distant period, the supervention of phthisis. Of these the second is, perhaps, the most frequent cause of the fatal result. It often happens, in cases where the patient has sustained very large losses of blood but where the tendency to hæmorrhage appears to have subsided, that after a few days have elapsed inflammatory symptoms supervene and rapidly carry off the patient. In all cases, as there is necessarily some injury to the texture of the lungs, inflammatory action must, however, ensue to a greater or less extent, and often there is a more general congestion of the lung, probably due to chill and exposure.

The prognosis in any given case of phthisis must be founded on the following considerations:—1. On the amount of blood which has already been lost, the degree of prostration which it has occasioned, and the ability of the patient to sustain a further hæmorrhage should it occur. 2. On the condition of the lung as affected by the hæmorrhage which has occurred, and the facility with which the blood effused has been expectorated. 3. On the previous state of health of the patient, as rendering it probable that the lungs were before in a healthy state, or more or less diseased. 4. On the family and personal history, as indicating the existence of a predisposition to phthisis, which may entail danger of the supervention of that disease, even though the hæmorrhage has been very slight, and the lungs at the time of the attack were entirely free.

It is often very difficult, when the practitioner is called to a patient who is spitting blood, and with whose previous state he is unacquainted, to ascertain the condition of the lungs before the occurrence. The engorgement of a portion—most generally the upper part,—on one or both sides, may give rise to all the signs of consolidation, and may lead to the conclusion that the patient was laboring under serious disease before the occurrence of the hæmorrhage; yet this conclusion may be incorrect. With the progress of the case the signs may wholly disappear and the patient may entirely recover, with little or no evidence remaining of any disease of the lung. On the other hand, the peculiar inflation of the anterior part of the chest, consequent apparently on the engorgement of the posterior portions, may entirely cover the dulness on percussion, which would otherwise have been occasioned by a limited deposit of tubercle or other kind of consolidation. If, however, the loss of blood has been small, and the patient is but little reduced by it, a very cautious prognosis should always be given; for at any time, when least expected, a repetition of the hæmorrhage may occur; and in cases where there is reason to believe that the lungs were sound previous to the attack, and where the predisposition to phthisis is but slight or altogether absent, that disease may, nevertheless, sooner or later occur. Of course, however, the immediate danger is greater in proportion to the extent of the hæmorrhage and the degree of prostration, and the ultimate danger of the supervention of consumption or of the aggravation of any disease which exists, is equally increased by these causes and by the existence of a decided predisposition to the disease. In no cases, however, does phthisis more surely and rapidly supervene than when repeated attacks of hæmorrhage occur at short intervals, though the amount of blood lost in each attack may only be trivial.

It is evident that the treatment which should be adopted in cases of hæmoptysis must vary considerably according to the circumstances under which the discharge of blood occurs. In the first form of the affection, or that in which the hæmorrhage occurs in connexion with considerable vascular excitement or actual inflammatory action, the treatment must be chiefly directed to the relief of that condition; though it may be necessary to employ remedies calculated directly to arrest the bleeding in conjunction with such treatment, or after the excited action has subsided. In such cases, therefore, we must employ sedatives, together with remedies that reduce the condition of the blood, and promote elimination, especially by the skin and kidneys; and I believe, though I have not myself of late years adopted the practice, that general bleeding to a moderate extent would in cases of this kind be often beneficial.

In cases in which the blood is expectorated from the rupture of vessels very greatly distended in consequence of obstruction to the flow of blood through the lungs, in connexion with mitral valvular disease or otherwise, the loss of blood is not generally so large as to be in itself very important, though the lungs are often still further embarrassed by the infiltration of the blood into their tissue. In these cases the attention must mainly be directed to relieve the condition upon which the disease is primarily dependent. We should chiefly employ eliminants, so as, by promoting the flow of the urine and the secretion from the alimentary mucous membrane, to lessen the amount of the circulating fluid and relieve the general congestion. It is questionable whether in some of these cases the greatest relief would not be obtained by blood-letting to a moderate amount. This I have two or three times tried, and certainly in one case with very striking benefit. Most generally, however, the indication is to be carried out by the use of purgatives and diuretics combined with medicines calculated to give tone to the muscular structure of the heart. It is in these cases that digitalis is often found eminently useful, acting as it does as a diuretic, and, if not too long continued, giving strength to the pulse, and reducing its frequency. On this subject what I have said elsewhere has been somewhat misunderstood. I cannot doubt that the first effect of the exhibition of digitalis is to quiet the action of the heart and arteries, but I feel well convinced that this effect is only of temporary duration, and that, if the remedy be long continued, it exercises a generally depressing power, under which the pulse again rises. I believe digitalis to be eminently useful, partly by its sedative and partly by its diuretic action, especially in those cases in which, with mitral incompetency, there is dilatation and weakness of the left ventricle, giving rise to great irregularity in the action of the heart, and generally combined with dropsy.

In reference to the third and fourth forms of hæmoptysis, they are mainly to be met by sedative and astringent treatment.

In all cases it is of the greatest possible importance that entire rest and absence of all excitement should be enforced; and this whether the hæmorrhage be only slight or more copious. It must be borne in mind in cases where for the first time a small hæmorrhage occurs, that such often precedes the outpouring of a large amount of blood, and every precaution should therefore be taken. It is also desirable that these precautions should be continued for some time, as it is quite impossible to say when the hæmorrhage may not recur. After copious hæmorrhage it is better not to disturb the patient for the sake of obtaining a good examination of the chest; the information so gained may be very valuable as furnishing the basis of a correct prognosis, but it may be gained at too great a cost. I have seen the discharge of blood brought on by simply raising the patient so as to examine the chest, and a fatal result all but occasioned. For the immediate checking of the hæmorrhage, the spray of tincture of perchloride of iron is often very useful; and the presence of the medical attendant, and his calmness and composure when the friends and attendants are in the greatest alarm, will often do much to encourage the patient, and to lessen the loss of blood which would otherwise be sustained.

The remedies to be applied must vary with the nature of the case. If the hæmorrhage be only slight, and the patient previously in good health, mild astringents, with slight sedatives, such as the sulphuric acid and sulphate of magnesia, may be all that is needed; but when the hæmoptysis is profuse, the more powerful sedative styptics must be had recourse to, as the acetate of lead or gallic acid, and especially combined with opium to quiet the nervous excitement. In cases, on the other hand, in which the patient is previously feeble or much reduced, the more stimulating astringents, as the oil of turpentine, must be employed. The food given must be mild and unstimulating but nutritious, for few persons who suffer from hæmoptysis are really robust and capable of bearing much reduction. The food also is generally given cold or cool, and it is probably best to do so; but I am not satisfied that much benefit results from swallowing ice. It cannot, I think, exercise much constringing on the vessel which is bleeding in the lungs, and may cause gastrodynia or diarrhœa. Still less do I approve of the application of cold water or ice to the exterior of the chest. It seems improbable that such application can have much effect in checking the hæmorrhage, while it may, by increasing internal congestion, promote it; and it must certainly, by chilling the patient, be liable to cause inflammation of the lungs, which, it has been before said, is very apt in all cases to arise after pulmonary hæmorrhage, and not unfrequently occasions the fatal result.

Whatever be the course of treatment which it is thought right to adopt at the commencement of an attack of hæmoptysis, sooner or later it is generally necessary to have recourse to more stimulating and supporting measures. Thus, if the hæmorrhage continue, in place of sedatives the stimulating and tonic styptics must be given in combination with more decided support and the diffusible stimulants. It is often difficult to decide when the time has arrived for this change to be made, and it may be necessary to adopt at first a somewhat tentative plan. Thus ammonia may be given, or brandy diluted with water, beef-tea, or in arrowroot, in very small doses, not more than half a teaspoonful or a teaspoonful, may be administered. If the pulse become quieter, there can be no doubt of the propriety of continuing the treatment, the doses being exhibited at more or less frequent intervals according to the urgency of the symptoms and the effects produced. I have thus frequently seen hæmorrhage checked which was previously but little influenced by the treatment adopted. In some cases, and especially where the hæmorrhage takes place into the lungs, and the patient, from weakness or other cause, is incapable of expectorating the blood, stimulants must be given most freely, and form indeed the only chance for the life of the patient. Above all things in such cases the most entire quiet must be enforced. Great care is often needed in exhibiting aperients. I have known alarming and almost fatal sinking caused by their action. The bowels should be relieved, but it is safest to accomplish this by the use of enemata.

In cases of the fourth form of blood-spitting, when there is already extensive and advanced disease of the lungs, and the patient's strength is much reduced, the treatment must be cautiously adapted to the amount of the hæmorrhage and the power of the patient. In some such cases the hæmorrhage depends upon some temporary congestion from exposure to cold, and if only to a slight extent, it may be very much left to itself, or even met by some mild eliminating treatment. When, however, the bleeding is at all profuse, powerful stimulating styptics must be given. The support and stimulus which the patient was previously taking must also be very cautiously reduced, or it may be necessary still to continue it, or even to increase the quantity. The most complete absence of excitement must be enjoined.

The after-treatment of cases of hæmorrhage rather falls under the course to be adopted for the prevention of phthisis, and has already been alluded to on a former occasion; it is therefore not necessary to dwell upon it at present. I may, however, remark that the danger of the occurrence of subsequent disease of the lungs being super-induced is greater in proportion to the large extent of the loss of blood which has been sustained, the prostration which it has produced, and the evidence of previous disease of the lungs, or of predisposition to phthisis, hereditary or acquired. Great caution also is needed, in carrying out the subsequent course of treatment and regimen, that the patient should not suddenly become plethoric, that all active exertion of body or excitement of mind should be avoided, and especially that he should be carefully protected against the influence of cold. It is always desirable, if the patient's circumstances allow of it, that he should spend the first winter following an attack of spitting of blood in a warm climate or warm situation; or if he be not too weak and be a good sailor and not liable to suffer from sea-sickness, he should take a sea voyage, so as to enjoy the invigorating effects of fresh air and change of scene without fatigue or undue exertion. It is undesirable that he should travel much by land, lest he should so take cold or have a return of the hæmorrhage; and this is especially to be avoided in countries where, in the event of any serious symptoms supervening, he would be incapable of obtaining adequate medical care and attention.

Original Papers.

CONTRIBUTIONS
ON THE
PHYSIOLOGY & THERAPEUTICS OF FOOD.

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No. I.

INTRODUCTORY REMARKS ON THE DYNAMIC RELATIONS OF FOOD.

The discoveries and inductions of the present age have thrown new light on the physiology of food.

Around us we have to deal with Matter and Force: the one a substantive entity; the other recognisable only as a principle of action. It has long been known that, as cognisable in our own era, matter can be neither created nor destroyed. It may be variously combined and modified; but it remains the same in essence and unaltered in amount. Force also has, more recently, been recognised as similarly conditioned; and in order that the bearings of food in relation to this principle may be understood, some preliminary considerations explanatory of the views now entertained regarding it are necessary.

First, then, we may take it as accepted that, like matter under present circumstances, force can be neither created nor destroyed. "Ex nihilo nihil fit," and "Nihil fit ad nihilum," are axioms received by the mind as incontestable. If we except the inconsiderable accession derived from the occasional descent of a meteoric body, the earth's matter remains fixed in amount. It is otherwise, however, with respect to force. Under the form of heat and light, force is constantly being transmitted to us from the sun; and it is from the force thus derived that, in a manner to be explained further on, life on earth originates and is sustained.

Under the title of "Correlation of Physical Forces," Grove demonstrated that one kind of force was capable of producing another. His views were first made known at a lecture delivered at the London Institution in 1842. The word "correlation" he employed as meaning "reciprocal production—in other words, that any force capable of producing another may in its turn be produced by it." The position sought to be established was that heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative, or are reciprocally interdependent. As a force, either might produce the others; from which it flows as an incontestable inference that a force cannot originate otherwise than by generation from some antecedent force or forces.

Just at this time the same field was being investigated by other workers. While Grove was asserting that the great problem awaiting solution in regard to the correlation of physical forces was the establishment of their equivalent of power, or their measurable relations to a given standard, Mayer, Joule, and Helmholtz were announcing the actual equivalents themselves.

Mayer, of Germany, had the priority in the publication of his researches. As a medical man he approached the subject through his interest in the solution of physiological questions. In 1842 he proclaimed, in its full comprehensiveness, the doctrine conveyed under the title "Conservation of Force."

Nearly at the same time Mr. Joule, of Manchester, discovered in mechanical motion the equivalent of heat. He had been led to prosecute researches in that direction with the view of ascertaining the relative value of heat and motion in relation to engineering science. He found that what sufficed to raise the temperature of a pound of water one degree Fahrenheit, would, under another mode of action, raise 772 pounds a foot high; or, putting it conversely, the fall of 772 pounds of water through one foot of height would give rise to an amount of heat sufficient to elevate the temperature of one pound to the extent of one degree Fahrenheit. Thus the mechanical work corresponding to the elevation of 772 pounds a foot high, or, what comes to the same thing, one pound 772 feet high, forms the dynamic equivalent of one degree of heat of Fahrenheit's scale.

It is necessary to state here that the term "force," when used in a strict sense, is employed under a more limited acceptation now than formerly. Originally, it represented what is now distinguished as both "force" and "energy." By "force," in a rigid signification, is understood the power of producing energy; by "energy" the power of performing work. To give an illustration: powder has force, the cannon-ball energy; but to speak of the force of the cannon-ball is inexact. I may also remark that the words "actual" and "potential" are in frequent use to qualify the state in which energy is met with. By actual energy is meant energy in an active state, energy which is doing work. By potential energy, energy at rest—energy capable of doing work but not doing it. In a bent crossbow there is potential enery, energy in a state of rest but ready to become actual, or to manifest itself when the trigger is pulled. Again, actual energy is evolved from the sun. By vegetable life this is made potential in the organic compounds formed. In these organic compounds the energy is stored up in a latent condition; potential energy is reconverted into actual energy when they undergo oxidation during combustion or in their utilisation in the animal economy.

The doctrine of conservation of energy implies that energy is as indestructible as matter; that a fixed amount exists in the universe; and that, however variously it may be modified, transferred, or transformed,—in spite of all the changes of which it may be the subject throughout the realm of nature,—it cannot be created or annihilated, increased or diminished. The doctrine further implies that the different forms of energy have their definite reciprocal equivalents; that so much chemical energy, for instance, will produce so much heat, which is the representative of so much motive power, and so on. The ascertained equivalents of heat and motive power have been already given.

Accepted as applicable to the physical forces, the doctrine of conservation next began to be applied to living nature. Grove in his "Correlation of Physical Forces" suggested (second edition, p. 89) that the same principles and mode of reasoning adopted in his essay might answer equally for the organic as for the inorganic world; and that muscular force, animal and vegetable heat &c., might, and one day would, be shown to possess similar definite correlations. He proceeds no further, however; but only remarks that he purposely avoids a subject not pertaining to his own field of science.

At this time the general belief prevailed that the processes going on in the living body were determined by "vitality," or the "vital principle." The physical forces, it was supposed, were overruled in the living body by the vital principle. Without discussing whether we are to admit or deny this principle as a distinct operating force—a question which has been handled by some of the leading men of science of the day,—we must, I think, concede, as a matter of experience, that in the living organism there are influences in play which have no existence in the dead matter around. Matter which has been impressed with life can produce effects which dead matter cannot. This does not conflict with the extension of the law of conservation of energy to living nature. The effects produced may have their origin in the physical forces—the living matter forming the medium through which they operate. With artificial contrivances force may be made to produce various effects, according to the nature of the instrument employed. With the same force in operation different kinds of work are performed, according to the character of the machine set in motion. Between the two—living matter and a machine—there exists an analogy which admits of being followed still further. It is only when in a certain state that matter is capable of forming the medium for the exercise of force in the production of living operations. Modify this state, and though there may be the same matter to deal with, yet it is no longer capable of fulfilling the same office it before performed. So in the case of an ordinary machine: it must possess a particular construction before it can form the medium for the operation of force. Disarrange this construction, and, although the matter remains unchanged, the application of force is quite without its proper effect. Thus a disarranged machine may be compared with living matter devitalised. In both the capacity of being set in operation by force has existed, and in both that capacity has been lost. Further, it may be said that a machine in working order, but unoperated on by force—that is, in a state of rest,—is like matter possessing vitality, but in a dormant state. Both are ready to move directly the proper force is supplied.

Applying the law of the conservation of energy to living nature, the forms of force which we observe in operation are in the first instance derived from the sun. When a weight is lifted by the hand, to go to the sun for the muscular force employed in the act seems a long journey. Yet the doctrine of the conservation of energy justifies, as I will proceed to show, the conclusion that its origin is there.

In the first place, the force evolved in muscular action has its source in the material which has been supplied to the body in the form of food. Now all food comes primarily from the vegetable kingdom, and vegetable products are built up through the agency of the sun's rays. It may be said that the energy contained in these ray, which has been employed in producing the compound, is fixed or rendered latent within it. When a crossbow is bent, the force derived from the muscular action employed in bending it is stored up, ready to be again liberated when the trigger is pulled—no matter whether this be at once or a hundred years hence; and the force given to the arrow when it is launched is neither more nor less than that which has sprung from the muscular action employed in bending the bow. The same with vegetable products. Their formation is coincident with the disengagement of oxygen from oxidised principles and the development of combustible compounds. To effect this disengagement the operation of force is required. Now the force so employed has its source in the heat and light evolved from the sun, and that which is used for the purpose may be said to become fixed and to exist in a latent condition—to exist stored up in the product, ready to be again liberated when exposed to conditions of oxidation. Thus may these vegetable products be compared to a bent crossbow, containing as they do a store of latent force which may for an indefinite period remain as such, or may be liberated soon after it has been fixed. Whenever liberated, it is no more nor less than the equivalent of the force which has been used in the formation of the product. Our coal-fields represent a vast magazine of force drawn, ages ago, from the sun's rays, and capable at any moment of being set free by the occurrence of oxidation.

Vegetable products, then may be regarded as containing a store of force accumulated from the mighty supply continually emitted with the sun's rays; and, upon the principle of the indestructibility of force, that which has been employed in unlocking the elements in the combinations from which vegetable products are built up, and forming the new compound, is contained in the latter in a latent state. Now, as above stated, animals either directly or indirectly subsist upon these vegetable products and are supplied, in them, with accumulated force. By oxidation the force is set free in an active state under some form of manifestation or other. It matters not in what way, whether rapidly or slowly, or under what circumstances, whether inside the living system or outside the living system, the oxidation occurs. The result is the same, as far as the amount of the force liberated is concerned, it being implied in the doctrine of conservation of energy that it should constitute the equivalent of the solar force originally drawn upon. This is presuming complete oxidation to occur; but in the processes of animal life although carbonic acid and water are formed, and these constitute fully oxidised products, yet urea is also a final product, and this, being an incompletely oxidised compound, contains a certain amount of latent or unutilised force.

Thus it is that the various forms of force manifested in the actions of animal life trace their origin to that emitted from the sun. Plants are media for fixing solar force—for converting actual into latent or potential energy. Animals reconvert latent into various forms of actual force. Thus, in the various forms of actual force liberated by the actions of animal life, we have the equivalent of the force which has been fixed by plants from the sun. As there is a revolution of matter, so is there a revolution of force in the universe within and around us.

In the liberation of actual force a complete analogy may be traced between the animal system and a steam engine. Both are media for the conversion of latent into actual force. In the animal system, combustible material is supplied under the form of the various kinds of food, and oxygen is taken in by the process of respiration. From the chemical energy due to the combination of these, force is liberated in an active state; and, besides manifesting itself as heat, and in other ways peculiar to the animal system, is capable of accomplishing mechanical work. The steam-engine is fed with combustible material under the form of coal, which differs from our food only in representing the result of the vegetative activity of a former, instead of that of the vegetative activity of the present, epoch. Air is also supplied, and from the combination which occurs between its oxygen and the elements of the combustible material, heat is produced, which in part is dissipated as such, but in part is applied to the production of mechanical work. According to Helmholtz, the animal economy, in respect of its capacity to turn force to account in the accomplishment of mechanical work, is a more perfect instrument than the steam-engine. His calculations lead him to conclude that whilst in the best steam-engine only one-tenth of the force liberated by the combustion of its fuel is realisable as mechanical work, the rest escaping as heat the human body is capable of turning one-fifth of the power of its food into the equivalent of work. On the other hand there is this to be remarked, that the fuel of a steam-engine is a far less expensive article than the food of an animal being.

The animal body, then, may be regarded as holding an analogous position to a machine, in which a transmutation of chemical into other forms of force is taking place. Food on the one hand, and air on the other, are the factors concerned in the chemical action that occurs. It is through the interplay of changes between food and air that the manifestations of animal life, consisting of heat-production, muscular contraction, nervous (including mental) action, and nutritive or formative, secretory and assimilative action, arise. The egesta, or substances dismissed from the system, are metamorphosed products of the ingesta, or substances entering the system. The elements are the same, and also their quantity, in the two cases; but their forms of combination, and, with them, their relations to force, are different. The force expended in the building up of the organic compounds belonging to food is again evolved as these compounds descend, through the action of oxygen, into simple combinations; and in the force evolved we have the simple representative of the active manifestations of animal life. If the products discharged from the system were fully oxidised principles, the force developed in the body would equal that contained in a latent condition in the food. Such, however, is not completely the case, and some latent force remains in the egesta. The changes may be formulated thus: the latent or potential force of ingesta = force developed in the body + the force escaping with the egesta. In other words, the unexpended force in the egesta and the force disengaged by the operations of life, and manifested under the various forms of vital activity = the force contained in the ingesta.

What is required in food, then, must consist of material susceptible of undergoing change in the system under the action of oxygen. Life implies change; and the manifestations of life are due to the interaction of food (as the primary material) and air. While in the organic kingdom a tendency to a state of rest prevails—while the closest affinities tend to become satisfied, and so establish equilibrium; in a manifestly living body rest is impossible. It is true, living organisms under certain conditions may exist in a state of rest; but then there is a suspension of vital manifestations. The state is known as one of dormant vitality, and the seed of a plant naturally remains for a while in this condition. There are also animal organisms which may exist in a similar state. Molecular rest and, with it, an absence of any show of vital activity prevail. Concurrent with the manifestation of vital action molecular change in a particular or prescribed direction occurs. The higher compounds become resolved by the agency of oxygen, the other factor in life, into simple combinations as carbonic acid, water, and urea, and with this the capacity for further service is exhausted. To maintain a continuance of vital action fresh material is required; and hence the demand for food. But food by itself, although in association with oxygen, does not suffice for the accomplishment of what is required. It is necessary that it come within the sphere of the influence of living matter, and this it is which determines the particular line of direction which the changes assume, and in which originate the phenomena of life.

No. II.

THE SOURCE OF FOOD.

The vegetable kingdom forms the primary source of our food. Dumas even at one time affirmed—"L'animal s'assimile, donc, ou détruit des matières organiques toutes faites; il n'en crée donc pas." This, however, as he afterwards admitted, does not strictly hold good. The animal no doubt is incessantly consuming or destroying organic substances, and is incapable of forming them from the inorganic principles; but, if supplied with organic matter, organic compounds of various kinds are constructed.

Mulder's discoveries in 1838 led up to the doctrine that the albuminous compounds of plants and animals agree in composition and properties; whence it was inferred that the animal simply took the compound of the plant and made it a component part of its own body. Liebig, however, was the first to maintain that animals possessed the power of forming one kind of organic compound out of another. Upon this arose a controversy, with particular reference to fat. In opposition to Liebig's teaching, that in the animal system fat was producible from sugar, Dumas and Boussingault held that whatever fat was found in an animal being came through its food from without. It is now, however, matter of history that the French authorities have had to abandon their doctrine in favor of the German.

One kind of albuminous compound, it is almost needless to say, can be constructed from others. In the young mammal, subsisting on milk, the caseine is the source of fibrin and albumen; in the animal feeder, when excreting milk, the casein comes from the fibrin and albumen of the food. Gelatin is not found in vegetable food. None of these nitrogenous principles (it is now established) enter the system in the form in which they occur as food. During the digestive process they are all converted into albuminose, which is the principle absorbed and subsequently transformed by the animal's assimilative powers into the various principles met with.

The capacity really existing in animals is that of transforming one organic compound into another. All organic matter has its origin in the vegetable kingdom, from which, therefore, all our food is directly or indirectly derived. The vegetable feeder goes at once for its food to the vegetable kingdom. The animal feeder is not less dependent on the products of the vegetable kingdom for its pabulum. But this it obtains, so to speak, in a second-hand way only; its food consisting of the flesh of animals which have themselves been nourished on vegetable products.

Plants grow under the action of the sun's rays, to the influence of which we must refer the production of food in the first instance, and the source of all terrestrial life. I have already shown how the energy emitted from the sun, as heat and light, is able, through the medium of the plant, to disengage oxygen from its combination with carbon and hydrogen in carbonic acid and water and lead to the formation of re-oxidisable compounds; and how the energy evolved from the re-oxidation of these compounds, whether by combustion or within the animal system, represents the equivalent of that employed in effecting their construction.

We cannot help being struck by the immensity of the amount of power that must have been emitted from, and is still stored up in, the sun, when we come to consider that from the small pencil of rays which has impinged upon our earth at a distance of nearly 95,000,000 miles has been derived all that energy, or source of power, which, under the view now held by philosophers, has been fixed by past and present vegetation, and much besides that has escaped being so utilised.

Geology, indeed, teaches us that early in the history of the globe solar influence must have manifested itself in a much stronger degree than it does even at present. The vast coal-beds in the earth's crust are vegetable in their origin, and carry us back to a time when, under solar influence operating on an atmosphere surcharged with carbonic acid and humidity, the most luxuriant vegetation existed, and even so in the arctic regions, as coal-deposits are there to be found.

Now, the force of the sun's rays co-operating with the action of the plant leads to the building up of organic compounds, and it is in the green parts of the plant—those containing chlorophyll—that the decomposition of carbonic acid and water occurs, accompanied with the fixation of carbon and hydrogen and the liberation of oxygen. This is the plant's distinctive function, which ceases to be performed when light is absent, or when no chlorophyll exists. Under these latter conditions—absence of light and chlorophyll—oxygen is absorbed, and carbonic acid set free, as in the animal being. I have been informed that it is a fact known to florists that, in the case of the variegated-leaved geranium, a slip that happens to possess white leaves only, will not grow, like other slips, alone. The absence of chlorophyll explains the non-capacity to effect the changes necessary for independent growth.

The solar beam consists of rays having different properties and degrees of refrangibility; and the question has been raised, what part of the solar spectrum exerts most power over vegetable growth. The order in which the colored rays, producible by passing a beam of light through a prism, are arranged, is as follows: violet, indigo, blue, green, yellow, orange, red. The brightest yellow rays contain the highest illuminating power of the spectrum. The greatest heating power is in rays below the red, and therefore of less refrangibility than any of the colored rays; whilst the violet, and still more the invisible rays just above, contain the highest chemical power.

In 1843, Draper satisfied himself, by causing plants to effect the decomposition of carbonic acid in the prismatic spectrum, that the yellow rays were by far the most effective; and the relative power of the different colored rays was said by him to stand as follows: yellow, green, orange, red, blue, indigo, violet. Others maintain, on the contrary, that it is to the blue and violet rays that must be referred the maximum power of effecting the decomposition of carbonic acid through the medium of the plant. Helmhóltz, for example, says,* "The observations upon vegetable life have shown that plants can only grow under the influence of solar light, and that as long as solar light, and principally the more refrangible parts of solar light, the blue and violet rays, fall upon the green parts of plants, the plants take in carbonic acid and exhale oxygen." In exerting this influence these rays are completely absorbed—solar light which has passed through fully developed green leaves being no longer capable of exerting any chemical influence.

I have spoken of light as a factor in the construction of organic compounds by the plant. The elements of which these organic compounds consist are drawn from the inorganic kingdom, and chiefly, as Liebig pointed out, from carbonic acid, water, and ammonia—principles which exist to a greater or less extent in the atmosphere, and from the atmosphere are always to a large extent, and sometimes entirely, obtained by the plant.

The stages passed through in the history of vegetable life towards the provision of fitting food for animal existence may be thus represented. Beginning, say, with a barren surface freshly exposed to the atmosphere from some volcanic or other agency, the germs of low vegetable organisms settle upon it, and abstract from the atmosphere their appropriate elements of growth. Passing through their term of life, they die, and fresh ones subsequently spring up, similarly to live and die. So the process goes on, higher and higher forms making their appearance. The decaying remains of this primitive growth encrust what was a barren surface with mould, in which ultimately the highest plants find a suitable situation for taking root and growing. Thus clothed with vegetation, a fit locality is found for the support of animal life, animal beings finding in the vegetable products they meet with the necessary material for their subsistence.

Such is the general position occupied by the vegetable kingdom in relation to the production of food for animal beings; but there is one tribe of plants, it may be remarked—viz., the fungi,—which seems to stand in an exceptional situation, and to resemble in phenomena of life animal rather than vegetable organisms. This group of organisms, for instance, is not dependent in the same manner as other vegetable growths upon the influence of light; has no green surfaces for decomposing carbonic acid; seems, like animals, to subsist upon organic matter; and further, like them, abstracts oxygen and sets free carbonic acid. The appropriation of organic matter may, however, be more apparent than real, for the growths in question live only where decay is going on; and if it is not actually the inorganic products of decay that are applied, there is nothing to show that any other than organic compounds in a state of decomposition can be made use of.

No. III.

ALIMENTARY PRINCIPLES.

Of the various elements in nature only a limited number enter into the constitution of the human body; and of these, carbon, hydrogen, oxygen, and nitrogen occur in much larger quantity than any others. Food being the source from which the elements forming the constituents of the body are derived, it follows that no food can meet the requirements of life that does not contain all the elements existing in the body. It is only in a state of combination that the elements are of any service to us as food, and the combination must constitute an organic product.

Now these organic products may, by analysis, be resolved into a variety of definite compounds—the "alimentary principles," in short, so called in contradistinction to "alimentary substances," or articles of food as supplied by nature.

In a scientific consideration of food it is necessary to treat of the alimentary *principles*; only, indeed, by looking at it in this light can we discuss its physiological bearings. First, then, let attention be directed to the classification of these principles.

Classification of alimentary principles.—Popularly the ingesta are spoken of as consisting of food and drink: the one supplying us with solid, the other with liquid, matter. In a physiological point of view, however, this classification is worthless: milk, for example, being a liquid article, but rich in food or solid matter; while in almost all the food we consume there is a large proportion of water. The separation of the ingesta into "food" and "drink" has, therefore, no physiological significance.

The two material factors of life are food and air; and food may be considered as comprising all that assists in the growth and nutrition of the body, and, by oxidation, in force-production. Regarded in this comprehensive light, food embraces both liquid and solid matters; and the first natural division is into organic and mineral—that is, combinations of elements producible only through the agency of life, and chemical combinations drawn simply from the mineral kingdom and incorporated with the others.

The mineral portion of food consists of water and various saline principles. The organic may be subdivided into compounds of which nitrogen forms a constituent, and compounds from which it is absent; in other words, into nitrogenised and non-nitrogenised compounds. The non-nitrogenised alimentary principles are composed of the three elements, carbon, oxygen, and hydrogen, variously united together; whilst the nitrogenised likewise contain these three elements, but, in addition, nitrogen and usually sulphur, or sulphur and phosphorus, as well.

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* Lectures on the Conservation of Energy, Med. Times and Gaz., vol. i., 1864, p. 473.

Liebig, regarding the nitrogenised and non-nitrogenised principles as contributing to quite distinct ends in the animal economy, referred to them as forming the basis of a physiological classification. The former he looked upon as destined for appropriation towards the construction and maintenance of the body, and therefore he called them "plastic elements of nutrition." The latter he regarded as simply designed for undergoing oxidation, and, in this way, for serving as a source of heat. These he termed "elements of respiration"; but the expression is a bad one, as it does not really convey what is meant. Dr. R. D. Thompson suggested that the term calorifiant should be adopted instead. The more correct modification of this—"calorifacient"—has found its way into frequent use.

It stands to reason that for the growth and repair of the several textures of the body, as these have nitrogen forming an essential ingredient of their construction, nitrogenised compounds must be supplied; but from what is now known, it must also be said that these compounds are further susceptible of application to heat-production. They are indeed "histogenetic" or tissue-forming materials; but by the separation of urea (which is known to occur in their metamorphosis in the animal system) a hydro-carbonaceous compound is left which may be appropriated to heat-production. It may be regarded, in fact, as demonstrable that the nitrogenised principles subserve both purposes in the animal economy.

In fact, again, we have a non-nitrogenous principle, belonging to the calorifacient group; but fat nevertheless is essential to tissue development, and must accordingly take rank among the nutrient principles.

Hence Liebig's classification is not to be rigidly accepted. Although nitrogenised principles constitute true "elements of nutrition," yet it neither follows nor appears to be the case that they are limited to this purpose. Fats are undoubtedly calorifacients, and cannot, it is true, *per se* supply what is required for tissue development; they nevertheless, however, contribute to the process. According to present views, which will be discussed more fully further on, fats are also concerned in a manner not previously suspected, in muscular force-production. Taking all these considerations into account, Liebig's classification loses the scientific force it was originally supposed to possess. The subdivision of the organic portion of food, however, into nitrogenised and non-nitrogenised groups is still practically and physiologically convenient.

The classification which appears to me most convenient to adopt is one which involves no expression of physiological theory, but is based on the chemical nature of the principles.

It is first assumed that food falls naturally into

Organic, and
Inorganic

divisions. Next, that the organic is subdivisible into

Nitrogenous, and
Non-nitrogenous;

and, again, that the non-nitrogenous is naturally and conveniently re-subdivided into

Fats, and
Carbo-hydrates:

the former consisting of carbon and hydrogen in combination with only a small quantity of oxygen: the latter, of carbon with oxygen and hydrogen always in such relation to each other as to be in the exact proportion to form water. To this latter group belong starch, sugar, gum, &c.

Having said thus much on the classification of the alimentary principles, I shall next deal with them in relation to their respective physiological bearings, taking first

THE NITROGENOUS ALIMENTARY PRINCIPLES.

Nitrogen enters largely into the composition of the animal body, and must therefore be freely supplied from without. Although living in an atmosphere consisting of about four-fifths nitrogen, it is not from this source that our supply of nitrogen is derived. Nitrogen, to be available for us, must be supplied in a state of combination—in compounds which answer to the term "organic."

Among the nitrogenous matter found in different articles of food chemists recognise several well-defined compounds, some of which are characterised by yielding proteine when subjected to the action of an alkali and heat; while from others no proteine is similarly procurable. The former comprise the albuminous group, and are often referred to as the proteine compounds; the latter constitute the gelatinous principles.

Proteine, consisting of four elements—carbon, hydrogen, oxygen, and nitrogen,—constitutes a chemical and not a physiological principle, serving to link together certain important physiological compounds, such as albumen, fibrine, and casein, and certain other modifications of these.

Albumen may be regarded as the most important of these compounds, on account of the large extent to which it is encountered. It consists of the four elements, carbon, oxygen, hydrogen, and nitrogen, with the addition of some sulphur and phosphorus. It exists under two states—viz., soluble and coagulated albumen, and it can afford *per se* the nitrogenous matter required for nutrition.

Fibrine has the distinctive property of undergoing spontaneous coagulation; while casein differs from albumen not only by being non-coagulable by heat, but by being thrown down by organic acids.

The modifications of these proteine compounds are such as vitelline, existing in the yelk of egg; globuline, a constituent of the blood-corpuscle, and also elsewhere existing; and syntonine, or muscle-fibrine, first recognised by Liebig as distinct from blood-fibrine.

Besides the proteine compounds in animal productions, there are also in vegetable productions certain compounds which, in the language of Liebig, are identical with the albumen, fibrine, and caseine of the animal kingdom. Vegetable albumen, for instance, is met with in wheat and the other cereals, and likewise in most vegetable juices; vegetable fibrine in the gluten which remains behind when flour is washed with a stream of water; and vegetable caseine in peas, beans, and other leguminous seeds.

The gelatinous compounds, though nitrogenous, do not yield proteine. They comprise gelatine and chondrine, and are to be obtained only from animal products.

Digestion of nitrogenous alimentary principles.—All the nitrogenous compounds must undergo digestion before they can enter the system; in other words, they must be liquefied and transformed in order to be fitted for absorption.

Beyond being reduced to a more or less finely divided state in the mouth, our nitrogenous food undergoes no change till it reaches the stomach. There it comes in contact with the gastric juice, which dissolves and transforms it into a principle highly diffusible, and thereby readily transmissible from the alimentary canal into the blood-vessels. Under the influence of this gastric secretion all the nitrogenous alimentary principles lose their characteristic properties on becoming converted into the soluble and diffusible product referred to—a product which Mialhe was the first to recognise and term albuminose; since called peptone by Lehmann. Mialhe held that the product obtained from the digestion of the proteine principles was identical with that obtained from the digestion of the gelatinous; but this is probably incorrect, as it would bring the gelatinous principles into the same position as the albuminous as regards alimentary value, which there is reason to believe they do not in reality hold.

No matter from what principle—albumen, fibrine, caseine, or gelatine—a digested product or peptone has been obtained, the following characters are found to belong to it. It is eminently soluble in water, in the acid, neutral, or alkaline state. It is not precipitable from its aqueous solution by heat. It is soluble in dilute alcohol, while absolute alcohol precipitates it. It is uncrystallisable, devoid of odor, and almost of taste. Its physiological significance is its high diffusibility, and hence capacity for absorption.

The *modus operandi* of the change wrought by the stomach cannot be precisely explained. There are two indispensable ingredients in the gastric juice—namely, pepsine (a neutral nitrogenised principle) and an acid. Pepsine is a secreted product peculiar to, and obtainable only from, the stomach. About the acid there is nothing peculiar, and different views have been held regarding its nature. Combined with it, pepsine forms a liquid which dissolves nitrogenous matter equally out of the stomach as within it. Lehmann holds it to be only hydrochloric and lactic acids, which, with pepsine, yield energetic digestive fluids; but, according to my own experiments on artificial digestion, other acids, phosphoric, sulphuric, citric, and so on, will produce equally striking results.

The solution of nitrogenous food in the stomach, then, is effected by a liquid which owes its virtue to two principles, pepsine and an acid. Its action is favored by the high temperature inherent in the body, and by the movement the stomach performs through the muscular fibres of its walls. Reduced to a fluid state, it constitutes chyme, and is forced on into the upper bowel. Besides nitrogenous matter in a dissolved state, chyme contains still a portion suspended in a finely-divided form which has not yet undergone solution, and, also in the same state, those constituents of the food which resist the solvent action of the stomach. I will next speak of the further action that occurs when chyme reaches the intestine.

THE CAUSATION OF CHOLERA.

By Alfred Carpenter, M.D.

In The Lancet for May, 1871, I endeavored to prove that the factor in the production of scarlatina is a compound made up of several parts; that in the formula x, y, $z =$ epidemic disease, x and y are constant members, having a corresponding signification in every epidemic; but that z varies according to the external circumstances in which it may be placed; that its variation will change the character of a disease, so that scarlatina, typhoid, cholera, or any other member of the series, may be developed according to the value of z. In The Lancet for Dec. 1871 I published a case which presented some features allied to cholera, which will assist in bearing out one part of the theory I have propounded, and which is now endeavored to be proved as to the value of z in the equation x, y, $z =$ epidemic cholera. It is our present purpose to find out the value of each member in the proposition. This formula differs somewhat from that put forward by Pettenkofer, inasmuch as that *savan* makes x equal to the cholera germ; y equal to the local and seasonal substratum; whilst the product generated by z and y—viz., the cholera poison itself—is made to equal z. The equation stated above makes x, y, $z = z$ in Pettenkofer's formula; or, in more definite language, it is intended to show that x, y, $z =$ the disease produced by Pettenkofer's z—viz., cholera as a whole. It is now proposed to analyse x, y, z—to assess each member of the equation at its supposed value, and to show that whenever x, y, z come together cholera must arise.

If z is imported, and x y are present, the epidemic is produced. There is nothing more curious in this production than may be often observed in agricultural districts. If a fire is allowed to burn out on certain soils, and the surface left uncovered except with the ashes the result of combustion, a particularly fine-leaved clover springs up. The germs which produce this clover must be wafted by the air, and whenever they find a soil suited to their growth they take root and flourish. So it appears to be with the myriads of molecules always floating in the air. Some of them become acted upon by imponderable agents, such as heat and terrestrial magnetism, in unknown ways; and if they meet with a suitable habitat they may develop into z. This z is assumed to be molecular, and the molecules which assist in producing cholera are called by Paccini "choleragenic molecules." Dr. Farr styles them a conquering host of innumerable particles of living organic matter, in molecular form. Mr. Simon has described them "as characteristic molecular living things"; and they probably correspond with one form of microzyme described by Dr. Sanderson "as germs in which a specific form is wrapped up, capable of developing into the higher organisms from which they spring."

It is evident that Pettenkofer's matter z is this matter, which he regards as produced by x and y rather than independent of those elements, which is the course assumed to be the case in this paper.

This molecular something, which is always present in an epidemic of cholera, either as cause or effect, does not appear to be destroyed by drying; it may even be preserved by the exclusion of air; but it is fertile only in moisture. Experiment has shown that it is not easily removed from water by filtration; and it seems to act upon persons susceptible to its influence in a manner which some observers consider to be analogous to fermentation, though this view has not been proved. It is, or seems to be, a zymotic agent, which requires for its increase and multiplication a proper nidus, and a favorable state of body and of climate. Dr. Farr styles this molecule a "cholerad"—a more comprehensive term is "cacozyme"; for it is contended that this molecular matter may give rise to other diseased states besides cholera, according to the treatment it undergoes and the character of the matter with which it comes into contact; that typhoid, plague, influenza, &c., may arise from the influence of the same molecule at other times, or in other situations at the same time.

It simply indicates a debased form of molecular matter, capable of becoming a cholera or a typhoid cacozyme, according to the effect produced upon it by certain imponderable and all but unknown agents. That it tends to produce other diseased states in the years that witness to the absence of cholera, according to the treatment it has received from such terrestrial or universal influences as heat, moisture, magnetism, &c. This cacozyme equals z in the equation.

It has been lately shown that certain geological conditions tend much towards the rapid development of z; but in every such case it is shown that water is the vehicle of distribution, so far as cholera is concerned. It has also been shown that mucous membrane is the door of entrance into the human economy; and it may be taken as proved that there is a characteristic living thing, which is the cacozyme, upon which the character of the epidemic depends.

Let us now assess the values of the other members in the equation x, y, z = cholera.

It is assumed that x = the waste of tissue—the used-up matter which results from the very act of living; ever present in the blood, yet always in process of removal by the various excretory organs of the body. In a proper state of health it never exceeds a certain but unknown quantity. It is made up of the matters which ultimately form urea, bile products, mucus, cholesterine, &c., such matters having been used in the manufactory of vital actions and other results of the act of living; they are not capable of being used again for the same purpose, and, if retained in the blood, would themselves act as poisons to the system. In the equation, x represents this waste of tissue, and is a result of oxidation—a means by which free oxygen is removed or fixed.

y, the middle member of the series, arises when x is not removed from the system in the ordinary manner. The waste of tissue represented by x is left in the blood: a matter remains rich in nitrogen, and renders the system prone to take on unhealthy actions. It enables other matters to find their way into the blood by altering the principle which allows of exosmose, and which alteration a proper cleansing would not permit.

It is certain that a man is at one time easily able to resist influences which at another time overpower him as easily. It is contended that this opposite state is caused by the retention of some matters which ought to have been excreted, but were not, and which, when circulating with the blood, make a man fitted to become the recipient of z, if the latter comes in his way. For this matter without free oxygen yet rich in nitrogen, is the very matter which is the source of activity to z whenever z finds entrance into the human economy.

We have now the values which attach to each member in the proposition x, y, z = cholera.

z being the cacozyme, it has to be brought into contact with x and y before cholera can arise. It is not an entity in itself, so that it can produce the disease; that is, it may be brought into contact with various individuals, but if y is absent cholera does not arise. It must find a proper soil, or it cannot grow into cholera, though it may attempt it; but it will be dwarfed, and not become a true case of the disease. Like diastase, the germ z must meet with organic matter prone to take on certain changes, or it cannot produce the result we are considering. Even if it is looked upon as the result of a simple chemical action, it cannot produce the effect if one of the bodies requisite for the production of the compound is absent.

It has been conclusively shown that defective drainage, and water-supply contaminated with excretions, promote most actively the spread of the disease; and it is contended that z has its origin in the *débris* of carnivorous animals, or in the destruction of the materials which make up the sum of animal life when those materials are exposed to certain unknown influences. The theory therefore is, that if animal *débris*, or the parts of animal tissues, be taken care of, so that they shall not come into contact with human living mucous membrane, cholera cannot arise: a proposition which experience has proved to be true, and upon which true sanitary science appears to rest.

z may find entrance into the body in various ways. It is considered by most sanitary authorities that it is through the alimentary mucous membrane that the factor of cholera finds its way; and that whilst the cacozyme which causes typhoid or scarlatina may enter by the pulmonary tract, it is through the gastric organs that the cholera cacozyme gains admission. z is probably heavier in the one case than in the other—more debased, more fungoid, and most likely to be first discovered. It may occupy a state corresponding to the cancer cell as compared with the granular matter of tubercle. Let z be carried by liquid into contact with moist mucous membrane; let it find the fluids of the body charged with more or less of the materials represented by y, which are known to be rich in nitrogen—z increases and multiplies at the expense of x and y, until the whole of those available matters have been changed into x, y, z, and a condition results which is incompatible with health. The result of this change, if intense and rapid, is a condition of blood incompatible with life, and death follows. If this change is not so intense, the sum total of x, y, z becomes evacuated, and sets up symptoms which are the characteristics of the class of diseases of which cholera is one.

The first idea which arises in considering a theory is the use of it. It is contended that this view of the causation of the disease is borne out by the results of empirical treatment; it assists in supporting certain methods, and directs a rational explanation of the good results which sometimes follow, as well as explains the cause of failure in others. It is contended that no person will become the subject of cholera unless more or less of the material represented by y exists in the fluids of the body. x must necessarily always be present. z may find its way by accident or neglect, but it cannot flourish if y is absent; and y will not be there if every function of the body is moderately well performed—if the *débris* of the body is properly excreted, and if the introduction of debased matter (as z) is prevented. When symptoms of cholera arise, an inquiry should be at once directed to the excretory organ which is at fault. Should this be the alimentary canal, the castor oil treatment so strongly insisted on by Dr. George Johnson will be found sufficient if the organisms have not been long absorbed; but if this is not so, and there is some other defective organ, as kidney or skin, attention should not be limited to the alimentary canal, and castor oil will not do.

Thus, to arrive at a rational course of treatment, we must determine the causation of the disease itself. It is contended that this arises from the entrance of the cacozyme z, from its finding a suitable pabulum for growth provided by defective secretory organs; and the remedies must be such as shall give relief to the defective secretory organ, and at the same time be destructive to the life of the cacozyme; otherwise treatment will be of no avail, and life or death will depend upon the quantity of pabulum stored up in the body of the patient.

We may now consider the conditions under which that life is carried on.

Pasteur has shown that it flourishes most when all free oxygen, or oxygen easily separated from its combinations, has been removed from a given material. It seems also as if the putrefactive changes which arise in animal remains or carnivorous exuviæ get rid of all free oxygen, and a material is left from which free oxygen cannot be liberated, but which can develop the cacozyme upon which cholera depends. It would be dwarfed and destroyed by oxygen, more particularly by ozone. It has also been shown that free oxygen does not exist in the discharges of choleraic patients. Free oxygen ought to be the remedy against the establishment of the disease, and for the rapid destruction of the cacozyme.

It may at once be replied that, if this theory be true, no person could suffer from cholera, because oxogen is freely presented to the factor in the circulation itself. This, however, is not the case long enough to effect the required change, for the great highway by which oxygen finds its way into the blood, and through that into the nerve-centres, is by the capillaries of the lungs; but if the cacozyme has found admission, and has found ready to hand a sufficient quantity of pabulum in which to develop, and has thus extended its influence to all parts of the system, a stage of collapse may be reached in a very rapid manner, and the requisite nerve-force for continuing the nutritive changes in the blood cannot be produced; whilst the presence of the cacozoon, or its resulting x, y, z, puts a stop to the action of exosmose and endosmose by which the blood is relieved of its superfluous carbonic acid. The whole of the changeable material becomes affected, the cacozoon runs riot in the system, it becomes x, y, z, and in this changed state has to be evacuated as an altered "waste of tissue." If the patient is to recover—that is, if there is not pabulum enough to destroy life by the intensity of the change,—the cacozyme fulfils its mission, and, like other used-up granular matter, is destroyed by the oxygen which gets into the blood, and then is ejected. But recovery will not take place if the quantity is sufficient to stop the action of endosmose and exosmose long enough and completely enough. The main entrance for oxygen is comparatively closed as soon as the nerve-centres of nutrition feel the effect of the cacozyme. The oxygenation of the blood cannot be sufficiently effected, and it circulates again without being deprived of its used-up matters. The cacozyme has thus time to come to maturity. If there is not sufficient y to produce an amount of x, y, z equal to destroy life, the oppressed nerve-centres will after a time recover from the collapse, and resume healthy action. The movement in the pulmonary capillaries is gradually resumed, the exhalation of carbonic acid returns, the endosmose suspended in the lungs, but as exosmose transferred to the mucous membrane of the bowels, begins again, and in a short time the patient recovers. He has now another risk to run in passing through the secondary fever which supervenes, and which arises from the presence of effete cacozootic life, which dead matter or formed material has to be evacuated through channels not used to such expulsion.

This theory of impeded circulation through lung-tissue corresponds with that propounded by Dr. George Johnson. He has shown that there is dark lung-tissue, whilst the pulmonary capillaries are comparatively empty; that the lungs themselves are greatly reduced in weight; that the circulation is carried on through bronchial vessels; and that there is congestion of bronchial membrane. A current of blood does pass through the pulmonary artery, but it has only the power of the right ventricle to assist it; and that power is not much when it is unassisted by the *vis a fronte* which arises when the carbonic acid is given out and oxygen taken in. This action must materially assist in promoting the pulmonary circulation, and its absence must retard it, whilst the demand for blood in the left heart keeps the pulmonary veins empty.

It has been proved by Rosignol and other anatomists that there is a communication between the bronchial arteries and the pulmonary veins; hence it follows that those arteries are able to send a supply of blood into the left heart, and for the time being the patient in part becomes a cold-blooded animal, and unaërated blood circulates through the system; but the quantity reaching the left heart is small compared with the required amount; hence the small, feeble pulse, the sunken voice, and the full systematic veins.

It may be suggested that there is no real reason why the blood of a cholera patient should not be oxygenated if respiration goes on; and it is proved that the blood drawn from the body during collapse does absorb oxygen. A sufficient answer is given to this query in the statement of the fact itself: the action does not take place, oxygen is not absorbed in the usual ratio, and carbonic acid is retained, showing that the obstruction to the action exists in the patient's own body. And it is evident that this obstruction is due to defective vital or physical agency: if vital, it must be by the effect of the cacozyme upon the nerve-centres; if physical, by the same matter altering the conditions under which exosmose takes place. The heat which would be produced by the change of carbon into carbonic acid and the exchange of carbonic acid for oxygen does not arise, and the circulation through the lungs is that produced by the heart's power only. The patient has the appearance of a cerulean, but there is no manifest dyspnœa whilst the patient is at rest.

It will be observed that I have adopted the germ theory of disease, though Dr. Bastian supposes that a last blow has been given to that theory by certain reasons advanced by himself and others, the main points of objection to the theory being based upon the statement that malignant parasitic diseases are always fatal (witness "malignant pustule" and the parasitic disease in silkworms called "pebrine"), whilst, on the other hand, the general tendency of contagious disease is towards a restoration to health. But it will be at once observed, that if cholera, typhoid fever, or scarlatina depend for their development and growth upon something not properly belonging to the blood, whilst the parasitic diseases mentioned live upon and alter the character of healthy blood, the objection falls to the ground and becomes untenable. Another objection very strongly urged by Dr. Bastian and Dr. Richardson is, that there must be about twenty different kinds of germs or organisms never known in their matured condition; and, because they are not known, we are called upon to disbelieve in their existence. The astronomical discoverers of former days would have had stern opponents in these physiologists. Would they forbid their pupils to disbelieve in the power of certain granular matter in the ovum to generate different kinds of nerve-force, though they are unable to distinguish any difference in the cells after they have reached a matured condition? Is it possible to distinguish the granular matter of cow-pox from that of small-pox or chicken-pox, although it has reached a matured condition? It is known that certain fluids in various contagious diseases are precisely alike to our finite vision, yet they can mature entirely different complaints. To be consistent, this possibility should be denied. It is certain that analytical perfection has not yet been reached, and that there are discoveries with the microscope and test-tube yet to be made. Dr. Sanderson has already proved that liquids which do not contain any particles distinguishable under the highest known powers of the microscope, yet possess the property of evolving microzymes without contact with external media. Then, again, it is urged against the germ theory that, because certain Kalmuck Tartars are said to eat putrid flesh, therefore the supposed organisms cannot exist. The same authors thus show that, whilst they believe that the furthest boundary of analysis has been reached, they also limit the power of the human stomach in a state of nature to deal with such organisms, though we see that power daily exercised at home. We might just as well argue that, because an immense number of persons have been recently vaccinated a second time without result, therefore vaccine virus is not infectious. But the last and most deadly blow is supposed to be given to the germ theory because it is shown that the blood and natural secretions of animals suffering from sheep-pox are not infectious. This argument, however, will support the theory I have adduced—viz., that it is not the blood which is the infectious part, but that infection exists in something which is not proper and necessary to the perfection of the blood—a material which ought not to be there, and which the disease tends to evacuate from the system, either by discharges, as in cholera, or by specific eruption, as in scarlatina, and which varies in quantity every day.

If the virus peculiar to this series of disease was formed from element-parts of the blood, every person would become a victim according to the quantity of blood in the body—a result of disease which is well known not to be the case.

There are organic poisons which do produce spreading changes in the blood, but such poisons have no tendency to reproduce themselves. Their effects will be regulated by the dose administered; whilst in the case of those germs which it is contended are the source of epidemic disease, the quantity introduced is of little moment if there is time for reproduction before they are evacuated; that is, if a state of incubation can exist, a single germ finding a proper habitat, and time being given for its development before it is evacuated, will ultimately produce the evidence of its presence in the onset of symptoms which are typical of the form of disease which that germ is capable of originating.

Croydon, October, 1871.

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ON THE TREATMENT OF HYPERPYREXIA BY WITHDRAWAL OF HEAT.

By T. Clifford Allbutt, A.M., M.D., &c.,

Physician to the Leeds Infirmary, and Lecturer on the Practice of Medicine, etc.

The report of my introductory address at Leeds has brought upon me a quantity of correspondence, directed especially to two points: first as to the truth of the elimination or "effort of nature" theory; secondly, as to the treatment of hyperpyrexia by withdrawal of heat, and to this I propose, in reply to my correspondents, to add some further evidence. This evidence I shall put as shortly, I hope, as the claims of a monthly journal demand; but the subject is of the highest importance, both in itself as a direct means of saving life by art, and also as forming a great part of the constructive side of that dialectic which, on the other hand, aims at the destruction of the "effort of nature" hypothesis.

Many of my readers seem to have been of opinion that my own experience and that of Dr. Wilson Fox, which I quoted in terms of warm congratulation, stand alone, and I am assured by them that further observation will greatly modify our views. The very striking cases lately reported by Dr. Fox in The Lancet, and now republished by him in fuller detail, cannot be got over; and no future experience can deny that there must have been some successful art in the management of cases which under all other methods have ended in death. *Un sait ne se nie pas.* Dr. Fox can defend himself far better than I can defend him, but I would only urge those who very properly withhold their faith to remember that these cases of his do not stand alone. In England, the land of its birth, the water-treatment of fever is as yet little known; but those who are familar with German literature are aware that an immense body of experience has been gathered together on this subject by such men as Brand, Jürgensen, Liebermeister, Immermann, Ziemssen, Senator, and many others—men of undoubted skill and good faith, and having large opportunities for observation and practice. Now among all these men there is much difference of opinion, and a difference expressed in no measured terms; but it is a significant fact that there is no difference of opinion as to results. Senator and Liebermeister dispute vigorously the precise way in which the results are obtained; but it enters the brains of none to question the effects of cool bathing in fever, or to doubt its power of saving life with almost miraculous success. It is not a fair way of dealing with such work as theirs to bring forward isolated examples of failure—examples, too, not infrequently founded upon imperfect observations, or even upon mere hearsay. Among other correspondents, Mr. Churton, of Erith, wrote me a kind letter mentioning two cases in which bad results had followed the cold-water treatment.* These were both cases of enteria, but were not either of them under the care of Mr. Churton himself, nor have I any careful account before me of their course before and after the treatment. No doubt, however, the profession will pause, and very properly, before applying what seems to be an heroic treatment; and will claim, in the interests of the public, to know very clearly what they are about before they undertake it. And this the more as I think we must freely admit that, like a sharp sword, which must be in the hands of a finished swordsman, so the "cold-water treatment" is capable of doing much mischief in the hands of the rash or unlearned. For this reason I venture further to occupy the columns of The Lancet with these and the following remarks.

For example: I cannot conceive anything more desperately risky than the casual plunging of a fevered patient into a tub on the mere conclusion of a single visit, or even of one careful consultation. I feel satisfied, both from analogy and from observation, that those physicians are in great measure right who contend that such rapid withdrawal of heat is followed by a corresponding or a very considerable increase of heat-production. It is not yet familiarly known that the standard of animal temperature does not by any means represent a uniform production of heat, but represents the difference between the amount set free and the amount dissipated. Heat-production probably varies enormously from hour to hour, but heat-dissipation varies exactly as production varies so long as the balance of the organism is steady; in like manner, if we increase the dissipation, we by the same movement increase the production, in virtue of the action and reaction between surface and interior. This, which is true to demonstration in health, holds probably in great measure in disease also; so that when we abstract heat largely, even in disease, we probably increase production in something like a corresponding ratio, if not actually to the same degree. In plunging a patient with a temperature of 106°, say, into water at 60°, we set up an enormous outflow, the body being quickly reduced, say, to 100°; now, the beautiful adjustment of the body being lost, we certainly do not see a continual adequation of heat-production, but instead of numerous and infinitely small compensating oscillations, we seem to get fewer and larger oscillations, so that the organism, when removed from the bath, tends to "react"—that is, to swing back to, and even beyond, its former place. Ziemssen seems to have seen this difficulty more clearly than any of the writers on the subject, and to have drawn from it the practical conclusion that if we are to modify reaction we must modify our withdrawal of heat, and rather coax than compel the organism to enter into another equilibrium. Hence he lays down a rule with which I cordially agree—namely, that the withdrawal of heat must be the slowest possible for the due attainment of our end. We know that a heat of 104° is slowly killing the intimate tissues of the body and arresting molecular movement, and that a heat of 106° is doing this rapidly. It is clear, then, that we must bring the body below this point; but this may be gained by using baths of a temperature far higher than 60°. Theoretically, it would seem sufficient to use a bath of 100°, and to cool it artificially during use to about 96°; practically, however, the temperature falls so slowly as to be scarcely perceptible when the patient is placed in a bath of 100°. I still think, however, that a bath of this temperature, which when fully prepared will stand but little above the normal blood-heat, is the best for a commencement, and that its temperature should then be kept down, and, indeed, slowly lowered during the immersion of the patient. This lowering must depend upon the state of the patient; but a long immersion and a slow lowering is in all respects, I am sure, more desirable than a short immersion and a rapid fall. If the patient can bear an immersion of an hour, or even of two hours, the lowering may be made very gradually, and need not be carried below 80°. On removal from such a bath, the further diminution of temperature which generally succeeds the immersion will not be followed by a violent reaction—that is, its effects, more slowly induced will be more permanent. In addition to this treatment directed to increase the dissipation of heat, I always endeavor to prevent its rapid reproduction by administering quinine in repeated doses of from five to twenty grains, according to circumstances, as advocated by Professor Binz.

The next point I would urge is the continued presence of a medical man and the incessant use of the thermometer. No case can really be counted as evidence either for or against the bath treatment in which the thermometer was not watched as incessantly and as anxiously, during the whole treatment, as the steam-gauge of a rotten boiler. To speak nearer to the point; the doctor cannot take an observation, order a bath, and wait to take another until his next visit. The whole treatment must be managed with the utmost precision, and all tendencies to shiver or syncope should be watched by a skilled observer, or irreparable harm may be done in five minutes. No farther reasons need be urged on this important point; but I am satisfied, if more reasons are not wanted, that practical warnings, on the other hand, are much wanted, and will be needed more and more if the bath treatment becomes popular. If modern medicine is to be precise, and if its criticism is to be equipped with keener weapons, the awkward truth will begin to stare us in the face that much more will be required of us than the casual routine of the past.

My own experience of cerebral rheumatism—that is, of rheumatism marked by temperature so high as directly to disorder and oppress the encephalon—is somewhat large, considering the rarity of the disease. Out of about twelve cases under my care, two only have recovered. The first case of recovery occurred about six years ago, in the practice of my friend Mr. Scattergood of Leeds, with whom I saw the patient. The patient was a strong, temperate young man of about thirty years of age. His rheumatism at first was mild, and a consultation was called only for the purpose of watching the heart. My few visits soon ceased, but Mr. Scattergood then spoke to me anxiously about obstinate wakefulness which had set in. Upon this followed delirium and spasmodic motor disorders of great violence, but fortunately not associated with a tendency to loss of consciousness or to coma. The temperature, fortunately, never exceeded 104° during a very anxious period of some days' duration. Morphia controlled the delirium and the muscular violence in a remarkable degree, and we believed that it was only by the incessant use of this drug that life was saved. My second case occurred in September last, and recovery was in this instance due, I doubt not, to the withdrawal of heat—a plan which I adopted in accordance with my previous use of this means in other fevers of high temperature, and with the results obtained by our German brethren. I was called to the patient after the publication of Dr. Wilson Fox's cases, but I had not seen his papers as my call occurred almost immediately upon my return from a holiday in Switzerland, during which time no medical journals had fallen into my hands.

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* To show the uselessness of such fragments of evidence, I may say that on the same day a colleague told me of three cases in which he had obtained the best results, and this too in pneumonia.

I had also made use of the same treatment in a case of "cerebral rheumatism" under the care of Mr. Carter, of Chapeltown, near Leeds, but without success. I was called to the case late in the evening (Mr. Carter was, unluckily, out of the way; the patient's temperature was then 111°, and his state comatose. The cold bathing which I ordered and did my best to secure was most probably imperfectly carried out; at any rate, the patient died eight hours afterwards, without being seen again by either of us.* My case of last September was more fortunate in its results.

Dr. ——, a middle-aged man in large practice beyond Keighley, was under the care of Dr. Cockcroft, of Keighley, when I was called to him on account of the symptoms of "cerebral rheumatism," which had supervened upon an ordinary attack of rheumatic fever without heart complication. From the 11th to the 14th of September his state had gradually become very serious; sleeplessness and alarming head symptoms had set in. On the 13th there had been incoherence and delirium; no true sleep for some days; and there was an increasing tendency to coma. Under these circumstances, on the evening of the 13th Dr. Cockcroft had shaved the head and applied large quantities of ice very freely to the head, forehead, and nape. This application had been followed by a slight but decided improvement. Hitherto no temperatures had been taken. I saw Dr. —— on the afternoon of the 14th in consultation with Dr. Cockcroft, Mr. F. Greenwood, and Dr. ——'s assistant. The patient was then unconscious and delirious in a low and muttering way, his countenance turgid and oppressed, his pulse 130, and his temperature 105·8°. There was no inflammation of the joints or heart. This condition was reported to me as one of slight improvement, and I had little doubt that the temperature had been lowered by the free use of the ice. While obliged to give the gravest prognosis, I gave my full adhesion to the plan commenced already, and urged, on the grounds before explained, that the withdrawal of heat should be systematically carried out under the guidance of the thermometer. As plenty of ice was at hand, and the use of a bath not so convenient, it was determined to fill wide-mouthed bottles with ice and lay them along with the patient in bed. I think six bottles were so applied at first, and their number modified subsequently according to the temperature. The ice was also continued in a large bladder covering the head and neck, and dripping a good deal about the pillow. After settling all these details and others of less immediate interest, I took my leave. Mr. Greenwood and the patient's assistant remained in the house and superintended all the proceedings, skilled supervision being absolutely necessary in this case. Mr. Greenwood kept and forwarded me the report of its progress, and I here set down extracts from his diary. At 6 P.M. (about three hours after the commencement of the full ice treatment) the temperature was reduced to 102°; at 7 it was 101·4°; at 11 it was 100·2°, and at this time the patient fell into a quiet sleep of four hours' duration, only broken by the occasional administration of food. The bottles were continued more or less according to the fluctuations of the thermometer during the day and night; and I may pass on to Sept. 15th, 6 A.M., when the temperature was 99·4°. At this time the patient was intelligent enough to complain of rheumatic pains. During the remainder of the 15th the temperature fluctuated between 100·2° and 101·2°, the ice being regulated accordingly. He had "a very fair sort of night," and I may sum up the observations of the next five days by saying that the temperature was kept down to points between 100·2° and 100·8°. Sleep was not repeated in the same satisfactory way; there was much exhaustion and therewith restlessness. In addition to the usual stimulants, morphia draughts and chloral were continuously tried to relieve this restlessness, but produced only short disturbed dozes, with starting, moaning and muttering, and stertorous breathing. It was not thought desirable therefore to push the draughts. I should have added that during all this time, in addition to the ice bottles, the patient was regularly shifted from one bed to another, and sponged from time to time with cool water. No medicine was taken. On the 19th these symptoms of exhaustion began to pass off, and the hard fight was continued with renewed hope. On the 21st the temperature was easily kept to 99°—100°, and farther improvement was visible. During the next few days less and less ice was used until the 25th, when a steady temperature of 98°—99° was easily maintained without ice, and no more regular observations were made. From the 21st, indeed, the improvement was steady and satisfactory, and the patient entered on the 25th into a convalescence which has since progressed favorably.

In this case, although the applications were very cold, on the other hand their application was partial; nevertheless, I am still disposed to hold that a complete immersion in warm water is better than a partial application of very cold water or ice, except in the coma of hot scarlatina, when douches of very cold water to the head and neck are of great service.

Leeds, Dec. 1871.

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* My own criticisms apply in condemnation of my own practice on this and on more than one other occasion when my experience was less.

THE THEORY AND TREATMENT OF CHOLERA.

By George Johnson, M.D., F.R.C.P.,

Physician to King's College Hospital.

A true theory of cholera must be consistent with the undoubted facts of the disease, and with the established principles of physiology. In a work on Epidemic Diarrhœa and Cholera published in 1855, I put forth a theory, of which I venture to say, after an interval of more than sixteen years, that not one of its numerous assailants has succeeded in showing that it is inconsistent either with facts or with physiological principles. That a scientific theory of cholera must meet with much opposition is inevitable. Such a theory must clash with the convictions and prejudices of men who have framed or accepted incomplete and erroneous theories, and so it will excite their hostility. At no time have my doctrines wanted vigorous opponents, but recently the opposition has apparently been stimulated into renewed activity by the unwelcome fact that Sir Thomas Watson "has unreservedly adopted" my views as to the nature and treatment of cholera. That an authority so eminent, one, too, so cautious and conscientious in forming a judgment, should have pronounced in favor of my doctrines, and defended them with characteristic eloquence and logical acumen, has evidently been felt by my opponents as a heavy blow and great discouragement. Amongst these opponent Mr. William Sedgwick has lately taken a prominent position, in consequence mainly of the large space which his writings have occupied in the crowded columns of The Lancet. I am indebted to that gentleman for having done so much to obtain publicity for my doctines, and I should have taken no notice of his harmless criticisms if I had not believed it to be a public duty to direct attention to the unfairness of his mode of attack, and the serious and culpable inaccuracy of some of his statements. I therefore propose to illustrate by two or three examples the manner in which he attempts to refute an opponent's doctrines.

In a paper published last year (*Brit. and For. Med.-Chir. Rev.*, April, 1870, p. 494), referring to a case of diarrhœa in which the urine had been albuminous, I remark that "the temporary albuminuria is to my mind conclusive evidence of blood-poisoning." By this I meant, as is obvious from the context, that temporary albuminuria in connexion with choleraic diarrhœa affords the same kind of evidence of blood-poisoning as is afforded, for instance, by that symptom in cases of diphtheria &c. My opponent, in quoting the above passage, omits the first word, and thus misrepresents me as saying that "temporary albuminuria is to my mind conclusive evidence of blood-poisoning." As if I had stated that temporary albuminuria *always* results from blood-poisoning, and *never* from any other cause. He then wastes nearly a column of The Lancet to prove that albuminuria may result from a mechanical impediment to the return of venous blood &c., and therefore from other causes than blood-poisoning; as if I needed to be taught this elementary fact in pathology!

Again, my critic declares that I assert the absolute inactivity of the liver and kidneys as secretory organs during intra-uterine life. Abernethy, he says, believed that the kidneys did not secrete until after birth; but upon this he sarcastically remarks—"Since Abernethy's time some progress has been made in the study of physiology." He then proceeds at great length to expose my ignorance and to correct my error. Now in one of the papers referred to by Mr. Sedgwick (*Brit. Med. Jour.*, Feb. 17th, 1866) I express myself upon this subject in the following terms. I have placed in italics the expressions which my critic has evidently overlooked:—

"A very interesting and instructive illustration of the physiological correlation between the lungs, the liver, and the kidneys, is afforded by a comparison of the *almost complete inactivity* of these great excretory organs in the fœtus with the energetic discharge of their functions which commences simultaneously in all of them the moment after birth. During intra-uterine life *the lungs are entirely inactive;* no air is admitted into their cells, and the greater part of the blood from the pulmonary artery passes directly through the *ductus arteriosus* into the aorta. *The kidneys and the liver are nearly as inactive as the lungs. The bladder, it is true, contains some urine;* and the *meconium,* which is contained in the intestines is chiefly composed of bile. *A certain small amount of these excreta is formed during fœtal life;* but the quantity excreted is infinitely small in comparison with the rapid excretion which begins immediately after birth, when, with the establishment of the function of respiration, there is an evolution of carbonic acid, and a continuous formation of the two correlative secretions—the two joint products of oxygenation—bile and urine."

If I had foreseen that my statements would be subjected to the merciless criticism of Mr. Sedgwick I could scarcely have expressed myself more cautiously, or, as I think, more correctly, upon this interesting physiological question. There are oxidised products in the fœtal circulation which afford the materials for a scanty secretion of bile and urine. These products may be a result of what is sometimes called "placental respiration," or they may be derived from the maternal blood, from which the materials for the growth of the fœtus must obviously be derived.

Mr. Sedgwick affirms that I explain the comparative inactivity of the liver and kidneys of the fœtus by saying that it is "a necessary result of arrested pulmonary circulation and consequent apnœa." This is given as a quotation from one of my papers; but the sentence occurs there as a brief explanation of the suppression of bile and urine *during the collapse stage of cholera.* To say that the apnœa of the fœtus is a result of arrested pulmonary circulation would be preposterous and ludicrous; yet this is the statement which, by a misquotation, my critic attributes to me! Again, my censor erroneously charges me with what he calls "the fallacy of assuming that the cyanosis in cholera is due to defective oxygenation of the blood." On the contrary, I have always maintained that the lividity is mainly due to distension of the veins in consequence of the obstruction within the chest; and the fact mentioned by him, if it be a fact, that the cyanosis disappears from a limb under the influence of galvanism, confirms my explanation. The galvanism, by exciting contraction of the veins and muscles, lessens the venous distension, and thus temporarily removes the cyanosis.

Mr. Sedgwick, at the conclusion of his paper, endeavors to excite prejudice against the eliminative treatment of cholera by referring to statistics showing that, under its employment by myself as well as by others, the mortality has sometimes been very high. This evidence would be of great weight if it were not perfectly clear and generally acknowledged that, in cases of extreme collapse, neither evacuants nor any other treatment, except, perhaps, the hot saline injections into the veins, can be of any real service. Mere statistics therefore, without a detailed report of cases, teach absolutely nothing as to the influence of remedies. The chief value of the paper by Drs. M'Cloy and Robertson (Med.-Chir. Trans., vol. 50) consists not merely in the great number of cases treated (a number far exceeding all the other cases referred to by Mr. Sedgwick), but in the detailed reports given of many of the cases. A large proportion of the few cases admitted under my care in 1866 were of an utterly hopeless and intractable character, as I have shown in a lecture published soon after the epidemic was over (*Brit. Med. Jour.*, Nov. 10th, 1866), while in nearly three times the number of cases treated *from the commencement to the termination of the epidemic* in 1854, the results were far more favorable, the particulars of every case being published in my work on "Epidemic Diarrhœa and Cholera," 1855. The great object of treatment obviously is to prevent the passage of the disease into the perilous stage of collapse. When once that stage is reached the danger is greatly increased. That the state of collapse, which results from impeded circulation and the consequent check to the natural process of cure by elimination, is a very fatal condition, is an argument against a repressive course, and in favor of an evacuant plan of treatment in the early stage of the disease; and a high mortality in cases of extreme collapse affords no more evidence against the utility of the eliminative principle of treatment in curable cases of cholera, than does the necessarily large mortality in cases of malignant small-pox against the cooling treatment in ordinary cases of that disease. I maintain that flushing the intestines of a cholera patient by copious draughts of water, and, *if necessary*, by gentle evacuants, is based upon the same common sense principle as that of free ventilation in the treatment of small-pox; and I have the satisfaction of knowing that this principle of treatment is accepted and acted upon by a large and increasing number of unprejudiced men both here and in India. If space permitted I could prove this by abundance of testimony.

Mr. Sedgwick's own theory of cholera, the defence of which has led to his attack upon me, requires no refutation. To state the theory is to refute it. The attempt to explain the symptoms of cholera by some mysterious influence exerted through the alimentary canal upon the sympathetic nerve, without the agency of a specific blood poison, is obviously a hopeless task. There is one part of his theory which involves so curious a physiological error that it merits especial notice. It is well known that when, after death during the stage of collapse, the chest is opened the anæmic lungs collapse and shrink into a very small space. So far as I can understand Mr. Sedgwick, he believes that the pulmonary collapse occurs during life; at any rate, he explains it thus:—"The pulmonary collapse in these cases is caused by the amount of carbonic acid to be eliminated being reduced, whilst the nervous power of the lungs continues the same, or nearly so." ("On the Nature of Cholera as a Guide to Treatment," p. 20.) And again he says, and gives the statement emphasis by italics: "*The arrested formation of carbonic acid in the system has the same relation to pulmonary collapse as the arrested formation of urea has to suppression of urine.*" He evidently means that, as the urine is suppressed because urea is not formed, so the lungs collapse because carbonic acid is not formed; but he does not distinctly state when he believes the pulmonary collapse to occur. I venture to suggest to my zealous opponent that the true interpretation of the facts is this: During the collapse stage of cholera the stream of blood through the pulmonary capillaries is greatly reduced, in consequence of resistance offered by the pulmonary arterioles; the blood is the channel by which oxygen is conveyed to the tissues, and the supply of oxygen is, therefore, reduced in proportion to the diminished circulation of blood. Again, carbonic acid, and the chief constituents of bile and urine, being products of oxidation, their formation is diminished in proportion to the scantiness of the oxygen-bearing blood-stream. Lastly, when the chest is opened after death, the lungs collapse and shrink to an unusual degree, consequent on the emptiness of their capillary vessels. I believe that no physiologist will accept Mr. Sedgwick's explanation of the facts, or refuse to accept mine.

I have reference to several cases of embolism of the pulmonary artery, in which the symptoms resulting from the impeded circulation through the lungs have borne a striking resemblance to those of the collapse stage of cholera. The most completely recorded case of this kind is that published by Dr. Alfred Carpenter. In that case, as Dr. Carpenter remarks, "the only symptoms wanting to make it apparently one of cholera was alvine discharges and cramps in the limbs."

Dr. Talfourd Jones, of Brecon, accepting my explanation of collapse, has ingeniously suggested, as the physiological antidote for the cholera poison, the inhalation of the vapor of nitrate of amyl. This substance has the effect of relaxing the muscular arterioles and so causing capillary congestion. If it should be found to have the same effect upon the pulmonary vessels as it has upon the systemic, it may prove to be a remedy of great value in the treatment of cholera; it should therefore be carefully tried in that terrible disease.

Savile-row, Dec., 1871.

ON THE

FUNCTIONS OF THE SYMPATHETIC SYSTEM OF NERVES, AS A PHYSIOLOGICAL BASIS FOR A RATIONAL SYSTEM OF THERAPEUTICS.

By Edward Meryon, M.D., F.R.C.P.,

Late Lecturer on Comparative Anatomy at St. Thomas's Hospital.

(Concluded from page 77.)

SUGGESTIONS IN SUPPORT OF A RATIONAL SYSTEM OF THERAPEUTICS.

A volume might be written on the correlation of nerve and medicinal action upon the minute vessels of the stomach, and on the function of that organ generally; but the special influence of each does not appear to me to admit of the proof which I conceive to be adducible in relation to more exclusively glandular structures, because the evidence upon which any satisfactory inferences can be founded is so conflicting that I will not attempt to force the sway of fancy into the throne of truth. When men like Bernard, Kölliker, and Heidenhain maintain one view respecting the influence of the pneumogastric and splanchnic nerves on the stomach, and Bidder, Vulpian, and Nasse entertain the very opposite, I may be excused from speculating on either side. My conviction, however, is that there is a reciprocity of action between the vaso-motor nerves of the stomach and medicinal agents calculated to act on them.

The influence possessed by the ganglionic nerve-centres on the chemical phenomena of life can be simulated, increased, or diminished by special medicines. The increased supply of starch in the cells of the liver, and the diastose acting on it to produce an increased secretion of grape sugar, as caused by section of the sympathetic nerve-fibres, or by irritating the nucleus of the par-vagum in the medulla oblongata, may also be induced by the action of mercury.

And here I would adduce the liver to prove that something more than mere vascular fulness is necessary for the functional action of a secreting tissue; for such dilatation of vessels may, and often does, occur without a corresponding secretion of bile. By the suspension of the reflex influence of the sensory nerve-fibres, active hyperæmia of the hepatic artery may lead to hypertrophy of the organ, or hyperæmia of the portal veins may result in structural degeneration, without an increased outpouring of bile at any time.

The very term *erythema mercuriale* is something like testimony that mercury acts on the sensitive nerves; and the more manifest physiological action of that drug is to produce a dilatation of the capillaries in a great number of important organs, and on a large extent of mucous surfaces. Orfila and Christison agree that in all cases of poisoning by mercury, when the first result is not chemical decomposition or corrosion of tissues, the leading symptoms are those of inflammation of the mouth, throat, and alimentary canal—salivation and mercurial erethism in one or more of its multifarious forms; bloody vomiting and purging, in conformity with Cohnheim's and Stricker's views of the permeability of the vascular walls by the blood-corpuscles.*

The influence of the splanchnic nerves and of their ganglionic centres is distributed over a very wide area of the vascular system. Dr. Schmidt, of Rotterdam, associates Addison's disease with a morbid condition of the abdominal sympathetic; and there is great reason for the suggestion, seeing that the general anæmia, the palpitating heart, the soft and jerky pulse, and the feeling of faintness and breathlessness on attempting any exertion, are all indications of heart and vascular disturbance, with which Dr. Addison, assisted by Mr. Quekett, did find disease of the semilunar ganglion in one case of leucocythæmia.† Professor Tigri, of Sienna, has also reported a case of hæmorrhage into the ganglia of the great sympathetic in which there was a bronzed skin and every other symptom of Addison's disease.*

The wide distribution of the influence of the splanchnics, as vaso-motor nerves, has its counterpart in the wide extent of the action of mercury and of iodine.

In the catalogue of remedial agents, it is no uncommon thing to find some one which acts differently at different periods of time, or as the dose differs. Opium is one of these; its primary operation being that of a stimulant to the motor fibres of the vaso-motor nerves; but, ulteriorly, it is a sedative to both sensory and motor fibres; and thus it acts on those fibres of the hepatic plexus which, on entering the transverse fissure of the liver, are distributed, right and left, over the coats of the hepatic artery and portal veins. It has, therefore, an effect diametrically opposite to that of mercury. But I can well understand how calomel and opium may be combined to produce the incitive influence of the one with the calming effect of the other, so as to turn the balance of function on the side of the fibres of Remak, the attribute of which I have assumed to be to regulate the stream of blood which is conveyed by the arterioles into the cell territory for healthy secretion and nutrition.

This tranquilising influence is possessed by conium, and, especially so, by the bromide of potassium.

Mercury influences the vaso-motor nerves of the intestines, precisely as it does those of the liver. By stimulating the motor fibres, a preternatural secretion from the glandular and epithelial structures is induced, just as it is by the section of the cœliac and mesenteric plexuses, as performed by Rudge on rabbits. Opium has an antagonistic action, as have also hydrocyanic acid, cannabis, and many astringents.

The *modus operandi* of diuretics generally is so different that we should have a clear apprehension as to what we propose to accomplish by their agency before we can expect to use them with success.

Experiments on the nerves we must take with some degree of circumspection, because it has been found that certain lesions of the medulla oblongata, or section of the vagus or pneumogastric, may be productive of diabetes; but if diabetes be regarded as the result of disturbance of the functions of the brain and nervous system, or as cause rather than effect of kidney disease, for which we have high authority,† then may we reason on the correlation of nerve and medicinal action.

In nephritis we have the pathological counterpart of the experiment by Eckhardt (p. 602), except that the urine is scanty; but that may depend on the disease affecting the capillaries of the tubuli uriniferi chiefly, and not the Malpighian capillary system.

Turpentine and cantharides produce the same effects, even to the sympathetic vomiting, and pain in the testicle, thigh, and leg, by the specific irritation extending to the solar and spermatic plexuses. Belladonna, aconite, and uva-ursi, either by stimulating the inhibitory nerves of Remak, or by subduing the force of the sensory and motor fibres of the renal plexus, have the very opposite influence.

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* Ueber entzundung und eiterung. Virchow's Archiv, 1867, vol. ii.

† Addison's Works, published by the New Sydenham Society, vol. 36, p. 213.

* Journal de Medecine de Chirurgie et de Pharmacologie. Bruxelles: Nov., 1870.

† Neubaur and Vogel on the Urine: translated for the New Sydenham Society, by Dr. W. O. Markham, p. 320.

I do not of course pretend to say that these drugs should be depended upon exclusively in nephritis, and that general or local depletion should be disregarded as a means of relieving the congestive turgor of the renal capillaries. My great object is to illustrate the proposition with which I started —namely, that as each element or nerve-fibre in the ganglionic centre has its special attribute, either of dilating or contracting the capillary vessels, so have we medicinal agents which either stimulate the motor nerve fibres of the vaso-motor nerves, and dilate the capillaries, or which act as sedatives on the motor and sensory nerve-fibres of the vaso-motor nerves, or give ascendancy to the nerve-fibres of Remak, and contract the capillaries.

If it be so, we have an explanation why the salts of potash, which have been prescribed empirically in nephritis, do more harm than good; for they enter into the category of such medicines as juniper, buchu, colchicum, and squills, which stimulate the motor fibres of the renal plexus, and increase the secretion of the salts which characterise the urine. We may also understand how it is that, in thus acting as diuretics, they do not necessarily induce diuresis. The additional stimulus which digitalis gives to the general circulation by its action on the heart, may, I suppose, affect the capillary system of the Malpighian bodies, and so flush the tubuli uriniferi and wash out the secretion of their epithelial cells. Hence the great advantage of digitalis in nephritis; for just in proportion to the outpouring of fluid from the Malpighian capillary system, the turgescence of the tubular capillary system is diminished. Digitalis has been regarded almost as a specific by the Germans especially, and by Foville in France, in those cases of maniacal disease in which there is disproportioned strength in the carotid and temporal arteries, together with high-colored and scanty urine.

In the straight tubuli uriniferi, which serve as conduits to the pelvis of the kidney, and in the ureters and bladder, we find the lamelliform epithelial cells which secrete mucus; and in most cases of preternatural mucous secretion from these parts, unconnected with mechanical causes, I can vouch for the beneficial action of ergota and bromide of potassium. In many forms of leucorrhœa, also, a combination of these drugs is very effective.

These last remedial agents are nervine sedatives to the cerebro-spinal system, and consequently to the sensory and motor fibres of the vaso-motor nerves. As such, they lower the pulse, diminish the temperature of the body, and act as anaphrodisiacs on the organs of generation; whereas phosphorus, by exercising an antagonistic influence, is a powerful aphrodisiac. I need scarcely insist upon the inseparable association of this attribute with an increased vascularity of the parts concerned.

From the facts which I have adduced, it may, I think, be inferred, and with moral certainty, that there is an invariable correspondence between functional activity and circulation: furthermore —and I have the authority of Schiff in asserting it—that the dilatation of the arterioles is neither the result of reaction after previous contraction, nor the mere result of an afflux of blood to an irritated part, nor is it independent of nervous action, but that it occurs under the influence of sensory and motor nerves.* It can scarcely be objected to the proposition with which I started that I have not adduced sufficient evidence to prove that vascular contraction is also the result of nervous influence; and it is impossible that one nerve, or one form of nerve, can perform the double office of dilating and contracting the calibre of vessels.

Seeing, therefore, that most pathological conditions are but modifications of physiological actions, and the effects of derangement of the operations of the vaso-motor nerves, on which the healthy functions of all organs depend, it appears to me that on the knowledge and due appreciation of such aids we may found a rational and a scientific system of therapeutics.

In these suggestions I do not, of course, include those remedial agents which act mechanically or chemically, but those only which have an influence on the vaso-motor nerves, and the great majority of medicines with which we are acquainted will fall into the category of these latter.

There is every reason to believe also that they may be grouped in accordance with some natural principle, just as plants which belong to the same natural order possess similar characteristic properties, physiological and therapeutical.

An attempt has been made by Dr. Broadbent to form chemico-therapeutical groups, and to show "that substances closely allied chemically must have an analogous action on the system, or that the diversity of their operation should be capable of explanation on chemical principles." In other words, "that chemical groups ought to form therapeutical groups."*

I have purposely abstained from alluding to alcohol as a remedial agent in inflammation, because of the conflicting opinions which have resulted from its employment. But if, by acting chemically on the tissues generally, it restrains the metamorphosis of the blood-corpuscles from rapid disintegration; if it can restrain the albumen in the blood, which it is the nature of many diseases to cause to escape; if by such chemical action alone it can render the walls of capillaries less permeable to the blood plasma which is flowing through them —and all these attributes it has been said to possess, —then, I think, the diffusible action of alcohol as a stimulant to the body generally, equalising the circulation, and re-establishing the balance of function which it is the nature of disease to disturb, must commend itself to our reason, as it is said to have shown itself beneficial in practice.

Clarges-street, 1871.

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PRELIMINARY NOTICE ON

THE TREATMENT OF EMPHYSEMA OF THE LUNGS BY ARTIFICIAL EXPIRATION.

By J. B. Berkart, M.D., &c.

Emphysema of the lungs, whatever cause may have produced it, chiefly consists of a permanent morbid dilatation of the air-vesicles, and consequent increase of the residual air within them. It is attended by the so-called asthmatic attacks, habitual dyspnœa, and cough, due to a catarrh of the bronchi, and has moreover a pronounced tendency to lead to organic disease of the heart. Whilst under normal conditions (according to Donders), the pressure on the inner surface of the thorax is in the different stages of respiration from 7½ to 30 millimetres of mercury less than that on its outer surface, on account of the loss of elasticity of the lung-tissue this difference is considerably lessened—nay, almost entirely abolished. The immediate consequence is "the permanent inspiratory condition" of the thorax, in which the diaphragm is prevented from reascending to its expiratory position, because the co-operation of the most potent factor—namely, the traction of the elastic lungs on its upper surface—is lost. Thus ensue the disturbances of respiration and circulation.

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* Comptes Rendus, vol. lv., Sept. 29th, 1862.

* Proceedings of the Royal Medical and Chirurgical Society, 1868, p. 84.

When the mechanism of the pathology of emphysema began to be understood, manual compression of the chest, as well as that by the act of vomiting and the application of electricity to the diaphragm, were used in the treatment of emphysema. But the result of that treatment is not at all satisfactory; because the pressure exerted acts at the same time on the outlets also of the dilated alveoli, and the mucous membrane of the bronchi being in a state of catarrhal swelling, the outflow of the air from the alveoli is prevented rather than facilitated. Better results were obtained by the inhalation of compressed air. The patients who have tried it do not cease praising its beneficial influence on their sufferings. But, apart from the costly apparatus, which is not within the reach of everybody, the inhalation of compressed air has only a temporary influence, by supplying a greater quantity of oxygen in a smaller volume of air. By it the intra-vesicular pressure is in no way diminished, nor is the residual air renovated to any considerable extent. A current of air from the greater (intra-) to the lesser (extra-thoracic) pressure soon establishes a balance; and if any difference of pressure previously existed, it now ceases, for the reasons just indicated.

The idea naturally suggested itself that, if it were possible to reduce the intra-vesicular pressure, and that also resulting from it on the thoracic surface of the diaphragm, by some mechanical contrivance, not only the want of oxygen would be satisfied, the emptied space being retaken by the influx of fresh air, but the compressed healthy air-vesicles might re-expand, and, by a methodical application of that contrivance, a part at least of the functional activity of the diaphragm might be saved. For the long-continued contraction of the diaphragm must ultimately lead to fatty degeneration of its muscular fibres, just as it does in other muscles. The catarrh of the bronchi, also due to the stasis in the remaining capillaries, must be benefited by it. The pressure being taken off from the venæ cavæ and the heart, the return of the venous blood from the greater and lesser circuit would be facilitated. Now in each ordinary quiet respiration 500 cubic centimetres of air are inspired, and as many expired. In emphysema, in spite of the long duration of expiration, which is three times that in health (F. Riegel), a much smaller amount than 500 cubic centimetres is expired, and consequently inspired also. It is therefore perfectly safe to use a contrivance by means of which air up to 500 cubic centimetres can be withdrawn at each expiratory act; and, on considering the nature of emphysema, it must be equally safe to withdraw as much at the *end* of each natural expiration. My views on this subject have been confirmed by the experiments of Dr. Haulk of Vienna. With a somewhat bulky instrument, a modified Hutchinson's spirometer, which, if safety is to be ensured, has to be worked by two assistants, he first used compressed air in croup and in emphysema. Subsequently he found that, if emphysematous patients were allowed to expire into an atmosphere the pressure of which is from four to eight inches of water less than that of the air, the relief they obtained was greater and more lasting than by the use of the compressed air: and his brilliant results have been fully corroborated by Drs. Hagel and Mader, of Vienna. I therefore designed two instruments, which Mr. Hawksley, of No. 4, Blenheim-street, New Bond street, constructed for me; and I have been using them for some time with the best results. The annexed woodcuts represent the instruments.

Fig. 1: A is an exhausting syringe, the piston-rod of which is divided to represent cubic centimetres, so that by means of the adjusting stud, B, the amount of action of the piston may be measured and regulated. The piston-rod is always raised by means of a helical spring fixed in the upper part of the cylinder, and exhaustion is produced by depressing it with the hand. The capacity of this syringe is known. H is a mask of metal, shaped so as to be readily adapted to the face, and enclosing both mouth and nose. The juncture of the edge of the mask and the face is effected by a cushion of india-rubber, which effectually prevents the escape or admission of air. It is attached to the face by a band round the head, or is held by the hand. Proceeding from the mask is the chamber, D, and the tube, E. At the point D within the chamber is an inspiratory valve, and at the point E an expiratory one. C is also an expiratory valve. F is an inspiratory valve, under command of the finger of one hand. The mask and syringe are united by about eighteen inches of stout tubing.

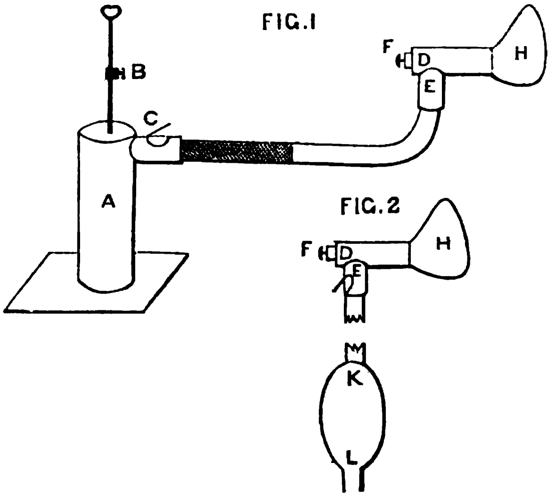

Fig. 2 represents the mask and valve detached from the syringe. The expiratory valve, C, in Fig. 1 is placed at E in Fig. 2. The necessary exhaustion is produced by an india-rubber ovoid, holding about 15 cub. in., and is compressed one or more times at the end of each expiration. The mask, H, is carefully fitted, and attached firmly to the face. On inspiration taking place air enters at F and D; upon expiring, the valve D closes, while E and C open. The last feeble flutter of the valve C indicates the end of expiration; then the spring valve, F, is closed with the finger, and the piston depressed the required amount, producing a further expiration. On removing the hand from the piston-rod, it rises as shown in the woodcut; and upon releasing the valve F, inspiration again takes place. In using the instrument shown in Fig. 2, at the end of expiration the ovoid is rapidly compressed and as rapidly released, when artificial expiration is produced. This way may be repeated as required.

The instruments are to be used twice a day for a quarter of an hour each time, and to be continued for some weeks. Patients of average intelligence are, after a few sittings, able to use the instruments themselves. Even after the first application of the instrument the patients experience great relief, and delight in showing that they are now able to take a deeper breath, and their oppression gradually ceases. The respiration becomes deeper, and its frequency diminishes from 25 to 16 per minute. The expectoration is at first increased, but easy, and soon ceases entirely.

Bronchorrhœa and great dyspnœa are no contra-indications; only in the latter condition the operation is frequently to be interrupted. As regards the amount of air to be withdrawn, the feeling of every patient is carefully to be consulted. I defer the analysis of my observations for some time; and I hope shortly to be able to lay down a series of rules, based on exact measurements and calculations, for the use of the instruments in individual cases.

Harley-street, Nov. 1871.

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ON

A VISIBLE STRIATION OF THE NORMAL CRYSTALLINE LENS.

By John Tweedy, Esq.,

Ophthalmic Assistant at University College Hospital.

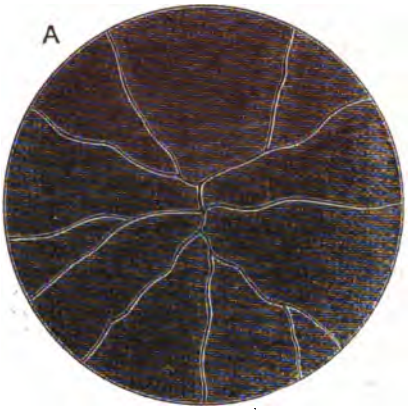
I AM anxious to direct attention to the presence of certain fine, dark, radiating lines, as seen, by oblique illumination, in the normal lens of the human eye, which I do not find described by any writer, and which I first saw in examining the following case.

J. W——, a tall, thin, healthy-looking young man, aged twenty-eight, came to University College Hospital on the 1st of November, complaining of weakness of the sight for the last three months, and of muscæ volitantes, semi-transparent beaded filaments &c., floating before the eyes. Otherwise the sight was good, patient being a Rifle Volunteer, and able to shoot at one thousand yards' distance. Health good. No history of syphilis.

On examination of the eyes by the so-called oblique illumination, fine dark lines were seen radiating from about the centre of the pupillary area in the lens of each eye. These lines appeared to be very superficial, and on the surface of the lens, immediately behind the level of the iris. On putting some of the ordinary solution of the sulphate of atropine within the eyes, the pupils dilated fully and regularly, and then the lines could be distinctly traced extending outwards to the circumference of the lens as far as the margin of the iris would allow of them being followed.

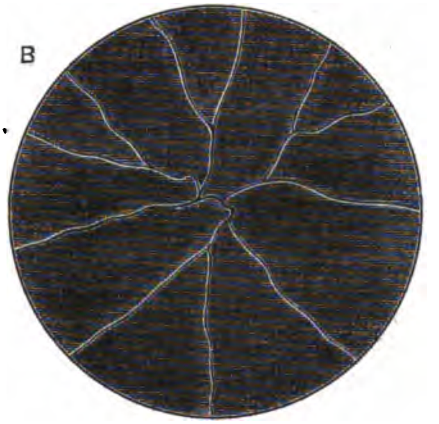
An examination of the accompanying figures will show that, although these lines are connected together, they do not all start from the exact centre of the anterior surface of the lens, nor do they all come from the same point; and that they are more or less irregularly curved in their outward course. Similar lines were seen in several other patients whom I then examined.

A



Right Eye.

B



Left Eye.

[These lines must be regarded as simply diagrammatic, for as seen in the living subject they do not present the white borders shown in the figures, but appear as black lines on a lighter ground, and this the engraver has been unable to represent.]

This case was shown to several gentlemen, but, at this time, all failed to recognise them; the reason of which was, I now believe, that they did not look in the right direction, nor concentrate the light properly. There is some difficulty in bringing the lines into view; but when this is overcome they are quite distinct. To see them, I use a biconvex lens of about 2¼-in. focus, and throw the light of an argand gas-burner obliquely on the eye of the patient, concentrating the light on the lens. I then look very obliquely (at about an angle of 130° to 140°) on the plane of the anterior surface of the lens, from the side opposite to that on which the light falls. I have never succeeded in seeing them from the front, nor can they be seen with the ophthalmoscopic mirror by transmitted light.

On November 20th this patient was taken to Moorfields Ophthalmic Hospital, where he was seen by seven or eight gentlemen, all of whom, with one exception, said they saw the lines when they took the precaution to examine in the manner I have just described. Mr. Streatfeild suggested that, in Mr. Bowman's work on "The Parts concerned in the Operations on the Eye," 1849, p. 68, there was a drawing of lines in the lens, the plan of which was very much like those I have figured in this case. Mr. Bowman says that the lines represented by him are the central planes of the lens. It will be seen, however, that the lines shown by this gentleman, which I presume are from the prepared lens, do not extend so far outwards as those I have described.

Mr. Wharton Jones was also asked to examine this case at University College Hospital, and he distinctly saw the lines, but said that he had never noticed them before. We then examined several other patients and found similar lines in all; and, in a case of commencing senile cataract, they could be seen intersecting the streaks of opaque lens matter, but apparently on a more superficial plane. In a case of advanced senile cataract, however, they could not be distinctly made out.

As far as I have at present been able to ascertain, these lines are not mentioned in any work as an ophthalmoscopic appearance. They are evidently not pathological, but only physiological; for I have now seen them in thirty or forty cases, of various ages, from two years and a half to seventy years, the majority being healthy boys and young men; in fact, I have never met with a case in which they were not to be seen, although sometimes they could only be brought into view after some considerable time had been spent in looking for them. In the child and younger persons they were less defined and distinct than in those older. As to their real nature, I do not at present venture to give an opinion. They are not light-colored and generally straight, as cataractous striæ are, but apparently darker than the body of the lens, and irregularly curved. In the adult they are ten in number, more or less, as in the case figured. I have not, so far, been able to make out satisfactorily whether the number differs in younger and older persons.

Stanhope-street, N.W., Nov. 1871.

◆

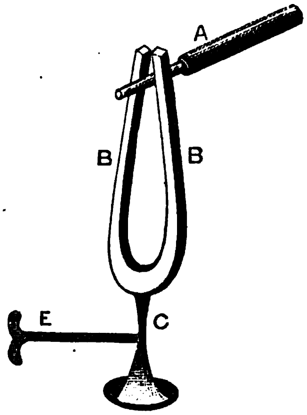
ON THE APPLICATION OF THE DIAPASON TO CLINICAL AUSCULTATION.

By W. Handsel Griffiths, Ph.D., L.R.C.P. & S. Ed.,

Assistant-Librarian to the Royal College of Surgeons, Ireland.

An extensive study of the science of acoustics with reference to clinical auscultation leads me to suggest, as a valuable adjunct to physical diagnosis, a modified form of the ordinary diapason, or "tuning-fork."

It is a well-known fact that the intensity of sound evolved, or the degree of resonance elicited, by a vibrating diapason, varies with the nature of the substance or surface on which it rests. I have been lately engaged in an experimental inquiry with a view to define the value of this principle as applied to clinical auscultation, and have been much gratified by the result. The form of diapason which I have hitherto used is intelligibly figured in the annexed cut. B B are the "prongs," which, on being suddenly separated by the "sounder," A, vibrate, and so evolve sound; C is the "connector" by which the sound is communicated to the small bell-like "dome" (similar in general size and shape to the small end of the stethoscope); and E is the handle by which the instrument is held in apposition with the surface of the organ or part under examination.



Now, although the variable resonance afforded by this instrument on application to different regions of the body is most remarkable, and beautifully indicative of abnormal condition or altered structure, I conceive that it is possible to invent a more delicate and definite indicator; but it would probably be in accordance with the same principle.

It is not in pulmonary affections only that such an instrument is likely to prove valuable; in almost every abnormal condition of almost every organ the indications afforded by it are likely to prove of inestimable service in forming a diagnosis.

Now I am not promulgating these suggestions as the result of mature or perfected research, but I have thought it expedient to publish them, crude as they are, in the hope that the attention of abler minds and more acute intellects may be attracted to the subject. The field of observation thus opened is, I believe, a wide one, and likely to repay a careful and protracted investigation. I trust that the co-operation of contemporary physicists and physicians will enable me, ere long, to offer to the profession a standard instrument, constructed on strict acoustic principles, which will afford definite and delicate indications of variable resonance, and which will be reliable as an index of altered structure and organisation.

I am not aware that the idea of applying the diapason, or any similar instrument, to clinical auscultation has hitherto been suggested; but if so, I trust that I may be corrected, and that the credit of priority will be claimed by the originator. At all events I am convinced that the subject, if not novel, is well worthy of revival.

In conclusion, I shall be grateful for any communications relative to the matter, and will faithfully record in future publications any suggestions or remarks of value.

17 Hume-street, Dublin, Nov. 1871.

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COD-LIVER OIL IN WHOOPING-COUGH.

By J. Prestwich, L.R.C.P. &c.

From time to time we see various methods of treating whooping-cough advocated; but no remedy can be prescribed with certainty until the pathology of the disease is more perfectly known. In this paper it is not my intention to discuss the many theories of pathologists or to promulgate any new theory of my own, but simply to report a few cases selected from more than thirty in my own practice, in order to show the value of cod-liver oil in this peculiar complaint, so fatal to infant life, or, if not fatal, so distressing to children and parents. The action of any remedy which produces benefit equal to that which I have experienced with this valuable agent ought to be widely circulated amongst the profession. The improvement following the use of cod-liver oil in my own practice has been such that for some time I have considered it as a specific for whooping-cough. As is well known, the spasmodic attacks of this complaint are almost always preceded for some days by inflammatory or catarrhal symptoms. During this stage it has been my usual practice to treat all cases as ordinary attacks of catarrh, and afterwards to administer the oil. A fact worthy of notice is, that the majority of children had considerable enlargement of the concatenate glands; and the consequent reduction in the size of these glands may account to some extent for the benefit derived. It is also a remarkable circumstance that not one death has occurred where the oil has been administered and has been retained on the stomach.

Case 1.—E. G——, aged nine months; male; a poor anæmic child, first seen Oct. 29th, 1869. Had had cough for ten days, with distinct whoop; paroxysms every hour, both night and day; was feverish; tongue furred and dry; pulse quick; respiration frequent, evidencing bronchitis. After pursuing the ordinary treatment until Nov. 13th, without any abatement of the cough, and the child being weak and emaciated, I ordered cod-liver oil, half a fluid drachm, four times a day. On the 14th, as he could not retain the ordinary oil, he was ordered Fox's "palatable cod-liver oil," one fluid drachm, every four hours. On Dec. 7th he was convalescent.

Case 2.—M. H——, female, aged eight months, was seen Nov. 20th, 1869. Whooping-cough came on every two hours during the night, and nearly as often in the day. Vomiting occurred after each paroxysm. Tongue clean. She was ordered to take a full drachm of Fox's cod-liver oil every four hours. On Dec. 6th she was nearly well. The patient was not seen afterwards.

Case 3 (which deserves special attention).—G. S——, male, aged four years. Had had cough for nine months, which came on every hour in the night, and six or eight times in the day, the paroxysms being severe, with scanty expectoration. He was thin, pale, and emaciated. Lobelia and chloral hydrate with syrup of white poppies was prescribed until Nov. 25th, with very little improvement, the paroxysms being as frequent as before. Two drachms of Fox's cod-liver oil were administered every four hours. On Dec. 5th the cough was almost gone, and there was no whoop; he was fuller in body, and was improving rapidly. On the 20th the patient was convalescent.

The above are three specimen cases taken from a list of thirty-three to whom Fox's "palatable" cod-liver oil was given because of its singularly easy retention on the stomach. Some patients did not experience the same marked improvement as those reported, but all obtained decided relief. Now the question arises—Is the improvement to be attributed solely to the beneficial action of cod-liver oil, or has Fox's preparation a peculiar influence over the disease, besides its suitability to the palate? I shall be glad to have the opinion of other practitioners on this point.

Oldham, Aug. 21st, 1871.

A Mirror
OF THE PRACTICE OF
MEDICINE AND SURGERY
IN THE
HOSPITALS OF LONDON.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

UNIVERSITY COLLEGE HOSPITAL.

CLINICAL REMARKS ON A CASE OF TUBERCULAR CONSOLIDATION OF THE LUNG, COMPLICATED WITH BRONCHITIS.

(By Dr. Wilson Fox.)

The patient is a man of middle age, has a cough accompanied by free expectoration, and is rather wasted. On examination of the chest, the right side was found to be flattened at the apex and the base anteriorly. In the former situation were observed the physical signs of a cavity; in the latter, flattening, dulness, and absence of respiratory murmur, indicated consolidation and shrinking. The left side was prominent, dull on percussion in places, and yielded the sounds characteristic of chronic bronchitis. The heart's apex was felt to beat to the right of the sternum, but the sounds were healthy. It appeared that since the patient had had "inflammation" of the right lung two years previously, his health had gradually but steadily declined. Shortly before admission the left lung suffered an attack of bronchitis, of which, as just stated, it now presents chronic symptoms.

Dr. Wilson Fox pointed out that this case afforded an illustration of the truth that pulmonary phthisis is not a disease distinct and independent in its origin, but one which is liable to be grafted on to any affection which impairs the nutrition of the lungs, in virtue of a predisposition which consists in an irritable condition of the lymphatic system. Given this irritability, an individual is liable to become the subject of tuberculosis under any circumstances which increase the vulnerability of his tissues; without it he will probably enjoy immunity from that disease.

Viewed in the light of the physical signs, the patient's history would be as follows:—Pneumonia of the right lung was followed by tuberculosis, of which the present results were a cavity at the apex, due to softening and breaking down of the lung-tissue, and consolidation of the base. The left lung, which probably became compensatively emphysematous, in consequence of the induration and contraction of the right one, had recently suffered from an attack of bronchitis, which had excited the same morbid process as that which followed pneumonia in the right, as shown by patches of dulness indicative of the existence of nodules of consolidation. That the heart was found to beat to the right of the sternum was in the main to be attributed to its having been drawn over by the shrinking of the right lung, though, in some small measure, it might also be due to emphysema of the left lung and dilatation of the right side of the heart produced by the pneumonia on the right side. It did not follow from there being but little observable jugular pulsation that the right side of the heart had not undergone dilatation, for the tricuspid valve might have maintained its competence in consequence of the diminution in quantity of the blood which might be expected to result from prolonged deficiency of oxidation.

In reference to the treatment of the patient, Dr. Wilson Fox remarked that the word "expectorant" is made to apply to two classes of drugs which have opposite effects, and which therefore must be used with discrimination where it is desired to act upon the bronchial mucous membrane. Lobelia and ipecacuanha are expectorants in the strict sense of the term, inasmuch as they promote secretion; squill, tolu, senega, and other remedies of this class, on the contrary, have an opposite effect. In the present case, he should at first restrict the patient to a mild and nourishing diet, and, with a view to checking the expectoration, order him to take a draught containing opium and ammonia, and to inhale the vapor of water medicated with one drop of creasote to the pint, and such a proportion of conium as might be expected to give repose to the bronchial tubes without interfering with the expectoration of their discharge. After the lapse of a short interval he hoped to give him a full diet, with steel and cod-liver oil.

KING'S COLLEGE HOSPITAL.

OPERATIONS ON THE 16TH NOVEMBER.

(By Sir William Fergusson.)

Operation for Internal Hæmorrhoids.—The patient was a man of middle age, who had suffered from internal hæmorrhoids, with prolapsus, for a space of fourteen years. When he was placed on the table, a mass, consisting of three large hemispherical protuberances, and several smaller ones of a dusky-red color, was projecting from the anus. The former were selected for operation. The procedure adopted by Sir Wm. Fergusson was that of ligature. A threaded needle was passed through each of the three prominences, the halves of which were then separately tied with the portions of the divided thread. Some of the loose skin around the anal aperture and the summits of the ligatured hæmorrhoids were then removed with scissors. Before the removal of the patient a suppository and the prolapsed mass were, at the same time, returned within the anus. The operator attached importance to the use of the scissors just indicated, as tending to remedy, by consequent contraction, the unnatural laxity of the spincter ani which long-standing prolapse inevitably produces, and as lessening, by removal of some of the thickened skin and mucous membrane, the chance of the formation of new hæmorrhoidal masses. The surgeon, he observed, errs more often in the sparing than the free use of the scissors in similar cases.

Disease of both Hips; Tenotomy; question of further operation.—A delicate-looking boy, suffering from disease of both hips, was next brought into the theatre in order that his condition might be carefully examined under chloroform. The right thigh was flexed and adducted, and the trochanter stood out prominently over the dorsum ilii. There were no sinuses or scars, but a small chronic abscess was to be felt beneath the skin of the anterior aspect of the upper third of the thigh. Movement at the knee-joint was quite free. The left thigh was somewhat less flexed than the right, and the head of the bone had not left the socket, but several pus-discharging sinuses communicated with the joint. On this side also there existed no impediment to free extension of the leg. Sir W. Fergusson observed that the disease of the right hip had completely subsided after permitting, among other mischief, the escape of the head of the femur on to the dorsum of the ilium, which had been followed by a degree of flexion and adduction which every appliance resorted to had failed to reduce. The disease of the other hip had followed a somewhat different course, but the patient was nevertheless reduced to the deplorable condition of not being able to plant either foot on the ground. Sir W. Fergusson then divided the tendon of the right adductor brevis, and the inner edge of that of the pectineus, with marked benefit to the position of the limb; but the thigh was not well brought down from the trunk until the tendon of the sartorius at the anterior superior spine of the ilium had been likewise divided. He reserved further interference for a future occasion; but expressed a hope that by a future operation—probably excision of the heads of the femora—the use of both limbs would be in a measure restored.

Mr. Henry Smith operated on a girl for *fissure of the lobule of the ear*, by paring the edges of the gap and bringing them together with two hare-lip pins and figure-of-8 sutures, as for hare-lip. The deformity had arisen by an earring ulcerating through the lobule.

Provincial Hospital Reports.

ST. MARY'S HOSPITAL, MANCHESTER.

SEVEN CASES OF OVARIOTOMY.

Communicated by Mr. Henry Runcorn, House-Surgeon.

(Under the care of Dr. Lloyd Roberts.)

Case 1. *Removal of the right ovary, with a fibro-cystic tumour of the uterus weighing twenty-three pounds; recovery.*—Margaret M——, aged thirty-two, single, noticed the enlargement twenty-seven months before admission. It was not more marked on one side than the other. She had never been tapped. The abdomen measured forty-three inches in girth over the umbilicus, and from the ensiform cartilage to the pubes twenty-three inches. Fluctuation could be felt over the whole of the tumour. The uterus was movable, slightly hypertrophied, and was raised with the tumour when the latter was elevated by external manipulation.

Ovariotomy was performed under the influence of bichloride of methylene, administered by Dr. Junker's apparatus. An incision, three inches in length, was made down to the tumour, considerable hæmorrhage arising from the divided vessels of the abdominal wall, which was arrested by torsion. Some firm adhesions between the front of the tumour and abdominal parietes were separated, and a large syphon trocar was plunged twice into the tumour, but no fluid passed through it. The primary incision was enlarged to the extent of one inch above the umbilicus, and the tumour was brought through the abdominal wound. The pedicle which was three inches broad and very short, was secured by a circular clamp. Some blood-clots were removed from the abdominal cavity, and the wound was brought together by five deep sutures of silk, and dressed with carbolised lotion and plaster, without the use of any abdominal bandage. With the exception of some dysuria on the sixth and seventh days, the patient had not a single unfavorable symptom. The clamp was removed on the sixteenth day, and a smaller one which was substituted was finally removed on the twentieth day. She left the hospital on the forty-sixth day.

The tumour when examined was found to consist of the right ovary in an early stage of cystic disease, with a fibro-cystic tumour of the uterus. It was pedunculated. The diseased ovary and Fallopian tube, except its fimbriated extremity, were firmly united to the tumour.

Case 2. *Ovariotomy; unilocular ovarian cyst of the left side; ligature cut short; pedicle returned.*—Elizabeth P——, aged forty-nine, was a widow, and had had one living child and one miscarriage. The catamenia had ceased four years previously. A year after that event she noticed a swelling in the left side of the abdomen. On admission the abdomen was found to be large and prominent, yielding general fluctuation over the front and sides of the tumour and resonance over both flanks. The girth at the umbilicus was thirty-eight inches; from the ensiform cartilage to the pubes measured eighteen inches; the uterus was two inches and three-quarters lower in the pelvis than normal. Fluctuation was felt in Douglas's space on the left side. The patient had not been tapped.

The tumour was exposed and tapped with a large syphon trocar. The fluid withdrawn presented a dark grumous appearance. The tumour was then drawn through the wound, and the pedicle, a broad and fleshy one an inch and a half in length, was secured by a ligature which was cut short, and, the tumour having been separated, the stump was dropped into the abdomen. No fluid entered the peritoneal cavity. The abdominal wound was brought together by silk sutures, and dressed as in the above case. The incision was three inches in length.

With the exception of flatulence which troubled this patient for a few days, she presented no unfavorable symptom. The wound had healed by the seventh day, and on the eleventh she was convalescent. The tumour was unilocular, and with its contents weighed 19½ lb.

Case 3. *Multilocular ovarian tumour of the left side; ovariotomy with use of clamp; recovery.*—Annie B——, aged twenty-four, was married, but had had no children. She first menstruated at twelve years of age, and had been regular ever since. The swelling commenced five years before admission after a fall. The abdomen measured, over the umbilicus, forty inches; from the sternum to the pubes, twenty-one inches. The globular fluctuating tumour inclined to the left side. Fluctuation could be felt in both flanks. The uterus was normal in size, with a small cervix. The tumour compressed the bladder and caused frequent desire to urinate. She had never been tapped.

The operation was performed under the influence of bichloride of methylene. One large cyst was tapped in front, and a smaller cyst was then emptied. The two emptied cysts, and a number of smaller ones unopened, were drawn through the wound, and the pedicle, two and a half inches in length, was secured by a clamp. The abdominal incision was secured by silk sutures. The incision in this case was two inches in length. On the fourth day after the operation the patient had an urgent attack of dyspnœa with bronchial râles, which was subdued by a linctus, sinapisms, and brandy-and-water. The wound had healed by the eighteenth day, when the clamp was also removed.

Case 4. *Multilocular ovarian tumour of the left side, weighing twenty-seven pounds and a half; recovery.*—Martha H——, aged twenty-eight, was a married woman. The abdomen was much distended, irregular in shape, and bosselated. The fluctuation was localised; the swelling on the left side was solid; the linea alba was drawn towards the left side. The cyst on the right side reached to the ensiform cartilage, was conical in shape, and fluctuated distinctly. The girth at the umbilicus was forty-one inches and a half; from the sternum to the pubes measured eighteen inches and a half. The uterus was slightly prolapsed, and the uterine sound passed to a distance of two inches and a half. Owing to the extent to which the breathing had been embarrassed by the tumour, the patient had not been able to lie in bed for three weeks. She had not been tapped.

The operation was performed under the influence of bichloride of methylene. After the abdomen had been opened in the usual manner, the bleeding which occurred from the vessels in the cut surfaces had been arrested, and the peritoneum divided, the right cyst came into view and was tapped. The fluid was dark-colored and grumous. A second cyst was then tapped, and about three pints of a similar fluid were drawn off. Traction on the collapsed cysts having brought them outside the abdomen, a third came into view. This was drawn through the wound without being punctured, as well as a number of other smaller cysts. The pedicle, three inches long and an inch broad, connected with the left ovary, was secured by a clamp, and the tumour was then separated. The wound was brought together and dressed as in the previous cases. The sutures were all removed by the seventh day, the clamp came off on the eleventh day, and her recovery was uninterrupted from this time.

Case 5. *Removal of a multilocular ovarian tumour weighing forty-six pounds; firm adhesions; use of clamp; recovery.*—Belinda H——, aged thirty-three, had had four children. The swelling commenced two years and eight months before admission. Eighteen pints of fluid had been removed by tapping three months previously. The abdominal walls were very tense, and fluctuation was very distinct. In the left iliac region was a solid substance which was very painful on pressure. There was considerable œdema of the abdominal parietes, both lower extremities, and the lower part of the back. No albumen was found in the urine. The girth at the umbilicus was forty-eight inches and three-quarters, and, from the sternum to the pubes, measured twenty-three inches. The uterus was lower in the pelvis than normal, and the cervix was directed towards the left side. The uterine sound passed to a distance of four inches and a half. The uterus was mobile.

Ovariotomy was performed under the influence of bichloride of methylene. The tumour was exposed by an incision three inches in length. There were firm adhesions between the tumour and the abdominal wall, which were with difficulty broken down. A cyst was then tapped and the fluid removed, and another cyst was emptied. The hand was then passed into the abdomen, and some firm adhesions which existed in the iliac and lumbar regions were broken down. Other adhesions above and behind were next separated. The incision was then lengthened upwards by one inch, and the tumour having been brought outside, some omental adhesions were detached, and the pedicle (about three inches in length) connected with the left ovary was secured with a clamp and divided. The cyst-walls were so friable that in bringing out the tumour one of the unemptied cysts adherent to the abdominal wall gave way, and the contents flowed into the abdomen. A great deal of time was spent in clearing out the cystic fluid and blood from that cavity. The wound was brought together by five silk sutures.

This patient had not a single unfavorable symptom. The wound, except where the pedicle issued, was healed on the fifth day; the whole of the sutures and the clamp were removed by the twelfth day; and the patient made a rapid recovery.

Case 6. *Solid ovarian tumour; ascites; ovariotomy; fatal result.*—Susan R——, twenty-four, unmarried, first noticed the swelling two years before admission. She had been tapped about twelve months previously, and again in three months. The abdomen was tense and fluctuating, especially in the flanks, and was resonant in front to the right of the umbilicus. A distinct hard tumour could be felt superficially to occupy nearly a central position with the umbilicus for its centres, and to extend above, below, and slightly to the left of it. The girth of the abdomen was forty-two inches.

The tumour was firmly adherent to the diaphragm, omentum, and abdominal wall; a considerable time was required to separate the adhesions, and the pedicle, which was four inches long, narrow, and attached to the left ovary, was secured by a clamp, and the tumour separated. The ascitic fluid was then removed from the abdomen, and the wound closed with silk, as in the previous cases. At 4 P.M. the patient had somewhat rallied after the operation. Her pulse was 120; she complained of great pain in the abdomen, which was relieved by a morphia suppository. She passed a very restless night, and on the following morning her pulse was 125; she dozed occasionally; still complained of severe pain in the abdomen, and was ordered iced brandy and iced milk. At 9 P.M. her pulse was 130; the abdominal pain had increased; she was very restless, and complained of flatulence. At 9 A.M. on the third day her pulse was 130; vomiting began during the morning and she sank exhausted at 5 P.M.

At the post-mortem examination the peritoneum, omentum, and intestines were much injected. About three quarts of bloody fluid in the abdominal cavity; right ovary healthy; uterus much atrophied; remains of adhesions seen in the omentum, diaphragm, and abdominal wall. The tumour weighed 4 lb. 2 oz. The ascitic fluid weighed 20¼ lb.

(Under the care of Mr. Walter Whitehead.)

Case 7. *Removal of multilocular ovarian tumour weighing twenty-five pounds; recovery.*—M. C——, aged fifty-nine, had been married thirty-seven years, and had borne seven living children and had two miscarriages. She first noticed an increase in the size of her abdomen about ten months before admission, but could not remember that it was greater on one side than the other. She also observed, about the same time, repeated blood-stains on her linen, as though she had been menstruating, although that function had ceased sixteen years previously. The circumference round the umbilicus was forty-one inches.

Ovariotomy was performed under the influence of the bichloride of methylene. The tumour was removed by a small incision; uniform and extensive adhesions to the parietes were broken down, several cysts of variable size were emptied and drawn out, the remaining mass of semi-solid matter and aggregation of small cysts were also drawn through the abdominal wound. The pedicle was secured by a clamp, and the wound closed by silk sutures. The tumour and contents weighed twenty-five pounds. The patient made a rapid and uninterrupted recovery.

NORTH STAFFORD INFIRMARY.

OVARIAN TUMOUR IN AN INFANT; OPERATION AT THE AGE OF THREE YEARS.

(Under the care of Mr. ALCOCK.)

IN the following instance ovariotomy was performed at the age of three years, which is probably the earliest on record. Unfortunately the result was not favorable.

The patient was first seen by Mr. Alcock when about two years old. She then presented an enlargement of the abdomen, which was obviously due to an accumulation of fluid. Though she was pale-looking, her general health appeared to be good, and her movements were much less interfered with than the great distension of her abdomen would have led one to suppose. She had been noticed to be undergoing abdominal enlargement for six or seven months. On proceeding to tap the abdomen a few weeks later, Mr. Alcock first became aware of the real nature of the patient's disease, which, in consequence of her extreme youth, he had not up to that time suspected. On introducing a trocar he withdrew something less than a pint of clear straw-colored fluid, and that only with some difficulty. By means of a probe passed through the canula, he felt an elastic body which was evidently the wall of a fluid-containing cyst. This he attempted to pierce by reintroducing the trocar and thrusting it backwards, but the cyst seemed to recede, and so to evade puncture.

The consent of the parents being withheld, no further steps were taken until about nine months later, when the child was admitted into the infirmary, and about nine and a half pints of fluid, which presented a remarkable appearance on account of the large amount of cholesterine it contained, was withdrawn from the cyst. Through the flaccid abdominal walls a large nodulated tumour could then be distinctly felt in the right iliac fossa. The umbilicus, which had all along protruded, retained its prominent position, and could not be made to recede.

A fortnight later, the abdominal puncture having healed, Mr. Alcock removed the tumour. The operation was accomplished only after tedious tearing and dissection, in consequence of the tumour being universally adherent, except in front, where there was a free surface large enough to allow of the introduction of the hand. The pedicle was secured with a ligature, which was cut off short and allowed to remain in the abdomen. No hæmorrhage of importance occurred; and when the patient had been replaced in bed she seemed as well as could be expected under the circumstances. She shortly, however, began to suffer from sickness, which was relieved by sucking ice and drinking small quantities of champagne. On the following day there were evident symptoms of exhaustion, and, about forty-eight hours after the operation, the patient died.

The bulk of the tumour consisted of a cyst with thick and rather vascular walls, within which numerous septa converged towards a common central mass. Near the pedicle was a number of smaller and independent cysts, several of which had been punctured in the operation in order to facilitate extraction. From the anterior wall of the main cyst there sprang a smaller one, of about the size of a walnut, which had occupied the protruded umbilicus, out of which it had been peeled with difficulty.

Medical Societies.

FEBRUARY—MARCH.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

MR. CURLING, F.R.S., President, in the Chair.

CASES OF INTER-MENSTRUAL OR INTERMEDIATE DYSMENORRHŒA.

BY WM. O. PRIESTLEY, M.D.,

Professor of Obstetric Medicine in King's College.

THE author pointed out that, although much had been written concerning dysmenorrhœa, and several forms of it had been described in accordance with the pathological views taken of its causes, the description of the several varieties was ordinarily limited to the time of the catamenial period, with the two or three days additional which may precede and follow the menstrual flow. From time to time, however, cases of a more obscure kind presented themselves, in which the chief suffering is remote from the actual menstrual period, but comes on, nevertheless, with the same punctuality, and is probably dependent on organic changes associated with the production of the catamenia. Probably other practitioners had observed like instances, as they were not unfrequent, but as the author had met with no description of them, he brought the subject before the Society as a fragmentary contribution to the pathology of uterine affections, which might possibly evoke further elucidation by discussion.

In all the cases detailed severe pain was experienced by the patients midway in the menstrual interval. The pain commonly came on about fourteen days after a catamenial period, and, after lasting a variable number of days, ceased before the supervention of the next expected period. In one case, the pain, beginning midway in the interval, ran into the following monthly period, and was relieved by its flow. The suffering was constantly referred to one or other ovarian region, and in three cases out of four marked tumour, or thickening from old adhesions, was found in that locality.

The reason for the occurrence of pain in the inter-menstrual period, and with such regularity, was not, in the present condition of our knowledge, perfectly obvious. A study of the physiological and pathological conditions left little doubt, however, that it was due to perturbations in the function of "spontaneous ovulation" habitually going on in the ovary. Hypertrophy of the structure of the ovary, or thickening of its indusium, would lead to undue vascular excitement, and impede the advance of ova to the surface in their attempts to attain maturity. It was not unreasonable to suppose, from all the known facts of the case, that preparation for an approaching period began in the ovary ten or fourteen days before the occurrence of the monthly uterine discharge, and if the initial steps in the process of ovulation were opposed by certain pathological conditions, pain would ensue. Nay, in the absence of distinct organic change, it might readily be imagined how special irritability in the ovary would cause an unusual amount of disturbance whenever there was occasion for the exercise of fresh activity in the organ. This latter class of cases would partake more or less of a neuralgic character.

The treatment would depend on the pathological condition as ascertained by examination. The pain being only a symptom, it would be needful to inquire into the cause; and if there were tumour, or thickening depending on former inflammation, absorbent remedies would be indicated. If no organic change of structure could be detected, anti-neuralgic remedies, such as quinine, iron, and arsenic, would best answer the purpose of cure.

Dr. Elam asked how far this idea would correspond to that commonly entertained with regard to fecundation.

Dr. Priestley said the facts in nowise militated against the ordinary ideas.

ON THE PATHOLOGY OF SCARLATINA, AND THE RELATION BETWEEN ENTERIC AND SCARLET FEVERS.

BY JOHN HARLEY, M.D. LOND.,

Late Physician to the London Fever Hospital, Assistant-Physician and Joint Lecturer on Physiology to St. Thomas's Hospital.

In the first portion of the communication the author treats of the morbid anatomy of scarlatina, and gives the details of twenty-eight cases of his own observation. Of these, the greater number died on days ranging consecutively from the third to the fifteenth; the remainder died on the seventeenth, twentieth, twenty-fourth, twenty-ninth, thirty-third, forty-first, and sixty-ninth days. More or less albuminoid and fatty degeneration of the kidneys existed in six of the cases, and these died on the fifteenth, seventeenth, twentieth, twenty-ninth, forty-first, and sixty-ninth days respectively; the kidneys were healthy in the remainder. The pathological changes common (with a few exceptions depending chiefly upon the time of the decease of the patient) to all the cases are as follows:—

1. *The formation of fibrinous clots in the heart and great vessels during a pyrexial condition at any period of the disease.* This is the commonest cause of death during the early stage of the disease. It is indicated during life by the reduction, often very sudden, of a full and bounding pulse of 120 to a dribble of 150 or 160 almost imperceptible impulses; and the failure of the heart's action is commonly attended with orthopnœa and delirium from obstruction of the pulmonary and cerebral circulation. On opening the body very soon after death, before it has lost a degree of temperature, and while the blood is therefore hot and fluid, the right heart will be found distended partly with dark blood, which coagulates on exposure—and partly, sometimes chiefly, with a large, firm, white, bifid clot, continuous through the auriculo-ventricular opening. Each portion is interlaced with and firmly adherent to the tendinous cords and outstanding muscular bands of the cavity in which it lies; and each portion sends a rope-like prolongation into the orifice of the great vessel connected with the cavity in which it is situated—the one entering the ascending cava, and the other the pulmonary artery. These processes frequently occupy half the area of the blood-tubes, and are prolonged in ramifications corresponding to those of the bloodvessels, upwards into the cranial cavity, and laterally into the lungs. On withdrawing these partial casts of the great vessels, they may often be found nine inches long, and to have occupied vessels of the sixth and seventh degree of ramification.

2. *Marked derangement of the hepatic function.*—The bile was examined in 20 cases. In 5 only were the characters of the secretion normal, and in these cases death occurred on the third, fourth, twenty-fourth, forty-first, and the sixty-ninth days respectively—days probably in these particular cases too early for the development, and too late for the persistence, of any great derangement. In the remaining 15 cases the bile was in a deteriorated condition. In two, 10 and 15, the gall-bladder showed decided marks of inflammatory action, the coats being injected, and the mucous membrane rose-colored. In Case 10 there was a complete absence of bile, the mucous membrane being merely moistened with a few drops of colorless alkaline fluid. In 3 other cases the bile had a natural greenish-brown color, but it was greatly deficient, as was that of the remaining 10 cases, in solid matters. Thus the specific gravity in none of the 13 cases exceeded 1014, and the amount of solid matter in 1000 grain measures in no case amounted to more than 36·4 grains, or less than one-third of the normal amount. In one case there were only 11·1 grains of solid matter in 1000 grain measures of the bile. In the majority of the cases the bile was turbid from epithelial *débris*, but on standing it became clear and transparent, and resembled pale urine. In all the 13 cases there was a notable deficiency of biliary acids, and in 2 a complete absence. The coloring matter of the bile was present in every case, and as the fluid trickled over a white surface it usually left a bright gamboge colored track. The contents of the intestines agreed with this condition of the bile. If, as rarely happened, the bowel contained solid fæces, it was of a pale ochre or sulphur color. But the fæcal matters were commonly fluid, grumous, or flocculent, often slimy, and of a pale ochre color. Such, also, were the characters of the stools before death in many cases.

3. *General inflammation of the lymphatic glands*, usually confined to those of the neck, but occasionally extending to those of the extremities; of the spleen and mesenteric glands; and of the whole of the solitary and agminated glands of the alimentary canal, but commonly affecting only those of the fauces, and of the ileum and colon. The morbid appearances due to this general inflammation of the lymphatic system were remarkably uniform, and they were observed in every case. The tonsils and solitary glandulæ of the tongue, and the external glands of the neck, were perceptibly affected in every case. In several cases large buboes formed in the neck; in three (15, 21, and 22) these were associated with diffuse cellulitis, and purulent infiltration; and in Case 22 the popliteal and axillary glands and their surrounding connec-

tive tissue were similarly affected. In these cases the glands themselves were slow to take on suppurative action, and although they were generally much enlarged and purple, comparatively few had softened centres. The spleen was enlarged in 23 cases, and in five of these (Nos. 9, 12, 14, 25, and 26) it was increased to nearly twice its ordinary bulk. In two others it was not examined; and in the remaining two cases (10 and 27, in which death occurred on the fifth and sixty-ninth days) it was of the normal size. The mesenteric glands were swollen and inflamed in every case. In Cases 2, 6, 7, 8, 12, 19, 22, and 24, the mesentery formed a thickened, heavy, lobulated mass, and many of the glands were as large as walnuts or pigeons' eggs. In Case 13 (death on the seventh day) there was only slight swelling of these glands, whereas in Cases 26 and 28, in which death occurred on the forty-first and sixty-ninth day respectively, the enlargement still persisted. Even the small glands in the attached borders of the transverse and descending mesocola were found purple and turgid. The solitary glanduæ of the ileum were in a condition of psorentery—i.e., forming white granular and more or less hard elevations, like a thick sprinkling of large sago-grains upon the mucous membrane—in fourteen cases. In six other cases, the solitary glandulæ were only partially affected, the swelling was more diffuse, the glandulæ being only moderately raised, but they were always deeply injected, and in some cases had an abraded appearance. In three cases, in which death occurred on the eleventh, seventeeth, and sixty-ninth days of the fever, there was only very slight swelling of a few of these glands; and in four cases, in which death occurred on the fifteenth, twenty-fourth, twenty-ninth, and thirty-third days respectively, the glandulæ were altogether unaffected. The agminated glands of the ileum were more or less swollen and inflamed in every case but one (25), that in which death happened on the thirty-third day. In Case 21 (death on the seventeenth day from suppurating buboes in the neck), there was only very slight swelling. In all the other cases the results of inflammatory action were decided, and in many cases severe. The glands were commonly raised the eighth of an inch above the surrounding mucous membrane, than which they were always more deeply injected. In the greater number of cases the difference was very striking, the mucous membrane being generally pale, and sometimes thin and bare, while the agminated glands were of a vivid red or claret color. The inflammatory action was usually confined to the glands in the lower third of the ileum, but in three or four cases the whole of the patches from the jejunum downwards were affected. In some of the larger glands isolated foci of inflammatory action were occasionally observed. The interfollicular ridges were often the eighth of an inch wide, giving to the paler glands a spongy appearance; but these ridges were as often vascular, with fine hair-like turgid vessels, and in some cases they were prolonged into folds a quarter of an inch in length (see Cases 3, 6, 7, 8, 12, and 15, in which death occurred on the third, fourth, fifth, sixth, and ninth days). In two or three cases an almost bleeding gland had a softened abraded surface. The mucous membrane of the ileum was itself severely inflamed in two or three cases, and was covered by a thick, adherent layer of white opaque mucus. The solitary glandulæ of the large intestine were enlarged and inflamed in eight cases (Nos. 4, 6, 7, 9, 16, 17, 19, and 22). In one of these (9) there was acute desquamation of the mucous membrane of nearly the whole of the bowel. In another case (27), the cæcum was severely congested. In those of the remaining cases in which the large intestine was examined, it was quite healthy.

After taking the above described pathological conditions into one general view, it appears that there is an increase of fibrin in the blood during an attack of scarlatina, and that death is likely to occur during the first week from its deposition in the heart and great vessels; that the condition of the biliary function is such as to lead to an outbreak of diarrhœa, if this has not already happened; that a latent enteritis, sometimes general, but commonly only affecting the solitary and agminated glands, exists in a high state of development during the pyrexial stage of scarlatina, and ready to declare itself openly upon very slight provocation; that this intestinal affection is only a part of a general lymphatic inflammation which involves the whole of the lymphatic system, including the mesenteric glands and the spleen, in one common action; and, further, that this condition may persist in some degree, either in the bowel or the mesentery, as late as the sixty-ninth day, and without any outward indication of its presence throughout.

From this view *one general conclusion* is inevitable—viz., that the pathological changes accompanying an ordinary attack of scarlatina include all those of the first stage of enteric fever, and that the transition from one disease to the other is but a natural pathological sequence, readily determined by any cause which may increase the intestinal irritation.

The proofs of this interchange, or sequence, constitutes the second part of the subject, and contains accounts of the following original observations:—

Case 29. Scarlatina. Convalescence on the thirteenth day. Relapse during sojourn in the hospital, with scarlet rash on the twenty-eighth day; fully developed enteric fever on the thirty-second day; and convalescence on the fiftieth day.

Case 30. Scarlatina. Convalescence on the nineteenth day. Relapse during sojourn in the hospital on the thirty-first day, followed during the next ten days by fully developed enteric fever; convalescence on the sixty-fourth day.

Case 31. Scarlatina. Convalescence on the thirteenth day. Discharged from the hospital on the twenty-third day. Supervention of enteric symptoms on the thirty-seventh day. Readmission in a typhus condition, and suffering from grave enteric fever on the fifty-eighth day; convalescence on the eighty-first day.

Case 32. Severe attack of scarlatina. Convalescence on the twenty-seventh day. Slight relapse, with a trace of albumen in the urine, on the thirty-second day. A second relapse on the forty-first day, followed by diarrhœa, with hæmorrhage from the bowel and pneumonia; and death on the fifty-first day.

Case 33. Scarlatina. Convalescence on the twentieth day. Discharge from the hospital on the thirty-first day. Readmission, with fully developed enteric fever, on the fifty-sixth day; and death on the sixty-fourth day. Extensive ulceration of Peyer's patches.

Case 34. Scarlatina. Convalescence on the twelfth day. Relapse during sojourn in the hospital on the twenty-seventh day. Development of enteric fever on the thirty-second day; and death on the fiftieth day. Extensive ulceration of the solitary and agminated glands.

The author next gives a series of cases to illustrate the coexistence of the two diseases; and he concludes as follows:—"The intercurrence or sequence of scarlatina and enteric fever has been frequently noted, and always attributed to accidental coincidence. In my article on 'Enteric Fever' in Reynold's System of Medicine, I have expressed my convictions on this subject; and so strong were they in favor of the natural sequence of the diseases at the time that work was publishing, that I find I have in the proof-sheet called my contagious variety of enteric fever 'abdominal scarlatina.' I abandoned the term then because it seemed to me that the evidence which I had adduced to show the close relationship implied in it was insufficient to convince those whose opportunities of examining the question in detail were rare, and whose scholastic principles moreover would be shocked by such confusion of two diseases which are commonly considered to be specifically distinct. Now, however, that I am enabled to lay before the Society such full and complete evidence as is contained in the preceding observations, I will submit this term to the profession as a definite description of a disease which the practitioner will occasionally meet with. Nor will I allow this opportunity to slip me, but in the interests of truth will ask my fellow-laborers to go with me one step further, and to discard those transcendental ideas of enteric fever which make of it a disease *per se*, and to open their minds to receive what nature will then soon teach them—viz., that enteric fever and all its attendant phenomena may occasionally become a part of almost any other more general inflammatory condition, specific or simple."

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MEDICAL SOCIETY OF LONDON.

DR. ANDREW CLARK, President, in the Chair.

DR. ALFRED CARPENTER, of Croydon, related the following

CASES OF MUSCULAR ANÆSTHESIA.

The patients were two intelligent young ladies, the daughters of a city gentleman. The elder, M. H——, aged twenty-eight, of average good general health, every function of organic life being properly performed, all the natural functions of the body being in a healthy state, has been under the care of Dr. Carpenter for the last three years without material alteration, except a gradual but decided decrease in muscular power. Now she cannot rise from her chair without assistance, and the attempt, when made with aid, is clumsily and awkwardly performed; when she has gained the erect position, she staggers and is uncertain in her gait. The foot having been raised from the ground, there is an uncertainty as to where it will be placed, and it goes down with force, the heel first reaching the ground with a blow; as she cannot see the foot she cannot tell how far it is from the ground. She can only retain the upright position as long as she knows assistance is at hand. She has more difficulty in starting than in continuing her movements. If she stands, and is told to shut her eyes and move, or if she is in the dark, she cannot remain upright, but at once falls. There is, however, no material loss of power, for the individual muscles are still strong, and some force is required to bend her joints against her will, though the muscular power is much less than it was two years ago. She can still lift a considerable weight, and carry it if her eyes are fixed upon it. Her sight is good, the pupils act equally and freely, and there is no defect in the co-ordinating power of the optic muscles; there is no congestion of the conjunctiva, no amblyopia; there is no want of association in the muscles of expression, but there is a slight effort required in speaking. There is clumsiness of movement in the upper extremity, for she cannot button a button or put in a shirt-stud unless she looks at it. She cannot use her fingers with any precision when out of sight, but can execute drawings of considerable merit. She used to play on the piano very well, but for the last five years the requisite movements have become unsteady and imperfect. Her memory and hearing are quite right. She never suffers from pain of any kind; there are no starts or jerks or muscular tremors. Urinary organs and secretion normal. She has a lateral curvature of the spine, with some flattening of the ribs on the right side, in the middle of the dorsal region. The distortion, which is considerable behind, does not alter the shape of the chest in front. There is no cutaneous anæsthesia; the neighborhood of the joints is naturally sensitive. No numbness or formication is felt in the ordinary position; but if she sits up in bed with the knees drawn up before her, and a weight is kept on her knees, there is a feeling of numbness and deadened sensibility in the lower limbs. There is no marked reflex action, and no effect follows upon irritating the soles of the feet. She is able to localise sensations excited by the use of a pair of compasses in a normal manner, though the tactile sensibility of the lower extremities is, if anything, rather lessened. She is the second of eight children; her parents are healthy. At twelve her shoulder was said to have "grown out." Dr. Little ordered a supine position, with no mental work and daily muscular exercise. Iron supports were used for four years. The catamenia appeared at fourteen; at eighteen the supports and treatment were omitted. Her general health was good, but the unsteadiness of gait continued, and, as she developed into a young woman, increased. She was placed under the care of a leading physician six years ago, kept perfectly recumbent fourteen months, and treated with steel after that time without benefit. She came under Dr. Carpenter's care in 1868.

The condition of the younger sister, E. H——, aged twenty-six, is very similar, but the symptoms are not so advanced, neither is she so helpless as her sister was three years ago. She has followed similar plans. The irregular gait was observed when she was fourteen. She is now well developed, with a healthy rosy face. When she speaks it is with hesitation, and as if she had something in her mouth. She is able to thread her needle, but could not touch the tip of her nose if her eyes were shut. She walks more nervously than her sister, and her progression is more sudden, amounting to a half run.

Dr. Carpenter summed up the points upon which he asked the opinion of the Fellows under four heads—viz. (1), the cause of the development of the disease, (2) its nature, (3) its pathology, (4) its treatment. He referred the first point to some hereditary defect in nutrition of nerve matter—that the curvatures were coincident in point of time with development of disease as effects of a common cause, viz., degeneration of nerve matter; that they were not cases of locomotor ataxy at present. He combated the opinion of the translator of Trousseau's Clinical Medicine, that the pathognomonic sign of locomotor ataxy "was the peculiar deficiency in the power of co-ordinating voluntary movements," basing his objection on the absence of pain, of urinary disturbance, or ocular complication, as well as the fact implied by their youth and sex. The want of co-ordinating power being the most prominent symptom, he considered that in these cases the mischief was limited to the cord, and that there was no cerebellar complication, this idea being supported by the absence of subjective symptoms of brain disturbance and the superior intellectual power. He referred to the experiments of Claude Bernard, which prove that when the posterior roots of the spinal marrow are divided there is less co-ordinating power, that the harmony of movement is interfered with, and that Dr. Lockhart Clarke had distinctly made out that the posterior roots of the spinal cord were diseased in locomotor ataxy. The author concluded that in these cases, when the posterior column was diseased, the irritability of the muscles was found to be depressed, whilst their sensitiveness to pain was increased, and that electric currents excited violent pains. This was not the case with these patients; moderate galvanic currents were not distressing. The author also pointed out a typical case of locomotor ataxy, showing the different manner of progression as compared with the gait of these young ladies.

Dr. Wiltshire communicated a case of Œdema of the Lung after Small-pox. Miss P—— had small-pox somewhat severely in July last, and six months afterwards there was a considerable amount of anasarca of the lower extremities. There was also a slight cough. One day the dropsy of the legs suddenly disappeared almost completely, and about twelve hours after severe dyspnœa, violent cough, and expectoration of enormous quantities of glairy phlegm came on; the attack was so severe that the patient's life was in great jeopardy. In addition to all the signs of œdema of the lungs, the heart was feeble, and there was a little but by no means marked amount of albumen in the urine, which latter was not scanty. The exhibition of repeated doses of compound jalap powder, ammonia, ether, and digitalis, swathing in hot blankets, and the administration of brandy, gave the patient great relief, and under this treatment she rallied and made a good recovery.

Dr. Broadbent exhibited morbid specimens from a case of Apoplexy following Disease of the Kidney. One kidney had undergone granular degeneration, the other was not much changed. The heart was hypertrophied, and in the left hemisphere of the brain there was a large irregular cavity. The minute arteries were also hypertrophied. The woman had recovered from a former attack of hemiplegia, and had gone on pretty well until six months ago, when she suffered from general weakness, pain in the head, indigestion, &c. The pulse gave at first an impression of being feeble, but it was not so—it could be rolled under the finger, and when pressed on above continued to give a thrill to a finger placed below. The area of the heart's dulness was increased, the apex beat below and to the left of its usual situation. At the interventricular septum the first sound was reduplicated; at the apex the first sound only was heard. The right ventricle produced the first portion of the reduplicated sound; the left ventricle the second, owing to its meeting with a momentary resistance. The patient was pregnant, but did not know it till just before death, as she had menstruated regularly, and was unwell at the time of dying. She was seized with convulsions, coma, rigidity, and paralysis of the left limbs. Hæmorrhage came from the middle cerebral artery, and the cavity was situated external to the motor ganglion and motor tract. Dr. Broadbent thought the rigidity was due to a disturbance, not a destruction, of the motor ganglion and motor tracts. In answer to a question from Dr. Hughlings Jackson, he said that there was no pulmonary apoplexy. The tongue was bitten.

The President remarked that the case was well told, and the points open to discussion were—1. Was the thickening of the arteries due to poisoned blood? 2. Was the reduplicated sound due to the lagging action of the left ventricle? 3. Was rigidity due to disturbance, not destruction, of the motor ganglion?

Mr. Walter Coulson exhibited two patients with Syphilitic Ulcers cured by Skin-grafting. The first patient had suffered severely for three years and a half from the effects of malignant syphilis. There had been ulceration in the face, loss of a portion of the nose, and one extensive ulcer on the leg. After a variety of treatment without much success as far as the ulcer on the leg was concerned, the man was treated with bark and nitric acid, and the surface of the wound was granulating healthily. The limb was placed in a straight splint, and on October 3rd five pieces of skin were taken from the left arm about the size of a pin's head and transplanted. On October 26th five more pieces were grafted. On November 1st all the five last grafts had taken. On November 20th the ulcer had healed with the exception of a small spot. The second man had been suffering for two years and a half from a large syphilitic ulcer of the size of a cheese-plate, and had taken seventy grains of iodide of potassium three times a day. On October 3rd skin-grafting was done, as in the first case, with great success, the medicine being left off when the grafting commenced. This plan of treatment was recommended in the chronic stage of the disease, when the ulcer was covered with healthy granulations.

In the discussion which followed,

Mr. Jabez Hogg recommended bromide of potassium in syphilis.

The President called attention to this, as if it was as useful as iodide of potassium, it was important to know it, as the latter was not always well borne.

Dr. Broadbent, who had specially experimented in the bromides, said that their action was quite contrary to that of the iodides.

Mr. de Méric condemned the use of bromides when the iodide could be given. He thought that iodide of potassium was pushed too far, as it interfered with the sexual capacity of the patient. He had advised a little mercury combined with the iodide. He had also transplanted in some cases, but had failed.

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CLINICAL SOCIETY OF LONDON.

DR. W. W. GULL, F.R.S., President, in the Chair.

MR. CHRISTOPHER HEATH read a paper on a "Case of Wound of the Intestine during Ovariotomy, with recovery." The patient was under his care in the Hospital for Women in November, 1870, suffering from an ovarian tumour, which had been repeatedly tapped, and for the removal of which an attempted ovariotomy had been undertaken by another surgeon a year before. The patient was worn out with pain and sickness, and was anxious that another attempt at ovariotomy should be made. This was undertaken by Mr. Heath on Nov. 25th, 1870, when very extensive adhesions to the surrounding structures were found. On enlarging the abdominal incision with scissors in the ordinary way, an empty coil of small intestine, which was closely adherent to the wall, was divided in three-quarters of its circumference. The removal of the cyst was accomplished with considerable difficulty, the pedicle being tied and dropped. Mr. Heath then attached the bowel to the skin with silk sutures, forming an artificial anus, and closed the abdominal incision with wire sutures. The patient made a perfectly good recovery, fæces and flatus passing by the artifical opening on the second day, and solid motions per anum. The silk sutures were removed on the eleventh day, and the patient was moving about at the end of a month. Three applications of the actual cautery were made to the edges of the fistula to contract it, but it did not close; and the patient left the hospital in April, 1871. She was presented to the Society in a very comfortable and healthy condition, the use of a belt and air-pad satisfactorily retaining all fæcal matter, and the patient having regular stools.

Mr. Maunder congratulated Mr. Heath on the result of the operation, and thought that the particulars communicated showed the evil of using scissors, and that, in fact, scissors were now discarded by most surgeons.

Dr. Edis, who was present at the operation, very warmly supported Mr. Heath.

Mr. Scott remarked that, whether knife or scissors had been used, the intestine must have been cut, as it was closely adherent to the abdominal wall; and that the operation, in all its stages, was most skilfully performed.

Mr. De Morgan made some casual observations on the great variety of results that occur. He complimented Mr. Heath on the readiness displayed, and remarked that if the bowel had been sewn up, a very different result would in all probability have occurred.

Mr. Lawson observed that serous membranes which had inflamed and become thickened may be handled with impunity.

Mr. Heath remarked, with reference to the employment of scissors, that he invariably used his fingers for the purpose of exploration, and should use scissors again in the same way if similar circumstances occurred.

Dr. John Murray read a paper on "Thoracentesis in a case of Simple Pleuritic Effusion." The patient, a healthy-looking and muscular man, aged thirty-eight, was the subject of very extensive serous effusion into the left pleura, the heart being pushed completely over to the right side of the chest, and the left lung completely compressed. His respiration was generally about thirty per minute, his pulse 120, and the temperature 101°. Although the acute symptoms had subsided a month after the commencement of the attack, and a fortnight after the pleura had become completely filled, still there were no evidences of diminution of the fluid, notwithstanding many of the usual remedies having been tried. The ultimate and complete recovery of the left lung was being endangered, and the healthy one and the heart and large vessels interfered with, while the man's general health was suffering. Dr. Murray decided to employ thoracentesis. Accordingly Mr. Hulke performed the operation, and drew off thirty-five ounces of clear serous fluid by means of Nyrap's modification of Bowditch's respirator, the trocar being passed into the chest between the fifth and sixth ribs, where the digitations of the serratus magnus meet those of the obliquus externus muscles. The effect of the operation was to diminish in six hours the respiration, pulse, and temperature in a remarkable manner. Ten days after the operation all evidences of fluid were gone, and vesicular breathing had returned to a considerable extent over the upper two-thirds of the lung. He was discharged five weeks after the operation with evidences of a good recovery. The breathing and percussion resonance were still deficient over the left side, and friction was everywhere heard; but he suffered no inconvenience from the last symptom, and appeared in excellent general health. Dr. Murray thought that paracentesis thoracis was the proper treatment in this patient's case; and expressed his opinion, after quoting the experience of Dr. Bowditch of Boston and others, that in all cases of extensive and uncomplicated serous effusions, the operation should be performed at least immediately after the subsidence of the acute symptoms.

Dr. Moxon asked how far therapeutical efforts were made to induce absorption, and quoted a case of two months' study that had come under his care, in which the absorption of a very large amount of effusion occurred after the patient had been kept for some days on a very small quantity (less than ten ounces) of fluid.

Dr. Anstie thought that the plan advocated by the last speaker was both painful and cruel, and spoke very strongly in favor of the operation, because, as now performed, it was altogether harmless. The admission of air was avoided by the use of the aspirator; the diagnosis could, if necessary, be completed by using an exploring trocar; and the operation was a far better mode of procedure than "pottering" about with drugs, the action of which is at the best doubtful.

Dr. Playfair, as a visitor, stated that the operation had been very frequently performed at the Evelina Hospital, and with entire success; that, according to his own knowledge and belief, a caoutchouc tube conducted under water, and an ordinary trocar, were all-sufficient. He was in the habit, when operating on cases of empyema, of passing the drainage tube over the canula, and conducting the lower end of the former into a water bottle placed under the bed. The proceeding had answered particularly well.

Dr. Thorowgood doubted if the increased success that had lately occurred in these cases was due so much to improved mechanism as to a careful selection of cases.

Dr. Wilks asked for more exact particulars as to the value of the instrument exhibited. He believed that in all cases of ordinary serous effusions, the ordinary trocar and canula are sufficient, and took exception to Dr. Anstie's dicta, having seen very good results from small doses of mercury and iodide of potassium. He had hitherto believed that one of the avowed objects of instrumental interference was to assist the lung to expand.

Dr. Douglas Powell believed that there was no object in removing all the fluid, as the lung would not expand immediately, and that in empyema the entrance of air should be prevented.

Mr. De Morgan, as the first surgeon who had used drainage-tubes, queried if Dr. Playfair's plan would succeed in cases where the pus was fetid, inasmuch as it is well known in such circumstances the presence of air is a decided advantage.

Dr. Playfair could give no precise information on this particular point, but remarked incidentally that the first of his cases was one of third operation, and in another two operations had been performed. Both did well.

Dr. Edis quoted the authority of Dr. Bowditch in insisting that the operation should be performed early, and remarked that in the twenty-six cases of this physician twenty-one recovered, and all of those that died were cases in which the operation had been delay.

Dr. Baumler suggested that electrolysis might possibly be employed to assist the absorption of some fluids.

Dr. Barclay doubted if the new were better than the old plans of treatment. He had treated many cases therapeutically with success. He thought the percentage of recoveries quoted by Bowditch by no means assuring.

Dr. Duffin asserted that we wanted distinct and definite rules to guide us in determining exactly the kind of fluid that is present, as well as the quantity. A large increase of fluid may take place without the occurrence of any marked mechanical results, but when the velocity of the respiration and of the heart's action is increased it is manifestly a duty to interfere.

Dr. Ogle instanced a case in which death took place during the operation. His own experience of the use of drugs in such cases was favorable.

Dr. Duckworth had seen both lines of practice, and cordially subscribed to the plan of early instrumental interference.

Dr. Langdon Down believed in early tapping.

The President thought that the members had misapprehended the exact point of the discussion, and were going far afield; that Dr. Murray's case was one of simple catarrh of the pleura, and that the sole question to be settled was, should you wait or tap. According to his own experience, such cases usually did well if they were made hungry and kept so.

Dr. Murray, in reply, said that the question was limited to thoracentesis in simple and extensive pleuritic effusion—a plan of treatment which he strongly advocated immediately the acute stage had passed off. The sufferings of the patient are in every way diminished; the danger to the compressed lung is reduced as far as possible; and much time is saved in getting rid of the fluid, as compared with the old plan of treatment by diuretics and the like. Moreover, the operation may be repeated again and again, if need be. He had found no difficulty on one occasion in drawing off under water fluid from the pleura by means of a common trocar and piece of tubing. The advantages of the aspirator over such a plan lay chiefly in this, that a fine trocar could be employed, and more fluid could frequently be withdrawn. He thought, however, the instrument had no influence in expanding the lung.

Dr. Glover described a case of Uncomplicated Aphasia in a patient under his care at the Holloway and North Islington Dispensary. G. P——, aged sixty-three, an intelligent workman in a varnish and color manufactory, came under treatment in the beginning of September with a very imperfect power of expressing himself, a furred tongue, high-colored urine, and a weak pulse. The affection of the faculty of language was peculiar. Many words the patient could say quite well, but he was greatly embarrassed for want of the proper words. This appeared in his attempt to answer questions, and especially when asked to say what the names of particular objects were. Dr. Glover gave several curious illustrations. One day, on being shown a book and asked what it was, he said, "good," "house," "butter." On being asked to write the name, he said, "a good"; then he remembered the right word, and said "book." On a watch being shown to him, and being asked for the name of it, he said "*Tempus fugit,*" but could not say the proper word; but, on being asked to write it, he wrote "watch." He called a ring "a knife," and purse "bug," "book," "bug," "a pocket-book." One day, after naming the door and the fire, and being asked the name of the window, he was greatly puzzled, and said, "fire," "glass and sash;" he was then quite confused for a few minutes, and being asked to write it, he put "finaway," "fender," "windway," "windway," "shot," "lock." Ten days later, being asked the same question, he said "windle," and quickly "a window." He seems to know when he answers wrongly, and is sometimes impatient and sometimes amused at his errors and his embarrassment. There was no other symptom of cerebral diseases, no hemiplegia, and either none or but the slightest difference in the sensation of the two sides. He walked well, wrote fairly, shaved well, and protruded his tongue straight. Dr. Glover remarked that the case was especially interesting for being simple and uncomplicated with any lesion of intelligence or of motion, such as right hemiplegia. There was nothing to indicate any affection of the left hemisphere more than one of the right, as M. Broca's remarkable theory supposed, which localised the disease in some lesion of the third convolution of the left frontal lobe. Seven or eight months ago the patient had had a similar attack, which only incapacitated him from work one whole day, by reason of the way in which he put wrong names on the varnish cases. The present attack began about eight o'clock one morning in the watercloset. He seemed to lose himself. He foamed at the mouth and fell down, but not quite unconsciously, for he tried to save himself. He lost his speech, but he spoke a little, cried a little, and seemed inclined to fret. He walked home with the help of a young man, who gave these particulars of the attack, and who thought that at the time the face was drawn to the left side. There was no drawing of the mouth since Dr. Glover attended him, and he saw him the day after the attack. Occasionally the heart and pulse were irregular. At other times, and generally, they were regular but weak. There was no abnormal sound. Mr. Carter had kindly examined the eyes, but the existence of lenticular opacity made it impossible to get any help in diagnosis from the state of the fundus. The patient's previous health had been good, excepting yellow fever in 1838 or 1864. He never had rheumatism or syphilis. The treatment at first was chiefly expectant. Latterly, ammonia and a little wine and beef have been its principle features. The aphasia had become rather less, though still persistent. Dr. Glover thought the symptoms might be attributed to either softening, or embolism, or slight extravasation, affecting a very limited portion of the brain, and inclined rather to extravasation, from the persistence of the aphasia, and the nature of the attack. The patient was present, and was examined by various members of the Society.

Dr. Bäumler said that he had several times seen temporary coma produced by lead.

Dr. Glover, in answer to Dr. Bäumler, remarked that he had examined the patient and his history carefully, and found no evidence of lead-poisoning.

Dr. Hughlings Jackson thought that, since the patient made mistakes in words and could not express himself correctly in writing, the author was justified by usage in calling his very interesting case one of aphasia. Dr. Jackson suggested that this term should be restricted to cases of more or less *loss* of speech. A term was wanted for the other condition, disorder of speech—e. g., mistakes in words. He believed these two conditions were fundamentally opposite; and that the mental states of the patient were different. For example, in cases of loss of speech the patient readily understands what is said; whereas in cases where mistakes in words is the chief phenomenon, it frequently occurs that the patient does not readily understand what is said to him. He supposed that disorder of speech was more frequently the result of plugging of vessels, in young people especially, than of other pathological lesions, such as clots; but would not speak so confidently on this point as he had formerly done. The absence of hemiplegia, in cases of affection of speech, was very uncommon.

Dr. Anstie said that he had seen a case in which this confusion of the name of one object with another occurred during an attack of autumnal diarrhœa.

Dr. Moxon thought that aphasia meant that state in which words are forgotten, and cannot be used when repeated to them.

Dr. Habershon narrated two cases of Disease of the Heart. The first instance was that of a young man aged thirty, who had been accustomed to great muscular exertion, and in whom the aortic valves were rendered imperfect by continued strain. Hypertrophy and dilatation of the left ventricle ensued; and the physical signs of aortic obstruction and regurgitation were accompanied by urgent dyspnœa and by paroxysms of angina pectoris. The mitral valve at length became so stretched that it ceased to act as a valve, and the indications of pulmonary and abdominal congestion became apparent. At this time a triple sound was audible at the apex of the heart, and continued for several weeks—till, in fact, the right ventricle became accustomed to the additional strain thrown upon it, and beat in unison with the left ventricle. After a time, a systolic bruit at the apex replaced the triple sound. The triple sound was explained by the want of synchronous action between the two ventricles. It has been noticed in rupture of the mitral valve, and also in cases of contraction of that valve; and in this instance it occurred for a short period in connexion with an overstrained mitral valve. The patient became slowly exhausted by the increased embarrassment of the heart and by dropsy.—The second instance was that of a child aged eleven, in whom severe cardiac disease, affecting the mitral valve, followed an attack of rheumatism. Three years previously he had the first attack, and had been ill for a fortnight before his admission into Guy's Hospital, on February 22nd, 1871. There was pain in the limbs; and, in the plane of the mitral, a loud systolic bruit, and a less distinct presystolic one, were audible. He improved in health till about the middle of April (12th), when the presystolic bruit became more distinct; and this indication of fresh disease was shortly followed by severe brain symptoms; vomiting and delirium, with hemiplegia on the left side, suddenly supervened; symptoms of great prostration followed, with convulsive movements of the right side; and, when consciousness was regained, he cried out and complained of pain in the head and in the spine. On the third day he began to rally, and in ten days the paralysis began to lessen, but the hand and arm recovered before the foot and leg. The presystolic bruit also ceased, and the systolic mitral bruit alone remained audible. He left the hospital relieved on June 24th. The presystolic bruit was probably due to deposit on the mitral and consequent obstruction, and increased deposit led to augmentation of the sound. The cerebral symptoms were traced to embolism; and soon after the onset of these symptoms the presystolic bruit was diminished in intensity, as if the mitral, relieved of a portion of fibrinous deposit, acted more freely. The retrocession of the hemiplegic symptoms was in the inverse order from ordinary hemiplegia, from apoplexy—namely, that the arm recovered before the leg and the hand, and the feet before the shoulders and the hips. Very little medical treatment was permitted in this case, and it illustrated in a remarkable manner the natural subsidence of the symptoms of disease as the circulation of the brain accomodated itself to the temporary obstruction.

Dr. Baumler, in discussing the first case, quoted a similar instance that had come under his observation. Digitalis and iron produced great benefit; but in his absence opium was prescribed. Anasarca and suppression of urine set in, and mortification and death speedily ensued.

The Chairman remarked that the diagnostic interest of the case appeared to turn chiefly on the triple sound described, and the thin condition of the mitral valve.

Dr. C. J. B. Williams had often met with similar cases, and believed that the first sound depended upon the tightening of the whole mass of heart, and that if all parts do not tighten at the same time, an abnormal condition of things is produced; or it might be said that the two ventricles alternate in their action. He supposed that aggravation of the disease was due to the superaddition of mitral regurgitation to aortic disease, thus deranging the whole circulation. He believed in the beneficial action of mercury on such cases, particularly when combined with digitalis, as promoting the action of the liver and kidneys; and was convinced that opium did more harm than good, because sleep cannot be obtained on account of the intense orthopnœa.

Dr. Broadbent remarked that the double stethoscope will, as a general rule, demonstrate the condition of things described by Dr. Habershon, and prove that a want of synchronism between the two ventricles may produce a reduplication of the first sound; that in cases of renal mischief the right has a start of the left ventricle, and in cases of lung disease, *vice versâ*. His belief in the efficacy of mercury in such cases was positive and defined.

Dr. Langdon Down quoted a case of reduplication due to want of synchronism, which soon passed away under the influence of digitalis.

Mr. Lawson wished to know in what position the patient was placed whilst paracentesis abdominis was being performed, and Dr. Broadbent asked for a definition of "coarse kidneys."

Dr. Habershon, in reply, agreed with Dr. Williams as to the bad effects produced by opium, and believed that mercury combined with squill and digitalis was particularly useful in these cases. The chief point of interest in the case was undoubtedly the triple sound, which was heard most distinctly immediately to the right of the nipple, and towards the right ventricle. He was unable to state the position of the patient during the operation.

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OBSTETRICAL SOCIETY OF LONDON.

Dr. Braxton Hicks, F.R.S., President, in the Chair.

Dr. Snow Beck exhibited an imperfectly involuted Uterus, and gave a minute account of its microscopical characters; from which he concluded —(1) That the essential condition of the organ consisted in the elements of the different tissues retaining a portion of the natural enlargement consequent upon impregnation. But this enlargement was due more to the increased size and amount of the soft tissue present in the walls of the uterus as well as at the internal surface, than to the increased size of the contractile fibre-cells. (2) That although the bloodvessels were large and loaded with fluid blood, yet there was not any evidence to show that any morbid process, similar to inflammation, had at any time been present. (3) That the whole of the bloodvessels to the minute capillary network at the inner surface formed one continuous system, though the character of the distribution changed towards the inner surface. And, considering there is no division in the uterine walls to justify the description of an internal mucous membrane, this distribution appears to offer a strong argument against the idea that the internal surface can be the seat of inflammation independent of the other portions. (4) That the pulpy condition of the tissue at the inner surface, with the loaded state of the bloodvessels throughout, appear to afford a probable explanation of the frequent hæmorrhages which attend similar enlargements. (5) That with regard to any treatment for the removal of this condition, it would appear that this should include topical applications to the whole of the uterine cavities.

Dr. Barnes read an abstract of a memoir on "Osteomalacia," by Dr. Casati, of Milan. This disease is remarkably frequent in some of the rural districts surrounding Milan. During the eight years that the author had acted as assistant to the late Professor Lazzati, sixty-two cases of clearly marked osteomalacia were admitted into the Lying-in Hospital. Almost all the sixty-two women came from that part of Milan known as the Valley of Olona, the same district which also supplies the greatest proportion of cases of pellagra and of petechial typhus to the Milan hospitals. They all came from wretched villages, where food was scarce, consisting for the most part of maize or rice, often musty. Their houses were ill-ventilated, damp, and built upon clay. The drinking water was found wanting in saline matter. Twenty-seven of the women were delivered normally after easy labour, four had difficult labour, while thirty-one were delivered artificially, but only two required the Cæsarean section. For the medical treatment Casati recommends the use of the biphosphate of lime or of "bone powder," and cod-liver oil, and the substitution of Liebig's bread for bread made of rice and maize.

Dr. Squire did not agree with the author that the pulmonary and bronchial congestions afforded any argument against the use of chloroform, since these depended on the mode of death, and in severe labours the use of chloroform tends to diminish the stress upon both respiration and circulation.

Dr. Rasch read a paper on a "Novel Method of using the Uterine Sound for Redressing a Flexed Uterus." The point upon which he particularly laid stress was that the part of the instrument inside the uterus should be kept steady in its place, and thus avoid the irritation which resulted from making a large semicircle of motion in the uterine cavity, according to the usual method. A sound introduced into the retroflexed womb with the point downward and backward, should be first used in that position as a lever to lift up the organ as far as possible. Then, instead of twisting the handle round and making the intra-uterine part swing round the ideal prolongation of the stem, the movement should be reversed, so that the part in the uterus and its ideal prolongation are made the centre of motion round which the handle and stem sweep in a large circle. The movement is well illustrated by grasping the curved part of the sound in one's hand, and swinging the handle round as it will go.

Dr. Phillips fully appreciated the value of the method described by Dr. Rasch. He would, however, be glad to know the comparative frequency with which it was found necessary to use the sound for redressing the flexed uterus. Doubtless it could not be dispensed with in some cases, but he always attempted to restore a retroflexed uterus without the use of any instrument, and he believed that in a large proportion of cases this could be effected.

Dr. Gustavus Murray could hardly admit that the method described by Dr. Rasch was new in principle. He had employed it for some years. In severe cases of flexion he was in the habit of introducing the sound in an easy manner, by passing it first in the opposite direction to the displacement until it reached the flexed point, and then, secondly, by turning the curve of the instrument in the course the fallen body of the uterus had taken.

Dr. Aveling and Dr. Edis also said that they had practised the method. Dr. Edis said that Spiegelberg had recently called special attention to the danger resulting from injudicious use of the sound, and any method which lessened the risk from its use was worth considering.

Dr. Conrad, of Pesth, read a paper on "Prolapse of the Female Genitalia," in which he maintained that the prolapse of the uterus is for the most part a secondary affection, a primary prolapse being very rare. Prolapse of the vagina is the most important part in any descent of the female genitals, and uterine prolapse is but a sequel of this, the prolapsed vaginal walls pulling down the uterus. Of the different forms of vaginal prolapse, that of the anterior wall is the most common, though frequently associated with prolapse of the posterior wall. A descent of the latter by itself is, however, rare. Should there be a considerable elongation of the cervix, we can certainly conclude that the vaginal prolapse was primary. The elongation of the vaginal portion of the cervix is of no diagnostic value, and is simply the result of mechanical irritation. Dr. Conrad believes it may be justly maintained that elongation of the supra-vaginal portion of the cervix and hypertrophy of the infra-vaginal portion are but secondary affections. The vagina alone is sometimes prolapsed without any descent of the uterus. The author then discussed the most important predisposing causes of prolapse—gestation, parturition, senile atrophy, &c. Prolapse in young girls occur suddenly from a sudden shock through *contre-coup*. The replacement of a complete prolapsus of long standing should always be preceded by an emptying of the bladder and the rectum. The palliative treatment is best conducted by pessaries, of which the best is Meyer's ring, or Hodge's modification of it. For the radical cure, Dr. Conrad prefers Professor Spiegelberg's operation to any other. It consists of a combination of three operations. The vaginal portion of the cervix uteri, if greatly hypertrophied, is first removed by the galvanic wire. If the posterior wall of the vagina has become prolapsed with the uterus, he performs Dieffenbach's operation, by removing a triangular piece of the mucous membrane of the posterior wall, having its apex to the os uteri, and for its base either the pared surfaces of the rent perineum, when this has been ruptured, to form a new perineum, or where this has not happened, the labia minora, so as to contract the orifice of the vagina, and aid in supporting the anterior vaginal wall. Spiegelberg then proceeds to unite the upper portion of the posterior vaginal wall with the inferior anterior, according to Simon's operation.

Dr. Edis thought the affection, on account of its great frequency, was too little studied. He had seen several cases where a pessary had been introduced, with the intention of keeping up an elongated cervix.

Dr. Philips agreed with the author of the paper that certain cases of prolapsus uteri were doubtless secondary to vaginal prolapse, cases in which the uterus was small, and which had been called instances of *passive* prolapse; but he believed that this class was a much smaller one than that which included those cases where an increased weight of the uterus itself was the primary cause of its descent. This was associated with the relaxation of the surrounding areolar tissue, and the heavy uterus was then sufficient to overcome the support which the vagina in the healthy state gave it by means of the strong bands of so-called areolar tissue inserted into its walls. The arguments brought forward by M. Huguier seemed to him conclusive against the view advocated by Dr. Conrad, that elongation of the supra-vaginal portion of the cervix was a secondary affection dependent on the traction made by the prolapsed vaginal walls. He doubted whether any shock would produce a displacement of the uterus in young women, if the organ was not increased in weight and there existed no impairment of the surrounding structures.

Dr. Heywood Smith said he agreed with what had just been said, that the chief cause of prolapsus uteri was an increased weight of the organ, producing relaxation of its supports. Another cause, however, was not generally recognised—viz., that as age advanced, the lumbo-sacral curve becoming more or less obliterated, the plane of the pelvis thereby becoming more horizontal, the natural support that the normal position of the pelvis, together with the abdominal walls, gave to the pelvic viscera was removed, and they tended to prolapse from gravitation. He suggested that the word "procidentia" should be held to signify the falling or tendency to fall, and that "prolapsus," being the past principle, and signifying the complete act, should be used only to indicate cases where the uterus was extruded from the vulva.

Dr. Routh believed that prolapsus uteri without elongation of the cervix was very rare. His own experience led him to agree with the conclusions arrived at by Dr. Conrad. Looking back at the operative measures which had been adopted for the relief of prolapsus, there were three chief methods which had been practised. First, the perineal operation, by means of which some cases were radically cured; in others, however, after some months the uterus dilated the external opening, and appeared external to the vulva. Secondly came Emmett's operation, or a modification of it as first practised by Dr. Rogers. This consisted in removing a very large triangular piece of mucous membrane (the apex being towards the cervix uteri) from the anterior wall, sometimes also from the posterior, wall of the vagina, and bringing the edges together by sutures. This operation also succeeded in some cases, though not in all. Thirdly came the plan of removing a portion of the cervix by the écraseur or actual cautery. This set up absorptive action in the uterus, and the elongation in many cases gradually but entirely disappeared. Dr. Routh thought that a cure, to be certain and radical, should comprise the three operations conjointly, or at any rate the last and one of the other two. He believed that any cause which produced a friction of the vagina on the cervix would produce allongement of the cervix.

Dr. Barnes said that for some years we had all been pursuing a tentative course to find out the best mode of treating these affections. As to the mode of production of hypertrophy, he was scarcely prepared to agree with Spiegelberg and Dr. Conrad. The traction of the vagina might come in as a factor in the course of the disease, but the initiatory stage was, he believed, congestion and increased weight of the uterus. These cases were rare in women who had not borne children; they almost all began after childbearing. The passage of the child through the cervix was a violent process: the cervix was forcibly stretched open; the mucous membrane was carried down before the head; the tissues of the cervix were bruised; small vessels were torn. Then, from getting about too soon and other causes, imperfect involution resulted; the lower part of the uterus especially was increased in weight and bulk, whilst the surrounding cellular tissue, having been greatly stretched and weakened, was less able to support the uterus. Hence continued congestion and a perverted nutrition of the cervix. Small polypi on the edge of the os frequently complicated hypertrophy. Their structure was identical with that of the cervix from which they sprang. Dr. Barnes could not help thinking that simple prolapsus was far more frequent than Dr. Routh's observations would indicate. He would, in conclusion, like to know how far the experience of others agreed with his own as to the usual extent of elongation being exactly five inches. He had found the cases in which this length was exceeded very rare.

Mr. Spencer Wells differed from Dr. Barnes in the opinion that restoration of the perineum was beginning at the wrong end in the treatment of prolapsus of uterus and vagina. It is quité true that prolapse of the anterior wall of the vagina is the first step in the progress downwards. But a sound perineum is the chief opponent to this prolapse. It would be absurd to expect the perineal operation to cure an elongated cervix, but in cases of ordinary prolapse he could say that the perineal operation often effected permanent cure. He had never seen an elongated cervix grow again after the vaginal portion had been removed.

The President said that when the uterus, from whatever cause, descended, it, by its pressure on the vagina, acted like a foreign body, and set up reflex irritation and tenesmus, which, acting constantly, coupled with defecation and the pressure of the bladder, would tend to extrude and elongate the cervix. It would clearly be better, before attempting any plastic operations, to endeavor to gain shortening of the uterine supports, either by pessaries or recumbency.

Editorial.

THE CAUSE OF VIRULENCE.

M. Chauveau having rendered it highly probable—as we endeavored to show last month—that the virus of any fluid capable of active propagation by inoculation is essentially a quality or property attached to and characteristic of the morphological elements found in such fluids, and not of the plasma or serum in which these elements float, it remains to be ascertained to which of them it really belongs. In every fluid of this nature—such, for instance, as the purulent discharge of glanders—numerous corpuscles are visible under the microscope, some consisting of perfect nucleated cells, others being nuclei, and others, again, the débris of disintegrated tissue and cells. That some of these are more active than others there can be little doubt, and M. Chauveau made a series of experiments to determine which amongst them was most active. The task is no easy one; for not only does it require a good instrument and a good observer, but it is necessary that the characters and changes of the formed elements which enter into the composition of virulent humours should be followed concurrently with the study of the genesis and of the development of the lesions in the midst of which these humours are formed. By this means many difficulties will be avoided. To take only one. It is obvious that a careful knowledge of the development of the formed elements will entirely prevent any confusion arising between accidental and constant elements; whilst, at the same time, by demonstrating the absence of certain formed elements from fluids of known and proved activity and virulence, positive statements can be made excluding these from any necessary connexion with or participation in the virulent effects. It may thus be shown that none of the proto-organisms, such as bacteria, vibriones, &c., are necessarily present in virulent fluids, or have any real connexion with their activity. In the vaccine lymph, for example, the best and most practised observers, examining with high powers, have not been able to discover a single specimen of any such proto-organism ferments, as M. Chauveau terms them, provided the examinations have been made speedily after removal from the body. So also in variola; for though, as Cose and Feltz have shown, bacteria may be present both in the pustules and in the blood, and rabbits when inoculated with such fluid may sicken and die, yet, according to M. Chauveau, the bacteria are only accidental elements which are by no means constantly present, and the disease these authors have propagated from rabbit to rabbit, he maintains, is not Small-pox at all, man alone affording a suitable soil for the *complete* development of variola; even the horse and cow do not show the general manifestations of the disease which are so characteristic of its presence in him, and the rabbit is totally insusceptible of it. That the disease produced in the rabbit by inoculation is not true small-pox is shown by the fact that it will not reproduce variola by inoculation of the blood in the horse and cow, though this can be easily accomplished with the variolous virus of man. M. Chauveau lays down the important maxim that, in opposition to what occurs in septic or septicoidal infections, virulent active principles are *not* to be looked for in the blood; and, amongst the latter, he has especially examined the fluids of glanders, the rot, the rinderpest poison, and vaccine and variolous poisons.

Proceeding further in his inquiry he shows that no other formed element is necessarily present in a virulent fluid than the fine molecular granulations. All other corpuscles may be entirely absent without in any way diminishing or interfering with the activity of the fluid. He at the same time acknowledges that the cellular elements may be the carriers of the virus; but this is simply due to the circumstance that they are thoroughly infiltrated with the molecular or almost dust-like elementary particles which, either separate or aggregated into a mass, constitute the virus itself. Whether all or only certain of these molecules are endowed with the virulent properties he confesses himself unable to determine. The spores of Hallier he regards as hypothetical. To arrive at any satisfactory conclusion, the culture of the micrococci should be conducted in the medium in which they are found, and not in an artificial one; whilst, as he strongly argues, it is impossible to produce the rot with Pleospora herbarum, vaccinia with the Torula rufescens, or the epizootic pneumonia with Muco mucedo, in whatever stage of their development, or in whatever manner these fungi are introduced into the system. In fine, putting aside those contagious diseases which, like trichiniasis or the silkworm disease, depend upon the development of certain animal or vegetable parasites, and those septic or septicoidal diseases which result from the multiplication of "proto-organism ferments," there still remain certain affections which may be included under the head of true virulent diseases, in which the virus is strictly limited to the granular protoplasm of the neoformations, and which are not propagated in the manner of a parasitic ferment, though their course may be complicated by the presence of the septic or septicoidal class of diseases.

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THE USE OF SEDATIVES.

Dr. Maudsley has recently called the attention of alienists to the very important question of the use and abuse of sedatives in the treatment of insanity. He suggests, rather than affirms, that the temporary peace obtained by the action of sedatives may be deceptive in its character, and hurtful in its eventual influence. He thinks that natural processes tending to the removal of noxious matters from the system may be arrested or checked; and that the brain-cells may be placed in a sort of chemical restraint, which, like bodily restraint, may increase the malady while it suppresses its manifestations. He asks for some experiments upon the treatment of insanity without sedatives; and relates a few cases of his own, which have done well under such management. He refers to the large use of opium that at one time prevailed in asylums, and mentions the researches of Dr. Clouston with regard to its action, which led to the conclusion that the drug "soon lost its effect; all the patients lost weight while taking it; their average temperature fell; the pulse was lowered; and in all cases it interfered with the proper nutrition of the body, and pushed it one step farther down hill in the direction of death." There can be no better illustration of the influence of routine upon treatment than the fact that a medication, which showed such results to a careful and systematic observer, was for many years accepted as the practice proper to be pursued.

The doubts raised by Dr. Maudsley have, it should be observed, a far wider range of application than to lunatics alone. The use of sedatives in general medicine is open to a great variety of questions, not the least important of which is the danger of causing patients to have habitual recourse to them after convalescence is established. On the very border-ground between alienist and general practice, in the treatment of delirium tremens, the present generation has heard opium extolled as a specific and denounced as a poison; and it is certain, we think, that the current of opinion among practical men is now distinctly adverse to its use, regarded merely as an agent for producing sleep. In the presence of pain, however, the verdict of clinical experienee would be emphatically in its favor, more especially in the treatment of ordinary inflammation attended by pain, the subjugation of which will usually be a necessary antecedent of favorable changes in the nutrition of the affected part. In specific inflammation, on the other hand —as in gout and rheumatism,—its usefulness is more limited, and is at most such as to place it only in the list of adjuvants to more essential treatment. Even on these points, however, our knowledge is almost entirely empirical, and we can scarcely claim to possess any certain information about the mode of action of a medicine that is given every day. If this be true of opium, how much more is it true of drugs more recently introduced or less freely prescribed. A few years ago there was a marked inclination on the part of young physicians to enter upon certain definite branches of therapeutical research; and this spirit, if it be not extinct, could find no better outlet than in investigating the action of the different agents that we roughly class together as sedatives, and in telling us something definite about their influence upon bodily functions and organs. We do not mean by this that a committee should follow the example of Dr. Bennett and his coadjutors, who fancied they could arrive at conclusions about the action of mercury on man by administering it to dogs; but we would suggest a real inquiry, founded on the legitimate (according to our present lights) use of sedatives in practice, and on a comparison of the results produced in sick men, under like conditions, by giving or withholding them. In such an investigation it would be in the power of alienists to render most important help, and to make good the claim they now seem anxious to assert—that of being fellow-workers with other practitioners in the advancement of the healing art.

DACTYLITIS SYPHILITICA.

It cannot be doubted that we are not sufficiently alive to the frequency and importance of the remote effects of syphilis upon the body. This conclusion is the inevitable consequence of a careful survey of the facts which pathologists have of late years disclosed—in regard, for instance, to lesions of the nervous system,—and also of the results of therapeutical observation. Whilst, on the one hand, we are discovering new syphilitic lesions, we are likewise recognising more completely the modifying influence of the syphilitic taint upon various diseases accompanied by structural changes. It is specially with the former category that we are now concerned. Our object is to direct the attention of English practitioners to a tertiary syphilitic lesion in the deeper structures of the fingers and toes, accompanied by peculiar deformities, which is practically unknown in England, though it has received special attention elsewhere. Its literature is scanty in details, and, as far as accurate information goes, of very recent origin. The best account is given by an able and rising dermatologist of New York, Dr. W. R. Taylor, in a recent number of the *American Journal of Syphilography and Dermatology*. The disease is called *Dactylitis syphilitica*. It is true that in 1859 Chassaignac called attention to it, but his description was very meagre; and subsequently Nélaton, Professor Lücke, M. Archambault, Risel, Berg, and Erlack, recorded solitary cases. But the disease has never been so carefully studied as by Dr. Taylor, who sums up all we know about it; and we believe we shall be doing good service to practitioners if we give a general sketch of the complaint. The disease is a chronic specific inflammation, involving one, and most frequently all, of the phalanges, without implication of the nails. There are two forms, according to Dr. Taylor: the one, in which the subcutaneous tissue, as well as the fibrous structures of the articulations and the phalanges, are involved; and the other, in which the morbid process begins in the periosteum and bones, and implicates the joints secondarily, and may or may not be accompanied by gummy deposit in the subcutaneous tissue. There is no absolute line of demarcation between the two. The invasion of the structure by gummatous growth is the essential disease. When the connective tissue is the seat of disease, the deposit is more marked in the dorsal than in the plantar and palmar surfaces; but it does not show the usual tendency to disintegration. The integuments covering the growth become violaceous in color, and are tense, resistant, and painless. One phalanx or all may be involved—generally one. After a while the fibrous coverings of the bone and the joints become affected—the ligaments, capsules, and cartilage probably. Deformity results sooner or later. In the second form of the disease periostitis may be the starting lesion, or the gummy deposit may take place in the cancellous tissue about the medulla. The enlargement of the fingers and subsequent deformity are very marked; the parts swell, and are distended into a hard, firm enlargement. The gummy material is not found, as a rule, subcutaneously in this form. The bone is absorbed, the shaft lessened, the fingers shortened, and false joints formed, but without suppuration—we have, in fact, what Virchow termed "dry caries" of syphilis. The fibrous textures of the joint may, as in the first variety, be implicated, but not necessarily.

The diagnosis of this affection has to be made from gout, rheumatic arthritis, whitlow, periostitis, and enchondroma. But dactylitis has concomitant evidences of the syphilitic taint; it has no acute or painful symptoms, as have gout, whitlow, and rheumatism. It is painless, and runs a chronic course; it involves specially the dorsal surface; often only one joint. If recognised early, the prognosis is favorable, since the adoption of the treatment for late syphilis is very successful.

Our main reason for directing attention to the disease is because Dr. Taylor's excellent paper has attracted a great deal of notice in every other country save our own, where this dactylitis seems to have been overlooked.

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THE RATIONAL TREATMENT OF MEASLES.

The last part of the *Zeitschrift für Parasitenkunde* contains a paper by Dr. Ottmar Hofmann, who is a staunch supporter of Hallier's views respecting the origin of many contagious and infectious diseases from fungi, "On the Rational Treatment of the Acute Exanthemata, and especially of Measles and Scarlet Fever." Long experience, he maintains, has shown that we possess in the numerous drugs that have been at one time or another prescribed no cure for these diseases, or, as he puts it, no means of killing the fungi to the presence of which the symptoms of the special affection are due; unless, perhaps, we except quinine, the good effects of which in typhus have been shown by the excellent researches of Binz, and corroborated by many observers. Binz himself admits, however, that, to produce any destructive action on the spores of these fungi contained in the blood, quinine must be given in such large doses as to render it a remedy which it is not advisable to prescribe in the case of children, especially when they are in such a condition of somnolence as to render its introduction into the system difficult, if not impossible. Seeing, then, that the spores cannot be destroyed in the blood, the most rational plan of treatment is obviously to effect their elimination from the system; and this he maintains can be effected by the skin. He accordingly adopts a hydropathic system of treatment, consisting of swathing the patient with damp cloths, and then covering him with a linen or woollen wrapper in the usual manner, by which means free perspiration is induced, the temperature of the cloths and the frequency of their application being of course regulated by the degree of the fever present. Were his views correct, it is obvious that the perspiration should contain micrococcus spores. To determine this point he collected the fluid discharged by the skin in the case of a little child of four years of age suffering from a severe attack of measles and treated on this hydropathic plan. The fluids were at once transmitted by Dr. Hofmann to Professor Hallier, who reported that they contained large quantities of micrococcus, and that he was about to undertake a series of researches on its culture to determine whether it was the true fungus of measles or not.

We are by no means disposed to adopt Prof. Hallier's views, and should be glad if these statements were criticised by other observers.

Medical Annotations.

"Ne quid nimis."

CONTAGIOUS IMPETIGO.

We understand that a form of skin disease, the exact cause and nature of which were at first somewhat disputed, has lately appeared among the men belonging to a battalion of Foot Guards stationed at Dublin. As many as 100 soldiers are stated to have exhibited manifestations of the disease, which is local in its nature, and unattended by any constitutional disturbance. The affection consists in the appearance of numerous small vesico-pustules about the scalp and face, which soon dry up and scab over. We believe the medical officers consider that the disease closely resembles, if it be not identical with, that form of impetigo which Dr. Tilbury Fox has described as inoculable and possessed of contagious properties, and occasionally capable of assuming an epidemic character. Impetigo contagiosa is most commonly witnessed in children. It tends to run a definite course, and exhibits a tolerably uniform character, although it differs in severity and features at different times. Professor Kōhn of Vienna, Dr. Taylor of New York, and others have recently confirmed Dr. Fox's views.

The outbreak in the Foot Guards originated among the children while the battalion was stationed at the Curragh, and the disease subsequently spread to the soldiers. In one or two cases its auto-inoculability was evidenced by the fact that some spots appeared on the skin of the arm from the head having rested upon this part during sleep. The old term "porrigo" probably comprehended this affection; but this term was not very definitively used, for it seems to have included several affections exhibiting scabs upon the skin, and possessing, or supposed to possess, contagious properties. The diseased manifestations being relatively insignificant, and hidden by the hair, their presence would easily escape detection; but the medical officers having very properly acted on the assumption that the disease is contagious, we may confidently expect that its progress will be arrested, and that the cure of the affected individuals will rapidly take place.

THE PERIPHERAL DISTRIBUTION OF NON-MEDULLATED NERVE-FIBRES.

A paper appears with this title in the October number of the *Quarterly Journal of Microscopical Science*, by Dr. E. Klein, in which he gives the results of his observations, accompanied by two beautifully-drawn plates, on the mode of termination of the nerves in the cornea and the membrana nictitans of the frog, the ciliated canal in the tail of the rabbit, and in the mesentery of the same animal. He gives the following excellent method for obtaining good preparations of the cornea. He carefully cuts it out from the just killed animal, and places it, with the convex surface upwards, in a watchglass, with a pure ½ per cent. solution of chloride of gold. In three-quarters of an hour he transfers it to a vessel containing distilled water, where it remains exposed to light for from six to ten hours. Its color has then become of a steel grey. It is then placed in a small wide-mouthed flask containing a little filtered and nearly saturated solution of tartaric acid. The cornea now assumes a greyish-violet color, and the flask containing it is immersed in a capsule containing water at 120° or 130° Fahr. In a few minutes the color darkens considerably to an intense violet-red, and ultimately to a dirty dark-brown red color. It is then washed for two hours or more in common or distilled water, and is now fit for the preparation of specimens, and laminæ may be torn off with forceps, commencing at the scleral border. The specimens thus obtained require a good light for their examination. Dr. Klein proceeds to describe the distribution of the coarser, and then the finer, nerve fasciculi, and shows that having reached the base of the epithelial cells on the anterior surface of the cornea, and forming there a deep intra-epithelial network, they run up between the cells, and form plexuses between the most superficial layers, being all but exposed on the surface, from which they are separated by one or two only of the thin laminated cells, and forming here a superficial intra-epithelial plexus. He does not exhibit in his drawings any free extremities, and regards Cohnheim's terminal knobs as being only intercalated swellings in the course of the nerves.

THE EPIDEMIC OF DENGUE AT ADEN.

The garrison at Aden has been visited by an epidemic of dengue—a disease vulgarly known as the break-bone fever. It would seem that the earliest attacks occurred about the end of June, but the nature of the disease was not at first determined. It spread with much rapidity, and was believed to be eminently contagious. More than 700 cases occurred, out of a strength of upwards of 900 individuals belonging to the Rifle Brigade and Royal Artillery forming the garrison of Aden. The disease attacked officers, men, women, and children alike, but it did not prove fatal to any.

It is believed that the disease was imported from Zanzibar, at which place, and along the neighboring coast, it had been prevailing for some time previously. Its introduction into Aden is attributed to the free communication that existed between that station and the infected localities by means of native trading vessels. Dengue somewhat resembles rheumatic fever, or that variety of scarlet fever, attended with marked arthritic symptoms, sometimes witnessed in this country. Its invasion is sudden, associated with fever and rheumatic pains, and followed by an exanthem, which seems to vary much in its character, being sometimes a diffused redness like that of scarlatina, sometimes papular, and occasionally even vesicular. After a remission of the symptoms, a relapse often occurs about seven days from the date of the first attack. Pains in the joints and muscles are often complained of for some time after apparent convalescence. Those pains partake very much of the neuralgic character; they are attended with the pricking, burning sensations of a disordered innervation.

DIMENSIONS OF THE RED BLOOD-CORPUSCLES.

Dr. **Manassein** contributes a paper on this subject to the *Centralblatt*, in which he states that there can be no question that any influence producing a great change in any function of the organism must necessarily modify the physical and chemical relations of those corporeal elements on which the activity of the function is dependent. The variations of the size of the cells is one of those physical changes to which, up to the present time, but little attention has been paid. That some change really occurs is highly probable on *a priori* grounds. The more general the change of function by so much the greater the quantity of cells which suffer modifications in different ways, and, amongst other things, in their size. Dr. Manassein has particularly devoted himself to the study of the changes produced in the red blood-corpuscles by exposure to various circumstances producing augmentation or depression of temperature of the body generally. Amongst the causes producing augmentation of the body temperature are septicæmia, exposure of the whole animal to heat, and the immersion of the animal in an atmosphere charged with carbonic acid. All these produce diminution of the size of the red blood-corpuscles in all their dimensions. On the other hand, the respiration of oxygen, exposure of the whole body to cold, the administration of quinine, of cyanic acid, of alcohol, acute anæmia, all produce enlargement of the blood-corpuscles.

SEA-WATER AS A TONIC.

At the meeting of the Académie des Sciences of Nov. 27th the perpetual Secretary stated the remarkable fact that excellent bread can be made with sea-water instead of ordinary spring-water, and that this bread appeared to constitute an excellent tonic. Soup or broth, on the contrary, made with sea-water proved totally uneatable. M. Boussingault reminded the members that various peoples living on the seaboard of America drank sea-water after the previous addition of some fragments of cane sugar. From the former of these facts, it would appear that the chloride of magnesium during the process of baking is raised to a temperature sufficiently high to effect its destruction, which does not occur when it is merely boiled as in making soup. In the latter case, it would seem that when cane sugar is added to it, a compound is formed of the sugar with the chlorides which has not the disagreeable taste of the latter. A commission, composed of MM. Chevreul, Boussingault, Balard, Jules Cloquet, and Dumas, has been appointed by the Academy to investigate the question.

RESPIRATION OF FISHES.

M. **Grehant**, in his lectures on Respiration recently delivered at the Ecole Pratique de la Faculté de Médecine de Paris, mentions some interesting facts in relation to the respiration of fishes. He refers to the researches of MM. Humboldt and Provençal, who found that a tench placed in a small quantity of water (three or four quarts) used up nearly the whole of the oxygen dissolved in the course of seventeen hours, whilst a quantity of carbonic acid, amounting to about four-fifths of the oxygen removed, was exhaled. They found that the whole surface of the body of the tench acted like the branchiæ in removing the oxygen, as the same amount disappeared when the tench was only so far immersed that its head and gills were free. In one of M. Gréhant's experiments, five gold fish removed the *whole* of the oxygen from a small quantity of water before they were asphyxiated, whilst double the amount of oxygen absorbed was exhaled of carbonic acid. A small amount of nitrogen appears to be exhaled. He points out the analogy that obtains between the respiration of fish and of the fœtus in utero.

TRANSFUSION.

Professor **Th. Jürgeson**, in the *Berlin. Klin. Wochenschrift*, describes three cases of transfusion of blood, of which two were followed by recovery, and one by transitory improvement.

The first case was one of phosphorus-poisoning. A man, twenty-eight years of age, had taken a solution of the ignition mass of eight bundles of matches. He had icterus on the tenth day, which was accompanied with bleeding from the mucous membrane of the nose and throat. The patient became weaker and fainter; the activity of the heart decreased; and after he had begun again to take food Professor Jürgeson thought first of the transfusion, in order to bring about a compensation for the destruction of cells, and to spur the heart to greater energy. On February 11th (the man had drunk the phosphorous mass on Dec. 9th) M. Völkers, by direction of Professor Jürgeson, effected the transfusion of 580 cubic centimetres of blood into a vein of the arm. At the same time about 500 cubic centimetres were taken from the radial artery, whilst during the operation the heart's activity was excited by champagne. The defibrinated blood used had been the moment before taken from three healthy persons. The course of the disease after the transfusion changed for the better quite strikingly and rapidly. The man increased in strength, and was able to leave his bed at the beginning of March. It is an interesting fact that the blood taken from the patient, on investigation, showed not only diffused blood-coloring matters, but was generally relatively about as rich in solid constituents (poor in water), especially in albuminous and extractive products, as the blood of a cholera patient.

The second case was that of a young lady of twenty-two years of age, who had already suffered for a long time from chronic ulcer in the stomach, which perforated, and was followed by circumscribed peritonitis, and this again by pleuritis. On account of great dyspnœa and frequency of the pulse, puncture of the thorax was performed on March 20th, and the fluid exudation drawn off. In consequence of great weakness of the heart, the depletory transfusion of 350 grammes of defibrinated blood was made into a deep-lying vein of the arm. The blood had been previously (the evening before) drawn from five or six healthy people, defibrinised, packed in small well-closed bottles, and kept in a cellar during the night, so that it was used for the experiment fourteen hours after being drawn off. This was necessary because the patient lived in the country several miles from Kiel. The temperature of the body increased, however, after the transfusion to about 40° R. The collapse could only be combated by champagne. In the night the patient was restless, but on the next day she was in a much better condition. On the following days the fever was very high, and the only means of avoiding death seemed to be by drawing away the exuded fluid in the pleura. With this object the patient was brought by steamer to Kiel —a distance of fifteen miles. Symptoms of collapse from weakness of the heart now made their appearance, and seemed to indicate on the 31st of March another transfusion. On this occasion Völkers injected about 175 cubic centimetres of blood into the radial artery, during which the resistance to be overcome appears to have been enormous. The good effects first appeared twenty-two hours later, but on the 1st April a thrombus was formed in the axillary vein and left vena femoralis. On account of the effusion in the pleura having increased, an incision was made in the fifth intercostal space, to get rid of the fluid, and to remove the pressure from the inferior vena cava. The patient died on the following day.

The third case was one of poisoning by carbonic oxide in a man twenty-eight years of age. He was quite unconscious; the breathing ceased at times; his body was affected by clonic spasms. Temperature 39° R., respiration 64, pulse over 200. 400 cub. centimetres of blood were drawn from the median vein, and immediately 375 cub. centimetres of healthy defibrinated blood injected. By special analysis *carbonate of Hæmaglobulin* was proved to exist in the blood. After the transfusion he was ordered warm baths with cold douches. Three hours after the operation the patient began to speak. Four days later an extensive gangrene of the skin showed itself on the gluteal regions, the trochanters, and the soles of the feet, and later a large abscess was formed under the skin of the back. Prof. Jürgeson conceives this to have been brought about in this way: that by the action of the carbonic oxide gas and diminution of assimilation of oxygen, so many injurious matters became heaped up in the tissues, that they fell into decay on the slightest provocation. The gangrene of the sole of the foot, however, could hardly be explained on this supposition, for the feet are subjected to no pressure by a quiet position in bed. On the gluteal regions 160 square centimetres of skin were destroyed. But, in spite of everything, the patient recovered, haying been kept for a long time in a permanent bath. Prof. Jürgeson comes, after a long theoretical disquisition, to the conclusion that, in poisoning by carbonic oxide, transfusion must come into use, not as the last, but as the first remedy.

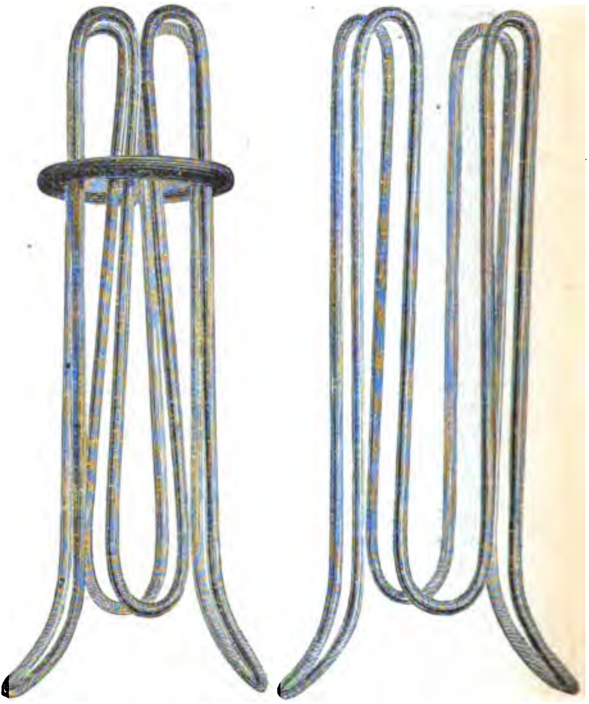
New Inventions

IN AID OF THE

PRACTICE OF MEDICINE AND SURGERY.

BLACKBEE'S RESILIENT SKELETON SPECULUM.

THE engravings here shown present the open and closed views of an entirely new form of speculum, of a most simple and novel construction, and which may be said to possess great advantages without mechanical complexity. Its chief features are as follows:—1. From its blades—outline or skeleton shape—being composed of hard German silver wire



strongly plated with pure silver, it offers the most extensive view possible of the entire circumference of the vagina, and cervix uteri, permitting the easy application of necessary remedies to their whole surfaces. 2. Its resiliency is a great feature in its favor, rendering it easy of introduction and safe in withdrawal, readily adapted, self-adjusting and self-retentive, thus allowing the free use of both hands of the operator. 3. Being free at both extremities, this speculum, by slight manipulation, may be contracted or expanded to suit special cases and requirements. 4. As to durability (having no screws or levers), it may be deemed practically indestructible, as nothing but absolute violence could possibly injure it. 5. It is extremely light for a metal instrument, and the cheapest speculum yet offered to the medical profession. The ring, useful at first, may, after a little acquaintance, be entirely dispensed with.'

This speculum may be obtained of Messrs. Maw, Son, and Thompson, Aldersgate-street.

Foreign Gleanings.

SECONDARY HÆMORRHAGE AFTER GUNSHOT WOUNDS OF THE NECK.

What is to be done when, after a gunshot wound on the left side of the os hyoides just below the lower jaw, hæmorrhage occurs two days after the infliction of the injury? Search for the vessel in the suppurating wound after having enlarged the latter. All surgeons, however, know the difficulties of such a proceeding; and they proved so great in a case lately published by M. Raynaud (meeting of the Surgical Society of Paris, August 9th, 1871) that he very justly tied the common carotid. The patient, a young soldier, rallied for two days after this deligation, but on the third fresh hæmorrhage occurred. M. Raynaud now trusted to plugging and to perchloride of iron; but this turned out to be unavailing; and, having taken M. Nélaton's advice, the surgeon resolved to have recourse to transfusion, as the patient was evidently dying. This was done with M. Raynaud's own blood, about eleven ounces being injected. A whole hour was taken up by the injection, one drachm after another being slowly transfused. The patient seemed to rally after the operation, but he became very weak in the evening, and died about ten hours after this last resource had been adopted. On examination, the ligature was found intact, about three-quarters of an inch below the large solution of continuity caused by the contusion of the projectile. None of the vessels were cut clean through, but eschars had fallen from most of them. (The precise vessels are not mentioned by the author.) One fibrinous clot had formed, the peduncle of which was still within the distal extremity of the tied vessel. M. Raynaud expresses some distrust of the deligation of the main artery, as secondary hæmorrhage is so frequent. He did not follow Mayo and Bérnard's advice to tie, besides the common carotid, the internal or external vessels, or to tie simply the last two. He thought such searching after the vessels, including the inferior thyroid, tantamount to a regular dissection on the living body. The patient had no cerebral symptoms, but he died too soon after the deligation of the common carotid to offer an illustration of the nervous sequelæ of this operation. M. Raynaud used Mathieu's instrument for transfusion, and was careful to beat the blood with glass rods, and filter it through clean flannel, to avoid clotting of the fibrine. He maintains that the care sometimes bestowed on preserving the temperature is useless, as the globules will regain sufficient heat on passing into the patient's vein. This position is supported by experiments of his own on animals.

This case was reported upon to the Society by M. Després, who blamed the author for not having gone on tying the external and internal carotid, as also the inferior thyroid. He thinks that, in all cases where the main trunk is tied, the distal end should also be sought for and deligated. M. Després discusses the likelihood of paralysis occurring after the common carotid artery is secured, and establishes the following interesting contrast:—Paralysis occurs from anæmia of the brain, the vaso-motor nerves no longer stimulating the capillaries; in such cases secondary hæmorrhage does not take place. But when the latter occurs, through powerful anastomosis, the circulation is renewed, the brain nourished, and no paralysis is observed. It should not be forgotten that some surgeons, especially Professor Richet, think that the latter is caused by some lesion of the sympathetic during deligation. A distinction should always be made between the stupor following immediately the tying of the carotid and the paralysis which may take place some days afterwards. M. Després considers that we have not as yet sufficient data respecting the actual results of transfusion to put any faith in it; he thinks that we do not know, in successful cases, whether the stimulants administered or the blood injected have been instrumental in the favorable results.

M. Verneuil has also tied the common carotid in a case of gunshot wound of the cheek, with a very small aperture of entrance. The case was remarkable for the long period before secondary hæmorrhage occurred (twenty-one days), and the number of times efforts were made to stop the bleeding by almost all the known hæmostatic agents (fifteen times). At last M. Verneuil tied the common carotid; the patient became hemiplegic during the night which followed the operation, and died the next day, forty-two hours after the operation. The post-mortem examination did not reveal which vessel had ulcerated and bled, and failed to lead to the discovery of the projectile. The operation was rather remarkable, inasmuch as M. Verneuil, after having laid bare the vessel and applied a ligature, feared the former had been slightly scratched or injured. On this account he placed a second thread about a third of an inch above the first, which thread was afterwards found to have constricted both the internal and external carotid. The author had at first intended to tie the external only, but having brought the common fairly into view, thought it best to secure it. He stated, in some remarks before the Surgical Society of Paris, that he was sorry he had not confined himself to the external, and that he thought the tying of the internal had been productive of the attack of hemiplegia. M. Verneuil is inclined to attribute the frequent recurrence of the hæmorrhage to purulent infection, and to certain states of the liver. Herein we must differ from him, as we firmly believe that causes need not be sought so far. The rule deduced by M. Verneuil from this case is, that in hæmorrhage from the face the surgeon should secure the external carotid.

FRACTURE OF THE SHAFT OF THE HUMERUS BY MUSCULAR CONTRACTION IN A SYPHILITIC PATIENT.

On the 4th of August last M. Verneuil admitted into the Lariboisière Hospital a man forty-three years of age, who, after using much force in wringing clothes, felt a pain in his arm, and heard a distinct crack. On examination, it was found that the shaft of the right humerus was broken. The patient had various cutaneous signs of syphilis upon him, and stated that he had suffered from the disease eight years before. A proper apparatus was applied, iodide of potassium was ordered, and in a few weeks afterwards consolidation had taken place. Pain had been felt a few weeks before admission, so that it may be inferred that syphilitic osteitis had here taken place.

ARTIFICIAL SULPHUREOUS WATERS.

M. Thommeret-Gélis states, in *L'Union Méd.*, Nov. 28th, 1871, that Béchamp has shown that sulphuret of sodium or calcium does not exist undecomposed in natural sulphureous waters, but that the aqueous fluid alters these compounds simply by its mass. By appropriate tests the state of combination of the sulphuretted hydrogen can be made out. Starting from these data M. Gélis, proposes for the artificial manufacture of sulphur waters, the sulphuret of sodium saturated with sulphuretted hydrogen. This salt does not irritate the tissues, is colorless, and crystallises in long deliquescent prisms, which contain 6 equivalents of water, NaS, HS, 6 (HO). With it may be prepared granules, syrups, and baths. Each granule may be taken as a pill, or when dissolved in a tumbler of water makes a glass of sulpher water. A bath prepared with this salt does not irritate the skin.

POWDERED CAMPHOR IN PHAGEDENIC CHANCRES.

Dr. Netter has shown, in the *Gaz. des Hôpitaux*, that hospital gangrene yields to the applications of powdered camphor. Thereupon M. Baudoin, as stated in *L'Abeille Méd.*, was induced to try the same remedy in phagedenic chancre. Three cases are quoted as proving the efficacy of the application; but the author gives no heed to the means which had been used before, the effect of which might not have quite passed off, and he leaves unmentioned whether any internal remedy or hygienic measure was resorted to or not.

PLEURAL HYDATIDS.

M. Moutard Martin has under his care, at the Hôpital Beaujon, Paris, a lad of seventeen, who was far advanced in all the symptoms of pleuritis with effusion. A fine trocar, aided by the vacuum syringe, failed to evacuate more than a few drops of pus. The regular operation for empyema was now performed; much purulent matter was discharged, and after it several flaccid hydatids. Profuse syringing with water and alcohol was now resorted to, and the patient is expected to do well.

TETANUS TREATED BY INHALATIONS OF OPIUM.

This is a Chinese mode of treating tetanus, and is recommended by Dr. Shrimpton. From four to five grains of solid opium are mixed with tea-leaves or dried roses, and carefully beaten together with molasses. The patient smokes this mixture, and endeavors to draw the smoke into the lungs, leaving off when the narcotic effects are produced. These last generally from three to four hours. The same operation should be repeated whenever there are any signs of returning spasms.

THE BLUE STAINS OF TYPHOID FEVER.

It is well know that Trousseau, Griesinger, Grisolle, and Monneret considered the appearance of these stains in fever as a favorable sign. M. Guilhem, however, publishes in the *Gaz. Méd. Chir. de Toulouse*, July, 1871, a case in which these stains were observed, and which ended fatally. It would be useful to collect cases of typhoid fever where the stains were noticed, to determinine their real value in prognosis.

SEDATIVE PILLS IN BLENNORRHAGIA.

M. Van den Corput recommends the following pills in cases of inflammatory erections in gonorrhœa, or in the genesic erethism of children: Extract of belladonna, two grains; lupuline, twelve grains; camphor, twelve grains: for eight pills; from two to four at night. The dose of belladonna seems rather large.

News Items, Medical Facts, &c.

Treatment of Cholera Infantum.—According to a communication from Dr. Irving C. Rosse, dated from the War Department, Surgeon-General's Office, Washington, in a late number of the New York *Medical Record*, bromide of potassium is said to be regarded by the medical officers stationed at the artillery school, Fort Monroe, as a specific for this affection—as prompt and efficient as quinine for ague. Careful attention to hygienics at the same time that it was administered was enjoined. The food was prepared by pounding a small quantity of fresh beef in a linen cloth, and expressing the juice, to which a very small seasoning of Cayenne pepper was added. Minute quantities of brandy were administered where its use was indicated. The medicine was usually prepared by mixing from twenty to forty grains of bromide of potassium with two ounces of mucilage, the dose being from ten drops to a teaspoonful, and regulated according to circumstances. Occasionally a drachm of krameria was added to this preparation. The therapeutical value of the bromide had been mentioned in Circular No. 4, Surgeon-General's Office, in a report of Surgeon George Cooper, U.S.A.

An Instrument for Micro-ruling on Glass and Steel.—An ingenious instrument for this purpose has been constructed by Mr. J. F. Stanistreet, the details of which, together with an explanatory plate, are given in the *Monthly Microscopical Journal*. The machine is constructed for ruling lines from the 1000th to the 10,000th of an inch apart; and Mr. Stanistreet has added to it the means of further subdivision to the 10,000th of an inch, but he finds a difficulty in procuring any diamond fine enough for ruling distinctly more than about 5000 lines per inch. It speaks highly for Mr. Stanistreet's mechanical ingenuity that, with the exception of one little steel pinion, he has constructed the entire instrument with his own hands.

Hutchinson's Syphilitic Keratitis.—M. Panas brought forward, at a meeting of the Surgical Society of Paris, Nov. 15th, 1871, several cases of keratitis which seem to him to disprove some of the views of Mr. Hutchinson. The author considers that some doubts may be raised as to this diffused keratitis being always connected with hereditary syphilis. Messrs. Demarquay, Giraud Teulon, and Giraldès spoke in favor of Mr. Hutchinson's views, and Messrs. Dolbeau and Perrin against. All the speakers were unanimous as to the skill and originality displayed by Mr. Hutchinson.

Three cases of Asiatic cholera are reported as having proved fatal at Halifax, Nova Scotia; the disease having been introduced into the country by the steamer *Franklin*, from the Baltic.

Dr. Alfred Meadows has been elected a corresponding member of the Gynæcological Society of Boston.

Dr. Tilbury Fox has been elected a corresponding member of the New York Dermatological Society.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery,
Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 4. NEW YORK, APRIL, 1872.

PRACTICAL PHYSIOLOGY.

Lectures
ON
EXPERIMENTAL PHYSIOLOGY.

By Wm. Rutherford, M.D., F.R.S.E.,

Professor of Physiology in King's College.

LECTURE XI.

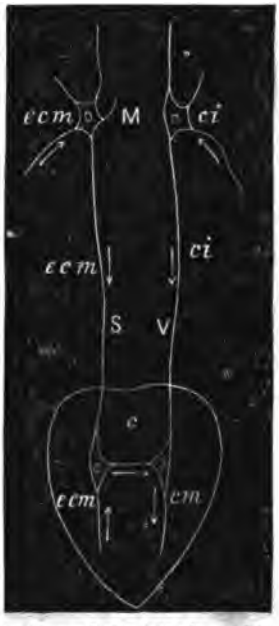
CIRCULATION—(CONTINUED.)

In the last lecture the following diagram was given as indicating probable nerve arrangements concerned in the innervation of the heart. It was shown that within the cardiac organ (Fig. 48, *c*) there is a reflex-nerve mechanism, consisting of cells connected with intra- and extra-cardiac nerves; the former being cardio-motor (*c m*) and excito-cardio-motor (*e c m*), the latter cardio-inhibitory (*c i*) and excito-cardio-motor (*e c m*). The extra-cardiac nerves are branches of the vagues (V) and sympathetic (S); both are connected with the medulla oblongata (M). Both nerves transmit their influence to the heart, the sympathetic accelerating the cardiac action, the vagus inhibiting or retarding it, and—if powerfully stimulated—even for a time completely arresting the cardiac contractions. During the arrest the heart is *dilated*. The branch of the vagus which does this is, in rabbits, the inferior cardiac. We studied the action of another branch of the vagus, the superior cardiac, and we saw that it is the sensory nerve of the heart, and, moreover, that it is an inhibitory nerve for bloodvessels—that is to say, when it acts it brings the contractile elements of blood-vessels into a state of rest, thereby causing dilatation. So while the inferior cardiac branch of the vagus is *cardio*-inhibitory, the superior cardiac branch is *vaso*-inhibitory. The former transmits its influence from the medulla oblongata to the cardiac ganglia, the latter transmits its influence from some portion of the heart to the medulla oblongata. We will presently consider how it brings about the dilatation of bloodvessels.

Innervation of Bloodvessels.

By the investigations of Bernard and Brown-Séquard it has been established that the contractile elements of the bloodvessels are presided over by motor nerves. These are in consequence named vaso-motor nerves. Their action produces diminution in the calibre of the bloodvessels. If the action of the vaso-motor nerves be suspended the blood-vessels dilate, most probably because, their contractile elements being paralysed, the pressure of the blood is able to distend the vessel. The vaso-motor nerves are immediately derived from the sympathetic; but Ludwig and Thiry have shown that the nerve-cells presiding over them are not contained in the sympathetic ganglia, but in the medulla oblongata. The *general* vaso-motor centre

Fig. 48.

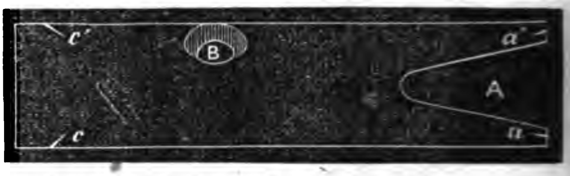


for the body is in the *cerebro-spinal* nervous system. I think it safe to say *general* vaso-motor centre, for possibly there are vaso-motor nerve-centres in the submaxillary ganglion, and in the ganglia found in the penis. These peripheric vaso-motor centres are quite insignificant compared with the great centre in the medulla. Recently the position of the vaso-motor centre in the medulla has been investigated by Ludwig and Owsjannikow. From experiments upon rabbits and cats they have ascertained that it extends from one millimetre below the corpora quadrigemina to five millimeters above the calamus. It lies on either side at a little distance from the middle line. From this centre the vaso-motor nerves pass down the spinal cord to various parts of the body. They leave the cord in the anterior roots of the spinal nerves; from thence they either pass through, or receive branches from, the ganglia upon the sympathetic, and proceed to the bloodvessels. One might anticipate that the vaso-motor nerves for the head and face would leave the medulla with the other nerves for these parts. This is not the case. They descend through the cervical portion of the spinal cord, leave it at the lower part of the neck, join the cervical sympathetic nerve, and ascend in that to the bloodvessels of the head. Even the vaso-motor nerves for the bloodvessels of the pia mater take this roundabout course.

The general vaso-motor centres appears to be more or less constantly in action; so that the bloodvessels are usually maintained in a semi-contracted state. In consequence of this we generally find that vascular dilatation follows division of these nerves. It is easy to demonstrate this fact. I have just said that the cervical sympathetic contains the vaso-motor nerves for the head. We take a rabbit with white ears. Look at the aural bloodvessels: the vascularity of both ears is similar; and when one closely observes the artery running up the middle of the ear one may often see it contracting rhythmically. At intervals it suddenly narrows and then dilates. A distinctly peristaltic contraction may also sometimes be seen in the veins. We now expose the sympathetic on one side of the neck. It lies just behind the vagus, close beside another small nerve, the superior cardiac branch of the vagus. You may be puzzled to know which is the sympathetic. Remember that the sympathetic is the *larger* of these two small nerves. [The cervical sympathetic was divided on one side. The consequent dilatation of vessels and increase of temperature were observed. It was pointed out that the rhythmical contractions of vessels had disappeared. The cranial end of the nerve was then stimulated, and the consequent pallor of the ear demonstrated.]

The largest vaso-motor nerve in the body is the great splanchnic nerve. It presides over the vessels of most of the abdominal viscera. It is sometimes necessary in physiological experiments to paralyse the bloodvessels of the body generally. This is readily done by dividing the cervical portion of the spinal cord at some little distance below the medulla oblongata. If, after the section, we desire to stimulate the vaso-motor nerves generally, we require to place two electrodes in the cervical portion of the cord (see Fig. 19); one on a level with, say, the third, the other on a level with the fifth, cervical vertebra. The contraction of vessels produced in this way may be readily seen in the frog. I have injected 1–2000 gr. of curara under the skin to paralyse the voluntary motor nerves, and so get rid of spasm of the voluntary muscles when we stimulate the spinal cord. I stretch the web, tie threads round two neighboring toes, and fix them in slits on either side of a triangular window in a piece of cardboard. (Fig. 49.) The back is uppermost. Two common sewing needles fixed in a piece of gutta-percha are pushed between the scapulæ into the spinal cord. The heads of the needle-electrodes are connected with fine wires, the other ends of which are joined to a faradic machine, so that we may faradise the cord when we please. When we watch the bloodvessels of the web with a power of some 200 diameters and faradise the spinal cord with a somewhat powerful current, there is generally little difficulty in seeing that the arteries and capillaries become narrowed. The narrowing is most marked in the case of the arteries. In performing this experiment it is necessary to beware of giving too large a dose of curara. A powerful dose paralyses not only the voluntary motor but also the vaso-motor nerves. Do not give more than 1–2000 gr. Everyone admits that the arteries and veins possess contractility. Bennett has always maintained that the capillaries also possess this property. Recently, Stricker has lent his support to Bennett's view. Others, however, are of opinion that the narrowing of the capillaries is always due merely to their recoil, when, owing to contraction of the arteries, a smaller quantity of blood is permitted to enter them. I must say that I have not in full-grown frogs seen any capillary contraction which might be not referred to simple elastic recoil. In the tail of the tadpole, however, the capillaries are distinctly contractile.

FIG. 49.



Frog plate, made of a piece of cardboard. A, Slit over which the web may be stretched. B, Slit partially surrounded by a crescent of cork, to which the intestine may be pinned when the mesentery is to be examined. A slip of glass may be put above and below the web or mesentery, if desired. *a*, *a'*, Slits for fixing the threads round the toes. *c*, *c'*, Slits for the threads round the arms. If the animal is not narcotised or paralysed, it is necessary to take a piece of muslin: make a hole in it sufficiently large to allow one leg to pass through it; after the leg has been thrust through the hole, gather up the muslin around the head so as to enclose the other leg and the arms; tie a string round the neck of the bag, and draw the ends of the string through the slits *c* and *c'*, and tie them together.

The general vaso-motor centre in the medulla oblongata appears then to be in almost constant action, thereby keeping up some degree of contraction of the vessels. The activity of the centre may be increased or diminished by the excitement of various nerves. It may be increased by stimulating sensory nerves, by accumulation of carbonic acid in the blood, and by certain emotions. Increase of its action following the stimulation of afferent nerves leads us to think that there is a reflex-nerve mechanism whose nerve-centre is in the medulla. I therefore construct this diagram

FIG. 50.

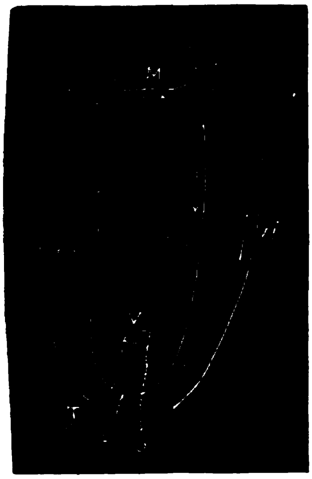


Diagram to represent what is probably the nerve-mechanism concerned in the innervation of bloodvessels. An afferent and an efferent nerve-cell in the medulla oblongata, M. V, A bloodvessel. *v m*, Vaso-motor nerve. *v i*, Vaso-inhibitory nerve, ending in the peri-vascular tissue. *e v m*, Excito-vaso-motor nerves, ending in the vessel and in the peri-vascular tissue.

(Fig. 50). I put two cells in the medulla oblongata (M) to represent the centres for the reflex action. One cell might do; but as I have always used two cells for the reflex mechanism, I use them here of course. I am disposed to think that these cells correspond to the *intracardiac* ganglia. (See Fig. 48.) I am inclined to think that the cells of the cardiac ganglia have, as it were, been taken out of the medulla away from the general centre for the vascular system, and have been put in the heart. Mark you, these intracardiac cells are *still directly connected* with the *medulla* by the vagus and sympathetic. Think over this point. Well, I put in the diagram three fibres to correspond to the intracardiac fibres (Fig. 48), one intercentral, the other two joining the cells to a bloodvessel (V). One of these, *v m*, is efferent, and vaso-motor in its function. About its existence there can be no doubt. The existence of the other fibre is not so certain. It is an afferent fibre, and excites the cells in the medulla. Hence I call it excito-vaso-motor. It seems to convey influences from the lining membrane or wall of the vessel to the vaso-motor centre, just as the corresponding fibre in the heart (*e c m*, Fig. 48) conveys excito-motor influences from the endocardium to the intracardio-motor ganglia. I am disposed to think that there is this afferent fibre proceeding from the vascular wall to the medulla, because if we force a cold fluid into the vessel of a warm-blooded animal, or any fluid under a great pressure, the vessel contracts. No doubt this contraction can take place even after destruction of the vaso-motor centre in the medulla, but it takes place much more readily if the vaso-motor centre and its nerves be intact. But, inasmuch as afferent nerves connected with other parts—e.g., the skin, or, indeed, nerves of ordinary sense anywhere—cause vascular spasm when stimulated, I put in another excito-motor fibre (*e v m*, Fig. 50) ending in the tissues. This fibre probably corresponds to the extra-cardiac excito-cardio-motor nerves (*e c m*, Fig. 48), and, in order to bring out this resemblance more strongly, I have put it in this diagram as a separate fibre.

The action of the vaso-motor centre may, however, also be diminished. The effect of this is diminished contraction or complete relaxation of the bloodvessel. The vaso-motor nerve being for the time paralysed, the walls of the vessels relax, and the blood-pressure and the elasticity open up the vessels. The nerves which do this I call vaso-inhibitory nerves. They correspond to the cardio-inhibitory nerves (Fig. 48, *c i*). Bernard discovered the fact that bloodvessels may be dilated by stimulating certain nerves. He found that when he divided the great auricular nerve in the rabbi and then stimulated the central end the bloodvesse of the ear became dilated. Slight contraction preceded this dilatation. It is not difficult to satisfy oneself of the truth of this. There are two nerves in the rabbit's ear which are readily available for this experiment—the great and the posterior auricular nerves. The former lies just under the skin in the middle of the ear, running from the base towards the tip, immediately anterior to the central vessel. This nerve is derived from the second and third cervical nerves. The posterior auricular nerve, a branch of the second cervical, lies near the occipital spine. It is at first covered by a thin layer of muscle, and afterwards penetrates a plate of cartilage, which can be felt with the finger between the ear and the occipital spine. The first nerve is most easily exposed. If we divide it and faradise the central end, the artery contracts and then rapidly becomes greatly dilated. The dilatation is often greater than that which follows division of the vaso-motor nerves. Lovén showed that the stimulation of the central ends of other sensory nerves in the rabbit produced transient contraction and then continued dilatation of the bloodvessels in the parts supplied by the nerves. But he ascertained something more than Bernard. He discovered that while stimulation of a sensory nerve produces dilatation of vessels in the territory supplied by the nerve, it produces at the same time contraction of vessels in other parts of the body. This double effect, however, takes place only when the brain is active. If narcotism be produced by chloral, or if the cerebrum be removed, stimulation of sensory nerves does not produce vascular contraction, but only vascular dilatation. From the researches of Droschfeld and Cyon it appears, then, that the ordinary sensory nerves contain fibres which are vaso-inhibitory, and fibres which are excito-vaso-motor. The former appear to act directly upon the vaso-motor centre in the medulla, so as to inhibit or diminish action; the latter appear to act on that centre through some part of the brain above the medulla oblongata, so as to increase the action of the vaso-motor centre.* The most remarkable vaso-inhibitory nerve which we know is the superior cardiac branch of the vagus. If we divide this nerve and stimulate its central end we cause a remarkable lowering of the blood-pressure, owing chiefly to dilatation of vessels in the abdominal viscera and lungs (Ludwig and Cyon). The vascular dilatation in this case takes place without any previous contraction such as happens in the other cases I have mentioned. In all these instances the vascular dilatation follows stimulation of the central ends of the divided nerves. The inhibiting influence seems to be conveyed to the medulla. But there are two instances where vascular dilatation follows stimulation of the peripheral, and not the central, ends of the nerves—to wit, the chorda tympani and the nervi erigentes of the penis. You have seen me perform Bernard's experiment on the chorda tympani. When the peripheral end of the divided nerve was stimulated the vessels of the submaxillary gland dilated. Eckhardt has shown that if in the dog the peripheral ends of the divided nervi erigentes of the penis be stimulated, dilatation of vessels ensues. The chorda transmits its influence to the submaxillary ganglion. There are many ganglionic cells upon the nervi erigentes. Are these cells peripheral vaso-motor centres, whose action is inhibited by stimulating these nerves?

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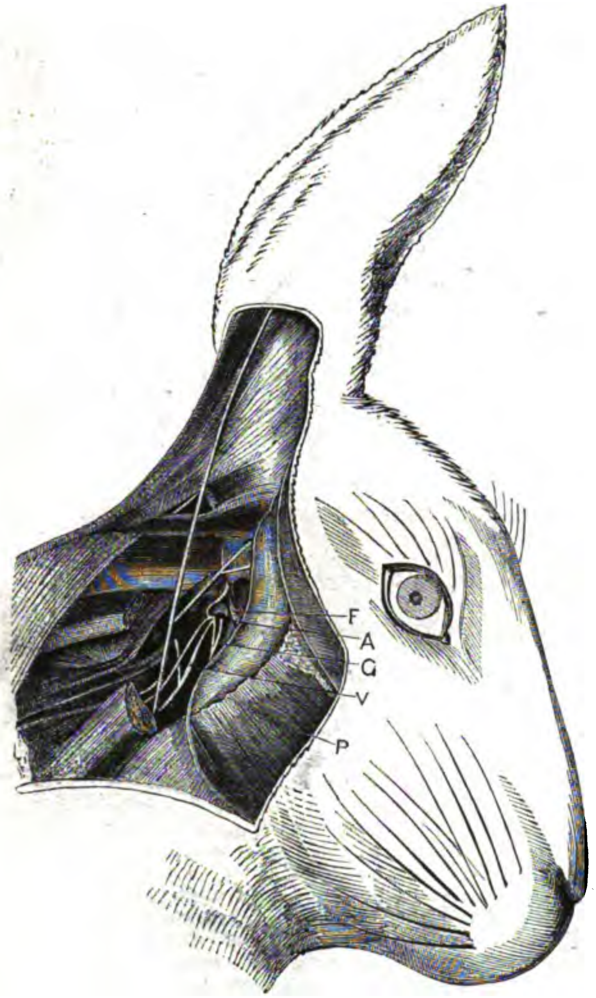
* The left cell in Fig. 50 ought to have been placed higher than the right cell.

FIG. 51.



Some of the nerves of the rabbit's ear. The great auricular nerve is seen running up the centre of the ear. The posterior auricular nerve is not seen. F, Facial nerve, giving off its auricular branch, A. G, Glosso-pharyngeal nerve. V, Vagus. Both send branches to the ear. P, Parotid gland. (After Loven.)

The idea commonly prevails that when a part becomes the seat of active nutritive change its bloodvessels undergo dilatation, by reason of the increased attraction for blood manifested by the tissues. The *vis a fronte* is supposed to become so powerful that it can overcome the contraction of the arterial walls, and thereby produce dilatation. It occurred to me some years ago that the vascular dilatation in such a case is possibly the result of an influence transmitted by the tissue through its vaso-inhibitory nerves. Lovén came near this idea, and it is surprising that he did not hit upon it. He showed that the bloodvessels of a part may be dilated by the artificial stimulation of the afferent nerves of that part; and, in speaking of the purpose to be served by this, he merely said that in an inflamed or irritated part the nerves bring about vascular dilatation, probably because "a broad and rapid stream" of blood is of service in such a case. Such a rapid stream must result "if the small arteries of the irritated part are dilated while all others are contracted." It seems to me that Lovén was only thinking of the phenomena which follow upon the irritation of a part by blisters and such-like things. The idea arrived at by me, quite independently of Lovén, long before I had seen his paper,* differed from this. I thought that, whenever the nutrient changes in a part are so affected that a greater supply of blood is needed, the vaso-inhibitory nerves of the part are thrown into action so as to dilate the bloodvessels of the part; so that the vascular dilatation ascribed to increased *vis a fronte* is really the result of the influence of vaso-inhibitory nerves. That this is so I was led to believe from observing the following facts regarding the gastric bloodvessels. These vessels receive their vaso-motor nerves from the splanchnic. The vaso-inhibitory and excito-vaso-motor nerves for the stomach are contained in the vagi. When gastric juice is being secreted, the gastric blood-vessels are much dilated. If we divide the vagi during digestion, the gastric bloodvessels contract and remain contracted. Stimulation of the peripheral end of the divided vagus never dilates the bloodvessels. Stimulation of the central end can do this. I therefore concluded that during digestion the gastric bloodvessels are dilated by the action of vaso-inhibitory nerves. I have thus been the first to succeed in finding these vaso-inhibitory nerves in action, and to suggest the *rôle* which they seem to play in enabling the tissues to regulate the supply of blood needed by them. When the gastric vaso-inhibitory nerves are thrown into action in this normal way, there is no evidence whatever of the other bloodvessels of the body being thrown into contraction, as Lovén contemplated in his idea of the mechanism brought into play when a part is irritated. But if the tissues dilate their bloodvessels by influencing vaso-inhibitory nerves, what is the mechanism by which the activity of the vaso-motor centre is kept up so as to partially contract vessels when little blood is wanted by the tissue? Here is a great difficulty. There is no evidence that the tissue, when wanting little blood, stimulates the vaso-motor centre by acting through its excito-vaso-motor nerves. When do these nerves act, then? Does the action of the vaso-motor centre proceed immediately from the centre, or is it reflex? Further research is needed on this subject.

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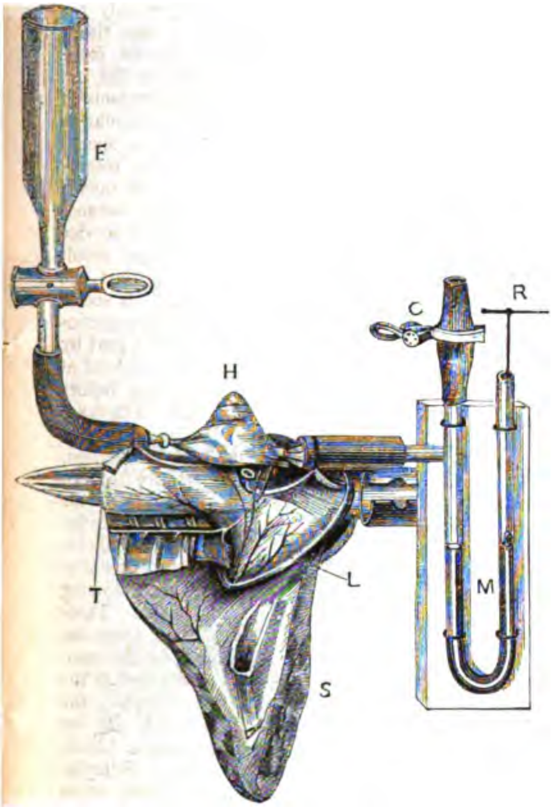
* Ludwig's Arbeiten, 1867.

Appendix to Innervation of the Heart.

The actions of poisons upon the frog's heart may be conveniently studied by means of an arrangement described in Ludwig's *Arbeiten* for 1870, by Dr. Joseph Coats, of Glasgow. The arrangement (Fig. 52) was specially intended for assisting in the study of the influence of the vagus upon the heart's movements, but it is well adapted for researches on the influence of poisons. The frogs must be strong ones. English frogs are not well suited for the purpose. The following is the manner in which the animal is prepared for the experiment:—1. Destroy the brain and spinal cord by thrusting a pin down the cerebro-spinal canal. 2. Remove an oval or V-shaped flap of skin from the front of the sternum. 3. Remove the sternum. 4. Open the pericardium fully. 5. Lift up the apex of the heart and put a double ligature round a small vein that passes between the dorsal aspect of the heart and the pericardium. Tie in two places, and divide the vein between the ligature. 6. Cut away the pericardium. 7. Tie a glass canula in the inferior vena cava. 8. Tie the superior vena cava. 9. Tie another canula in the left arch of the aorta, and ligature the right aortic arch. 10. Take a glass pipette and carefully fill both canulæ with fresh serum from rabbit's or cat's blood, so as to exclude all air-bubbles. Drive a little serum through the heart to keep it well nourished. 11. Cut the frog through below the liver. Remove the liver. Make a slit in the stomach and pass a pointed glass rod or tube through the mouth, œsophagus, and stomach. 12. Connect the aortic canula with the manometer, and the other canula with the funnel (see Fig. 52), and permit some serum to flow through the heart. Then dissect out the vagus if desirable. The serum requires to be often renewed, as the heart soon uses up the patulum contained in it.

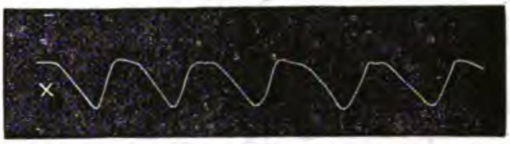
FIG. 52.



Apparatus for studying the influence of the vagus or the influence of poisons upon the frog's heart. —M, Mercurial manometer connected with aorta. R, A pin for scratching a trace upon a blackened cylinder. C, Clip upon a piece of caoutchouc tubing. H, Heart. L, Lower jaw. S, Skin. T, A glass tube thrust through the œsophagus. F, Funnel with stopcock. This is filled with serum from rabbits' blood, and being connected with a nozzle in the inferior vena cava, serum can be introduced into the heart when required. The sympathetic nerve is seen lying alongside the vertebræ. The vagus is seen extending between the heart and the angle of the jaw. (Slightly altered from Coats: Ludwig's *Arbeiten*, 1871.)

The arrangement of the apparatus represented in the figure was intended for the study of the influence of the vagus upon the heart. The chief use of the apparatus, however, is to study the action of poisons upon the heart and its nerves. For this purpose the heart may be removed from the body altogether; a portion of the vagus, sufficiently long to permit of its easy irritation, should be left. When the heart is excised, the sinus can be irritated more readily when required. When the influence of a poison is to be observed, it is added to the serum of rabbits' blood. After tracings such as that represented in Fig. 53 have been obtained from the heart supplied with normal serum, poisoned serum is placed in the funnel, and is permitted to flow through the heart. For example, suppose we feed the heart with serum containing nicotine. We should find that while irritation of the trunk of the vagus fails to retard the heart's action, the retardation takes place on irritating the sinus of the heart. This results from the terminal portion of the inhibitory part of the nerve mechanism in the heart having escaped the paralysing influence of the poison. If atropine be given instead of the nicotine, irritation of the vagus trunk is as powerless to affect the heart as in the case of nicotia palsy; but in the case of atropia, irritation of the sinus does not inhibit the heart, because the peripheral terminations of the inhibitory apparatus are palsied by atropia.

FIG. 53.



Tracing taken rom a manometer in connexion with the ventricle of a frog's heart (Fig. 52). This tracing is taken from an unpoisoned heart.

Clinical Lectures
ON
VARIOUS DISEASES.

By A. B. Garrod, M.D., F.R.S.,

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LECTURE I.

ON LEAD-POISONING.—THE RELATION BETWEEN LEAD-IMPREGNATION AND GOUT.—LIABILITY OF GOUTY PATIENTS TO BECOME POISONED BY LEAD.

Importance of the subject—Characters of lead—Authors on lead-poisoning—Characteristic blue line on gums—Methods by which lead is introduced into the system—Causes of lead-poisoning—Workers in lead—Lead in drinking-water, beer, &c.—Impure snuff a cause of lead-poisoning—Remarkable case of lead-poisoning produced by the use of snuff.

Gentlemen,—I propose to bring under your notice in this lecture some of the more important phenomena exhibited by individuals when under the toxic influence of the metal *lead;* to analyse and explain these phenomena; and to show you the different ways in which lead is introduced into the animal economy; and more particularly to point out a way by which doubtless many have been poisoned, although one as yet scarcely recognised either by the profession or the public. It is my intention afterwards to dwell somewhat on the power of lead-impregnation to induce another diseased condition—namely, gout; and, furthermore, to show that a gouty state of system tends to give a susceptibility to lead-poisoning. The cases I shall read to you from the hospital books will illustrate the chief features met with in patients suffering from saturnine influences, but I shall not scruple to make use of many other cases which I have met with in private practice, in order to illustrate any points which I think will be of interest and prove useful to you in your future career. Before speaking of the phenomena of lead-poisoning, it may be as well that we should glance at the properties of a metal which produces such injurious results upon the animal body.

In the first place we have every reason to believe, and, in fact, it may be considered certain, that lead is not a natural constituent of the body, and that, therefore, when present, it must be looked upon as altogether abnormal. Some metals not forming part of the framework are occasionally introduced into the system without the production of any permanent ill effects, but many others are not so innocuous. Can we find any properties, or group of properties, which pertain to the noxious metals, and not to those which are comparatively innocuous?

As a rule, the former class of metals have the power of forming insoluble compounds with the albuminoid constituents of the body; they become, therefore, to a great extent retained and accumulated in the tissues. Lead, for example, has been detected in almost all the tissues in cases where death has ensued; the same remark holds good in regard to mercury.

If, again, we consider the chemistry of the healthy body, we find many elements, as metals, metalloids, &c., forming part of the various tissues: for example, calcium, phosphorus, sulphur, and chlorine. And it will be observed that elements which resemble these, but have much higher atomic weights, usually induce marked physiological or poisonous effects: thus lead and barium, which are isomorphous with calcium, are poisonous; antimony and arsenic, which are closely allied to phosphorus; selenium and tellurium, the analogues of sulphur; also bromine and iodine, the analogues of chlorine, all possess a powerful influence upon the animal economy. These remarks, however, must be looked upon as at present entirely speculative, and valued accordingly.

The diseases which are known to be produced by lead are—1, lead colic; 2, lead paralysis; 3, lead arthralgia, often termed saturnine rheumatism; 4, lead encephalopathy, including various forms of cerebral disturbance.

By far the most common form of disease is lead colic, known also as painter's colic; next the arthralgia; then paralysis; and, lastly, the cerebral affection. In a table formed by Tanquerel des Planches the relative frequency was as follows:—Colic, 1217 cases; arthralgia, 755 cases; paralysis, 127 cases; encephalopathy, 72 cases.

Although I shall point out to you the chief symptoms, and those which are most commonly met with in cases of lead-poisoning, time will not allow me to speak of many of the less common and less marked symptoms occasionally observed; nor do I think it would be desirable to do so in these lectures, as it might distract your attention from the salient points of the subject. Those of you desirous of becoming acquainted with all the minute details which have been observed cannot do better than consult the work of M. Tanquerel des Planches, and an interesting summary of the effects of lead on the system given by Dr. (now Sir James) Alderson, in his Lumleian Lectures delivered before the Royal College of Physicians in 1852. Other authors, both ancient and modern, may be studied, from Hippocrates and Nicander, Paulus Æginetus, Dioscorides, Avicenna, Sydenham, Huxham, Baker, Chomel, &c., &c., to writers of our own times.

There is one symptom—it can scarcely be called a disease—that is so universally met with in lead-poisoning that it is desirable that we should speak of it at once. I allude to the presence of a *blue or bluish-black line* upon the gums. This sign is most valuable, for when marked in any individual the probability of his being affected by the metal is extremely great. This blue line first appears at the free margin of the gums next the teeth, but it sometimes extends to almost the whole of the gums; occasionally also the mucous membrane of the lips, especially the lower lip, where it touches the teeth, is stained of the same color, and the patches are often of considerable size. Sometimes the gums of a few teeth only are affected, and then those of the incisors and canine teeth chiefly. It is not difficult to explain the nature of the blue discoloration. It is evidently due to the formation of sulphide of lead caused by the action of sulphuretted hydrogen in the fluids of the mouth upon the lead compounds circulating or deposited in the gums, the metal being thus rendered insoluble. The lead discoloration on the gums and lips lasts a long time, often many months, after the patient ceases to come in contact with the metal and after the poisonous effects have passed away. In almost every case I shall have to comment upon, the blue line was found; in some there was likewise the discoloration of the inner surface of the lips. I may state that I have scarcely, if ever, known an instance of patients poisoned with lead not exhibiting this symptom; and although I would not deny the possibility of its being absent, yet it must be an extremely rare occurrence, provided the teeth are present and in a condition capable of showing the phenomenon. Sometimes the teeth are absent in these patients, sometimes they are so encrusted with tartar that it is very difficult to make out the line except around one or two teeth. I may observe that the blue discoloration usually precedes other signs; and if the mouths of painters and other workers in lead are looked at, a large number show the phenomenon, even when these individuals have never experienced other symptoms. The presence of a blue line on the gums of any individual becomes therefore a very important point in diagnosis. The discovery of this fact has usually been attributed to Dr. Burton, formerly a physician to St. Thomas's Hospital; it appears, however, that it was described a year before Dr. Burton's announcement by Tanquerel des Planches in his "Traité des Maladies de Plomb."

Lead may be presented to the body in different physical conditions and in different states of chemical combination. It frequently comes in contact with the body in the form of fine powder or dust; sometimes dissolved or suspended. Perhaps the most common salt of lead is the carbonate, called also white lead or flake white; and it was at one time asserted that this salt was the only poisonous one. This, however, is an error; and it can be proved that any salt of lead capable of being dissolved in the fluids of the body is efficient to induce all the toxic effects of the metal. Doubtless some of the salts, as the sulphate and sulphide, are the least poisonous—a fact due to their very sparing solubility. Lead may be absorbed from various surfaces of the body. It is frequently introduced into the stomach and intestines along with the food. This is the case with workmen who handle their food without previously washing their hands. Sometimes it is inhaled by the lungs, as by workers in white-lead mills; and by painters, especially when flatting. Sometimes it is applied medicinally to other mucous membranes in the form of injections and eye-waters; occasionally to the mucous membrane of the nares through impure snuff; and, lastly, it may be applied simply to the skin. As to the power of the skin, when uninjured, to absorb lead, some doubts have existed. It was altogether denied by Tanquerel des Planches; but, nevertheless, there are many facts which seem to favor the idea that it may be thus introduced. The recent common use of hair dyes, which very constantly contain a large amount of lead, has appeared to cause in many patients effects which probably are dependent on the absorption of this metal; and although in such cases the action at any one time is exceedingly slight, still the use of such dyes is generally continued for a length of time, and may thus slowly lead to injurious consequences. I have certainly met with patients suffering from headaches and other uneasy feelings who have ceased to experience these symptoms when they have given up the application of the dyes to their hair. I have examined several of these dyes, and found that they consist of a solution of acetate, or sugar of lead, with a considerable quantity of suspended sulphur. This latter substance, when the fluid containing it is at rest, forms a pale-yellowish deposit in the bottle.

We will now turn our attention to some of the more important causes of lead-poisoning. It will be seen that the cases selected from the hospital books for discussion in our next lecture have been, for the most part, of workmen who have had to use preparations of lead in their occupations; but saturnine poisoning is by no means confined to them, and both in private and hospital practice it is not unusual to find individuals of the middle and upper classes suffering from lead contained in the water, beer, and wine which they drink, and even in some of the other luxuries which they indulge in. Among workmen those who are more commonly affected are men engaged in white-lead mills; makers of minium, or red lead, or of any other preparation of the metal; house painters; plumbers; illuminators in water colors, using white lead and tipping the brushes in the mouth; composition doll makers; organ-pipe makers; engineers, using lead, and many others—in fact, anyone handling lead, or inhaling the dust, is liable to be thus affected, unless due precautions are taken to prevent the injurious effects of the metal.

In other classes of society lead-poisoning is usually caused by salts of lead prescribed as medicines, or liquids contaminated by the metal. Some years ago, during an epidemic of Asiatic cholera, it was a common practice to administer sugar of lead in cholerine or the diarrhœa then present. Many patients took this medicine for so long a period as to induce severe attacks of saturnine colic. I have never met with the more advanced forms of lead-poisoning from the medicinal use of the metal, although there are some cases on record.

At one time a form of colic was common in Devonshire and other cider districts, called Devonshire colic. It was at first supposed to be due to the cider itself, but on subsequent investigation was found that the cider in such cases was not pure, but contained a considerable amount of lead derived from the presses, cisterns, and other vessels employed in its manufacture and preservation. Wine, ale, beer, and porter have been also known to produce lead disease, and one of our hospital cases affords a good illustration of the fact.

The daily use of drinking-water containing traces of lead dissolved or suspended in it is a very frequent cause of disease, and one to which attention has hitherto been scarcely sufficiently directed. Some of you may probably have heard of the cases of lead-poisoning which occurred at Claremont during the time that the late ex-king Louis Phillippe lived there with his family. Thirteen members of the household were attacked with colic more or less severe, and it was ascertained that the mischief was due to the presence of lead arising from the leaden pipes and leaden cistern used in conveying the water from the spring. Chemical analysis failed to detect any trace of the metal in the water in the iron reservoir at the spring, but as much as one grain per gallon was found in the water of the cistern. The water was otherwise very pure, and contained but a small amount of saline constituents. It is found that soft water has more solvent power for lead than water which is rendered hard by the presence of carbonates and sulphates. In a case in my own practice I was led on first seeing the patient to the idea of water-poisoning, which proved to be correct, from having a few minutes before washed my hands in the water and noticed its peculiar softness. It may be taken for granted, as the result of many observations and experiments, that distilled and rain water, also water containing certain salts, as nitrates, arising from the decomposition of animal and vegetable matter, when kept in leaden vessels or passing through leaden pipes, is apt to become strongly impregnated with the metal. The carbonate and sulphate of lead formed from hard water, when deposited as a crust on the surface of the pipes and cisterns, act as a good protection; but if the pipes are alternately dry and wet, they become very dangerous conductors of water destined for drinking purposes. I am fully of opinion that cases in which slight saturnine effects are present are more common than generally supposed. It must be remembered—and it is a point upon which I shall afterwards dwell—that some individuals are distinctly affected by water so little impure as to produce no appreciable symptoms in others.

I have now to bring before you a remarkable case of lead-poisoning which has, within the last two years, come under my care; it is one which is most instructive, and the discovery of its cause has, I believe, already proved of service to the profession.

A gentleman who had lived in Calcutta for many years returned to England in broken health, and great fears as to his recovery were entertained. Shortly after his arrival there was a consultation on his case between Dr. J. Macpherson and myself. We found the patient looking extremely pale and sallow; a loud venous murmur was heard in the neck; he was able to walk, although extremely weak and tottering, but had no power of raising his arms from his sides. On close examination the muscles of both arms and forearms, as likewise those of the shoulders, were found extremely emaciated, the biceps being literally little more than a cord; the flexors of the thumbs were also withered; the tongue was pale; the digestion very weak, and the bowels torpid. The mental faculties were intact. At first the idea of progressive muscular atrophy suggested itself, but after a few minutes, when the gums were examined, we became almost certain that lead impregnation was at the bottom of the case. Then arose the question, how could such poisoning have taken place? He had just arrived from India by the overland route, and had been under different circumstances in regard to solid and liquid food, and had had no known contact with lead in any form. Knowing him to have been a great snuff-taker, the snuff became a subject of inquiry. He had long used a moist rappee, made in England, but purchased in Calcutta. His stock, however, had become exhausted at Aden, and he was of opinion, on reflection, that from that time his symptoms had somewhat improved. Seeing that the gums exhibited a well-marked blue line, and that the symptoms were such as arise from the action of lead upon the system, we had no hesitation in directing our treatment to the elimination of lead, to the restoration of the blood, and to the strengthening of the wasted muscles. Our efforts, we were happy to find, were shortly crowned with marked success, and in no long time this patient found himself so much improved in health as to be able to resume his ordinary avocations.

The question of the snuff was not neglected. Six packets were telegraphed for from Calcutta, and arrived in due time; three are now in my possession. These I have carefully examined, and will give you the results. The samples were in cases made entirely of lead, the metal being about one-sixteenth of an inch in thickness. A small quantity—about twenty grains—was removed from one of the packets and burnt in a platinum dish. A black residue was obtained, which was rendered colorless by being afterwards incinerated with nitric acid. The white ash was then treated with dilute acetic acid, and the solution filtered and evaporated until it became very concentrated. When a crystal of iodide of potassium was dropped into a small quantity of the fluid, it was at once surrounded by a bright-yellow ring; and when the salt dissolved, the whole solution was turbid and of the same yellow color. When slightly diluted with water and heated, the turbidity and color disappeared; but, on cooling, the beautiful and characteristic orange-colored spangles of iodide of lead were formed. Another portion of the concentrated liquor, when treated with sulphuretted hydrogen, produced a black precipitate of the sulphide of lead. All three samples of the snuff, which were of different ages, exhibited the presence of lead in very large quantities. A subsequent careful examination made with the microscope revealed the existence of lead. Very close inspection with the unaided eye was sufficient to detect the presence of very minute white points extending through the whole of the snuff; these were more numerous near the walls of the case, but were discovered even in the centre of the mass; and these points were shown by the microscope to consist of mammillated crystalline particles, which dissolved readily in acetic acid, with the evolution of carbonic acid. The number of white points increased after the boxes were opened, from the further crystallisation of carbonate of lead.

It appears from these observations that when moist snuff is enclosed in leaden cases, and especially when subjected to the action of heat in tropical climates, it becomes intensely poisonous from impregnation with the metal. The explanation is probably as follows: The moisture of the snuff, containing certain of its soluble salts, is evaporated and condensed on the sides of the leaden case, and acts slowly on the metallic walls; a gradual interchange takes place between the interior and exterior portions of the liquid, and thus the whole mass becomes infiltrated with the lead salt, which afterwards becomes crystalline. When these facts are made generally know, it will be incumbent on the manufacturers and vendors of snuff to avoid the employment of lead cases in packing it, more especially if the snuff be intended for hot climates; and on snuff-takers to shun the use of snuff thus packed. Within the last year I have been informed by a physician practising at Calcutta that since the above case came under notice, other patients have been found to be suffering from similar symptoms produced by the use of the same snuff.

ABSTRACT
OF
Clinical Lectures
ON
SURGERY.

Delivered at Guy's Hospital during the Session 1871-72.

By J. Cooper Forster, M.B. Lond., F.R.C.S.,

Surgeon and Lecturer on Surgery.

LECTURE I.

ON EXCISION OF THE ELBOW.

Gentlemen,—I propose to-day at our clinical lecture to remove a piece of bone from the left elbow of a patient, whose joint I excised some time back, after having previously performed a similar operation on her right arm. You may, perhaps, say: "What have you to tell us about the mere removal of dead bone?" In reply to your question, I tell you that this patient has now been under my care for some years; and that in her case I have defied the rules laid down in all your text-books concerning excision of the joints. And now that she is under the influence of chloroform, I want you for a moment to look with me at her condition, especially with respect to her right arm. This limb, as you may see, is a most perfect *flail*, the arm representing the one part, the forearm and hand the other, and the long interval of skin and soft tissues between the bone taking the place of the connecting thong. Many surgeons would say that the operation, with such a limb as the result, was a failure; but mark you, if the arm is supported she has a very useful hand—aye, a perfect one, for she can write and sew, and on the strength of this I say it is a great success. Unfortunately, this poor girl has had disease of the other elbow; and this also has been excised. The necrosed bone which necessitates an operation to-day is in the region of the old disease. I shall do nothing more than take it away; for, with the other arm as a pattern, nothing would induce me to amputate this one, or even to re-excise. [The operation was then proceeded with, and some small pieces of dead bone removed with the forceps and gouge. A fine carbolic spray was directed into the wound from the first entrance of the knife till the termination of the operation; the parts were then covered with protective and carbolised muslin, and the patient removed. The lecturer then read this report of the case, in which several points of importance occur.]

Louisa P——, aged twenty-three, a domestic servant, was admitted to my female ward, Charity, on the 9th of October last for the *fourth* time. Her parents were healthy people, now dead, and she knows of no hereditary tendency to rheumatism or gout. Thirteen years ago, when only ten years of age, she had rheumatic fever. She was only ill four weeks, and during that time both elbows were painful though not swollen. She quite recovered from that illness, and kept well for five years. Eight years ago she again had pain in both her elbows, and splints were applied to them for three months, at the expiration of which time the *left* felt quite well, and the right was improved only. Seven years ago the *right* elbow was so painful that she left her situation. In December, 1866, an abscess formed in the right elbow; this was opened and healed again, the pain still continuing.

When first admitted to Guy's in April, 1867, the right elbow was red, swollen, and painful, with several abscesses in the neighborhood, one of which was opened. Tonics, sedatives and stimulants were given, the arm was kept on a splint, and matter was evacuated from time to time. Getting worse, excision of the right elbow-joint was performed in July, 1867 (rather more than four years ago). The operation of excision was decided upon as being much better for the patient than amputation, the only other alternative, even though there was no possibility of obtaining a firm fibrous union between the arm and forearm after the extensive removal of bone necessitated by the disease. The operation was performed by a straight incision over the back of the joint, and all the diseased parts were removed, including about *two inches* of the humerus. She left the hospital fifteen weeks after the operation with the wound healed, except one sinus. She had slight motion in the joint, and a leathern splint was applied to give it support. This splint, as you see, is moulded to the outer side of the arm, from the shoulder to the wrist, leaving the hand free. The deltoid, scapula, and scapular half of the clavicle are embraced by its upper part, which forms a shoulder-cap, and this is kept in close contact with the parts by a strap which goes across the chest, under the opposite axilla, and buckles to the back part of the hood. The leather is moulded so as to surround the arm from the insertion of the deltoid nearly to the elbow, and again below the joint to the wrist. It is rectangular, so that the forearm is kept at a right angle with the arm.

After leaving the hospital, she went to Margate for five months, and while there an abscess formed near the left elbow. After this she went back to her situation and to work.

From this time the right arm troubles us no more, but the left comes prominently under notice. In March, 1869, it became stiff, followed by pain and inflammation. She kept at work till April, and was then readmitted at Guy's. The joint was then uniformly enlarged, and painful when moved; the skin was normal, but fluctuation beneath was felt; motion limited, the limb being bent at a right angle.

The condition of the right elbow as recorded at this time is well worthy of your attention (twenty-two months after excision of the joint, with removal of two inches of the shaft of the humerus). It is as follows:—Bones not in contact, humerus ending in a blunt point; end of radius not to be felt; ulna enlarged. When unsupported the forearm dangles uselessly, the muscles drawing the bones somewhat together, but she has no power of flexion, extension, or rotation. *When the elbow is fixed in the rectangular splint, all the movements of the hand and fingers are good, and she can do needlework and write well.*

The left arm went from bad to worse, though kept perfectly at rest on a splint. Abscesses formed in and around the joint, which became quite disorganised. Excision was, therefore, resorted to on November 2nd, 1869. On this side not much more than the articular surfaces were removed. The arm was kept on a splint, and she progressed fairly well, leaving the hospital after eleven weeks' stay, with good use of her fingers. She now went to Margate, had erysipelas there, and was readmitted to Guy's in August, 1870. Some dead bone was removed under chloroform from the ends of the bones. Eight days after this she again had erysipelas. She left Guy's on Nov. 26th, with the wound nearly healed, and wearing an anterior leather splint, which kept the joint bent rather within the right angle.

She has lately come under notice again. Her health has much improved since last we saw her, though the sinus has never healed, and bare bone can be felt at the bottom of it.

Nov. 11th, 1871.—Her present condition may be thus described. The left forearm is flexed to form a right angle with the humerus, and the patient is very careful to maintain this position, and declares herself unable to extend it further. There is a very slight power of further flexion, though not much. The hindrance to flexion and extension is pain at the elbow, and along the back of the forearm, apparently from some dragging on the extensor tendons, which also hinders her from completely closing her fingers on the palm of the hand. She has very fair motion, though, of all the fingers, and perfect mobility of the thumb in all its movements. The forearm is kept in a position midway between pronation and supination, and there is no rotatory movement of the radius, either voluntary or by the effort of the surgeon. The union between the arm and forearm is of a fibrous nature, no appreciable space existing between the humerous and ulna. She cannot at present use this arm either in dressing or eating.

Now, in reviewing this long but very interesting case, many points of practical interest come before us. Let us take only two or three of them. And, first, what can be assigned as a cause for this disease of both elbows?—I mean, of course, the disease which the report of the case tells us began in 1863. It is quite out of our power, as you know, to suggest any reason for the attack of rheumatic fever; but why eight years ago she had simultaneous pain and, apparently, disease commence in both her elbows is a question that I think we may legitimately enter upon. Looking back to the history, we find that great stress is laid, and I think rightly so, upon the pain that occurred in both joints during her attack of acute rheumatism. Taking the two illnesses, then, as bearing some relation to each other, we see in this only another instance of the great fact, of which many illustrations might be given, and which you may always take for granted and expect, that when once a part of the body has been subject to disease, it is likely to be so again; and in any debilitated condition, or state of generally impared nutrition, other things being equal, that formerly diseased part will be the first to suffer, even though to all outward appearance it may have been restored to a normal state. Patients, then, are predisposed to disease in certain parts; and it comes very much to this, that, once having had any severe illness, we carry about in ourselves some weak spot which must be kept carefully guarded, else the seed which has been sown may at any time produce serious results, and may even terminate life. For instance, a man shall get inflammation of his peritoneum and recover, some adhesions forming as the result of such peritonitis. He will go about apparently well, and twenty years after, it may be, a little fresh inflammation shall cause an abscess, or perhaps some strangulation of the bowel, which may bring about his death. Considering these things, I am disposed to regard the rheumatism as an important feature in the history of this case, and to give it credit for a share in the production of the serious joint lesions which afterwards resulted. You should also note, in connexion with this disease of spoiled tissues, that one of the joints got quite well of the first attack, while the other proceeded slowly on to disorganisation. Why this should have been I cannot say; possibly one side may have been more actively treated or kept more perfectly at rest. I have no data upon which to form an opinion.

The next point I would wish to dwell upon is, that from the first appearance of disease in the right elbow to the date of performance of excision a period of three years intervened—a fact which, when considering the expediency of an operation, would, to many, put excision quite out of court, it being asserted by some that the longer this operation is deferred the greater the certainty of an after-amputation being necessitated. Excision is said to be a measure for early disease of the joint—amputation a last resource. Now this rule implies a limitation of the operation to early disease, and from this I strongly dissent. I do not call in question the soundness of the doctrine that it is better to excise early; on the contrary, as I shall tell you, or you may have noticed from the report, in the other arm I operated at a much earlier period of the disease, and should no doubt have done so with this arm had the patient come sooner under observation; but that it is an operation which is *not* applicable to cases of extensive disease of the upper limb—to this I object most forcibly; and I tell you it is hardly putting the case too strongly when I say that in disease of the joints and bones of the arm you *never* ought to amputate. At any rate, I may say this, that I scarcely know a case of disease of the joint which would induce me to sacrifice the *upper* limb, if by any chance it could be left to the patient and made useful by any kind of splint. Look for yourselves at this girl, and tell me if you would not rather have, in your own case, a flail like this than nothing at all. For my own part, I could not make up my mind to lose that hand; and mark this—if you can save a thumb, an index, or even a little finger, and make that useful, you do more for a man than will the most elaborate contrivance of the ablest mechanician. Let me, then, repeat, and ask you to carry out in the practice of your after-life, this rule: never sacrifice an arm by amputation if after any lapse of time even a single finger, much more a thumb, may be reasonably expected to prove useful. In the lower limb, unless perfect bony union can be obtained, you had better not excise; but in shoulder, elbow, wrist, or finger joint, take away any quantity of bone, but leave the member. This patient's right arm is, as you have heard, not of much use without the splint—that is to say, she can neither flex nor raise it up, though she can write; but I may tell you that when it is thus dangling helplessly by her side, if you put your hand into hers, you will find she can grip you tightly; it must therefore be of considerable use in the act of carrying. But I would especially ask you to look carefully at the support she wears, seeing that on its proper adaptation to the parts depend in great measure the more delicate movements of the hand. How perfect these are I need not insist on further than by passing round the class a letter she lately wrote to me, the handwriting of which, I am sorry to say, may put some of us to shame. During the year 1868 she was free from any pain or disease, and did household work; and then she came under my care with disease of the left elbow. Probably, in her station in life, her left arm was inordinately worked, and the old disease was rekindled. With the right arm three years passed by before excision was done, but in this case disease had only lasted nine months. I had the experience of the former operation in her case to guide me, and thus, knowing the condition of the girl and the extent of bone that had to be taken away, it was clear that what was necessary in the way of operation should be done quickly. As soon, then, as I knew the joint to be suppurating and disorganised I excised it, hoping by this means, less bone being taken away, she might get a firmer union between the bones, and do without any mechanical aid. But you will notice that though the duration of the disease was not long, we have had a good deal of trouble with the case. This is the second time she has been admitted for the removal of dead bone. This may be owing to one of three reasons. In my endeavor to save bone at the first operation, I may possibly have not taken away enough. This I do not think, however, was the case, as I had the advice of my colleagues at the time, and, in their opinion as well as my own, healthy bone-surface was left. Again, the erysipelas with which she was attacked after the operation may have caused necrosis: this is not unfrequent. Or the parts may have been used too freely before their perfect consolidation and vascularisation. A case illustrative of the latter idea is now in Luke ward under my care. This man, whom I show to you with disease of the elbow, is a draper. His left elbow-joint was excised here a year ago. He went out well, and has appeared several times since to show what a perfect arm he had, and how he could use it. I then gave him leave to do without his splint, and within three months he came back with two sinuses about the parts. He had taxed the limb beyond what it would bear or what I had given him authority to do, in lifting heavy bales as much as half a hundredweight. He thus proved the success of the operation, and produced inflammation in the new tissues which have broken down; and he will now have to wait in the hospital some time, before —by a period of rest—the arm can be used. The same explanation, I take it, applies in the case of the girl: belonging to the poorer class, she was obliged to work, and you see the consequences. There is partial movement between the humerus and ulna; but this is not yet as perfect as usual. It is essential to the success of the operation that there should be good movement. How this is effected is the last point I shall touch upon to-day. Now the articular surfaces of the bones have been excised, so there cannot be any joint. It is questionable whether anything like a synovial membrane ever forms between the bones. The condition which exists is that which you recognise under the name "fibrous anchylosis." This is produced by the formation, in the first place, of a large quantity of inflammatory lymph, its development being modified according to the nature of the tissues amongst which it is formed. In this case it finds its way amongst ligamentous and fibrous tissues, and thus becomes developed into fibre itself. This, at first gelatinous and lacerable, gradually contracts into firm tissue, uniting the ends of the bones, holding them together, and also keeping muscles, tendons, &c., all in position. In the most favorable cases about the same amount of motion occurs as in the healthy joint; but in some cases much more. Too much movement, however, I think not desirable.

The interesting features of this case, gentlemen, are not exhausted by my touching on two or three of them; but the others I must leave you to think over for yourselves in connexion with remarks I make to you when seeing the patient in the wards.

Clinical Lecture

ON

A CASE OF RENAL DISEASE ENDING IN APOPLEXY.

By W. H. Broadbent, M.D., F.R.C.P.,

Physician to St. Mary's Hospital.

Gentlemen,—The anatomical pieces before you tell a tale to the educated eye far more eloquent and impressive than any I can shape into words. There are the kidneys; there is the heart; there is the brain. The kidneys diminished in size—one especially so,—rough and granular on the surface, with here and there a small cyst, and damaged in their intimate structure. The heart enormously enlarged, weighing 19 oz. instead of 9 oz. or 10 oz., —the walls of the left ventricle greatly thickened; and this change not accounted for by any alterations of the valves, which are quite healthy and competent. The brain with a large ragged cavity in the right hemisphere, from which a great blood-clot has been extracted. To complete the chain of causation, we ought to have under the microscope, between the kidneys and the heart, and again between the heart and the brain, some of the minute arterioles from various parts, showing the muscular hypertrophy of their coats, demonstrated and explained so admirably by Dr. George Johnson. The successive links of the deadly chain would then run thus:—Disease of the kidneys gave rise to accumulation within the blood of excretory materials which these organs should have eliminated; the circulation of the blood, thus rendered impure, was resisted by the stopcock action of the muscular coat of the minute arteries for the defence of the tissues; the heart, feeling this resistance, and not recognising its conservative character, began to act with increased vigor to overcome it and carry on the circulation; while the arteries, on the other hand, contracted all the more energetically: and so, in obedience to the law that increased exercise of function by muscular fibre induces hypertrophy of structure, heart and arteries, in their strife one against the other, became hypertrophied. But when the heart is acting with increased power on the one hand, and the minute arteries are continuing their resistance on the other, the blood is at high pressure within the vessels; and if there is a weak spot anywhere the vessel will be in danger of bursting. This is what has happened in the present case, the weak point having been found in the small vessels penetrating the brain-substance from the middle cerebral artery in the posterior part of the Sylvian fissure.

The poor woman from whose body these morbid specimens were taken was admitted under my care in September. She was for a time in the Alexandra ward, more recently in No. 2 bed in the Victoria ward, where some of you have repeatedly seen her with me, little anticipating from her appearance the sudden fate which has overtaken her. Her illness, according to her own account, dates from an attack of hemiplegia of the left side two years since, while pregnant. This seems to have been severe, as it was six months before she was able to walk about again. She got over her confinement, however without any difficulty or complication. After regaining a fair state of health, she had within the last few months been complaining of weakness, loss of appetite, imperfect digestion of her food, discomfort after eating, &c.

She did not look particularly ill, and she presented no appearance suggestive of any special disease. Very slight traces of her old paralysis remained. Investigating the causes of her complaints, we came at once upon the fact that the urine contained albumen. This we might have suspected from the occurrence of hemiplegia during pregnance. The amount of albumen varied considerably; there was never much, and sometimes it was absent altogether. The urine was pale, not perfectly clear, acid in reaction, and of a specific gravity of 1018, as a rule—never, so far as I know, below 1015—sometimes as high as 1020. Now this is a high specific gravity for the state of kidney we supposed to exist—contracted granular kidney,—and at times cast a momentary doubt on the diagnosis. I account for it now by the fact that in one kidney the morbid change was still in an early stage. We obtained little microscopic evidence as to the kidney mischief—a few particles of damaged renal epithelium, and rare and questionable fragments of small granular casts. There was no trace of dropsy or œdema, and never had been; but in this particular affection of the kidneys we do not expect it, and, as I have said, there was no marked renal look.

But we had evidence from another source in favor of our diagnosis—the indirect evidence of the vascular system. I frequently pointed out to you the peculiar character of the pulse. On asking a student to describe it, the invariable report was that it was weak; and to the fingers placed lightly on the vessel that was the impression conveyed; there was, in effect, very little pulsatile movement in the artery. But then it was very large and very hard, and the vessel slipped and rolled about under the finger like a bit of thick whipcord, reminding one of the vas deferens among the structures of the spermatic cord, only much larger; and when it was attempted to control the pulsation by pressure with the finger nearest the heart, it was found to be a very difficult matter. Press as one would almost, the pulsation could still be felt by the two fingers on the distal part of the vessel. The first impression of weakness was thus at once removed, and the pulse recognised to be extremely strong; it was the long, hard, large pulse of high arterial tension, which gives a sphygmographic trace of gradual ascent, low elevation, rounded summit, and gentle, unbroken fall.

Then as to the heart; the area of dulness over it was extensive; its impulse was a powerful and widespread heave; the apex-beat was conspicuously evident to both eye and hand below and to the left of its normal situation. This was of course evidence of considerable hypertrophy. A mitral murmur was reported to me by my resident medical officer when the patient was first admitted, but I did not succeed in hearing it: I thought I distinguished once or twice a smooth, feeble, systolic, tricuspid murmur; and a hæmic pulmonary murmur could sometimes be heard. I attached no importance to any murmur found to be present; but modifications of the heart-sounds of extreme interest were detected—namely, reduplication of the first sound over the inter-ventricular septum, extinction of the first sound and exaggeration of the second over the aorta. These have recently been beautifully explained as phenomena of high blood-pressure by Dr. Sibson. To distinguish the reduplication you must either have the double stethoscope and place one mouth over the right ventricle and the other over the left, or the common stethoscope must be planted over the septum between the ventricles. At the apex or over the sternum the first sound is pure and single; it is only when the sound given out by both the ventricles in their contraction is conveyed to the ear that a doubled sound is heard. This alone would suggest what is clearly brought out by manœuvring with the double stethoscope—namely, that the reduplication is due to the want of exact synchronism in the contraction of the two ventricles: that of the left is retarded by the extreme resistance to the entry of more blood into the already distended arteries, and thus the right gets a slight start. Each ventricle produces sound by its systole; and when, from any cause, the normal coincidence of the two is disturbed, the first sound of the heart, which is compounded out of the two sounds of the ventricles, is reduplicated. The diminution or extinction of the first sound over the aorta, again, is explained by the arterial tension, which prevents the left ventricle from throwing its contents rapidly into the aorta, and renders its action more gradual; while the increased pressure within the aorta, forcing back the semilunar valves with undue violence, causes the second sound to be louder.*

The high arterial tension so obviously indicated by the signs I have mentioned, which I repeatedly pointed out in my visits to the ward, and the cardiac hypertrophy, were corroborative of the diagnosis of contracted granular kidney. Another corroborative sign I look for in vain: I mean the characteristic changes in the retina. My note of the appearances presented by the fundus of the eye on opthalmoscopic examination is as follows:—The fundus of the eye generally seemed paler than usual, the optic disc a little more pink; so that, though the disc was well defined, the difference between it and the surrounding retina was less marked than usual. It was small, had no appearance of elevation, and presented in the centre a white spot from which the vessels issued. The retinal artery seemed smaller than usual. No white spots or hæmorrhages were seen.

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* The tension of the arterial walls is an element in the production of the second sound, but this and other points were not discussed. Nor is what has been said intended as a complete exposition of the relation of the modification of the sounds to the arterial tension; it was rather a reminder to students already familiar with Dr. Sibson's teaching.

The objects to be kept in view in the treatment of a case of this kind are simple and obvious. We cannot hope to undo the structural mischief already done in the kidney; in some cases we may perhaps arrest the morbid process when it is traceable to some known cause, as alcoholic excess &c.; but, generally speaking, what we have to do is to avert as long as possible the consequences—the accumulation of urea or other poisonous substances in the blood, and the loss of strength and deterioration of structure to which the state of blood gives rise. The bowels must be kept open, the action of the skin must be encouraged, so as to secure whatever vicarious elimination their excretions may afford; the diet must be regulated and digestion aided; and it is generally beneficial to give the tinct. ferri muriatis. This was the line of treatment we followed, and our patient seemed decidedly better; she was relieved of the symptoms which caused her to seek admission; the reduplication of the first sound of the heart was no longer heard; the artery at the wrist was not so full and hard; once, indeed, for a few days during the brief pyrexia the peculiar character of the pulse was lost altogether; and it was expected she would soon leave the hospital greatly relieved.

The dangers to which the subjects of this form of Bright's disease are exposed are, on the one hand, uræmic poisoning, so called when the nervous system is effected, or secondary inflammation of important organs, or effusions into the great cavities, from disturbance of the normal relations between blood and tissue; or, on the other hand, rupture of a vessel in the head as a consequence of the great arterial tension. We had no special reason to anticipate any of these unfavorable effects immediately. The specific gravity of the urine seemed to guarantee the elimination of a fair proportion of the urinary constituents; the patient had not the well-known renal look; she was not old; there were no advanced retinal changes, and the arterial tension was reduced. There was, however, an element of danger, of which I was not for some time aware. The woman was five or six months pregnant. She had stated all the time she was under observation that the catamenia were regular, and nothing had occurred to lead to a physical examination of the abdomen till shortly before her death, when she mentioned some uneasy sensation in the hypogastrium. I proceeded to see if any cause for this was to be found, and, coming upon the uterus, I said at once, "Why you are pregnant." This the patient strenuously denied, repeating her statement that she had been quite regular, and was, in fact, menstruating at the time; saying, also, that the tumour I felt had been there for some time. She was a married woman, and had no reason for concealing her pregnancy, unless, as it has occurred to me since, she had apprehensions of danger attending it, which made her wish to blind herself to its existence; and I accepted her denial *pro tem.*, with the intention of making further investigation when the catamenial period was over. Now, without saying at all that the pregnancy was an important factor in the causation of the hæmorrhage, the coexistence of pregnancy and albuminuria is a serious matter, which, had it been fully known to me, would have led me to look on the immediate future of the patient with more apprehension. But this is a point into which I cannot enter fully now, and we must come to the fatal attack, which was preceded, and perhaps determined, by two or three days of more than usually obstinate constipation.

At about 3.30 on the morning of Thursday, November 30th, she was apparently as well as usual, and asked the night-nurse for drink. A few minutes later her right leg and hand were knocking against a screen which stood by her bedside. When the nurse went to her, the right limbs were thrown about, but more as if the movements were voluntary than convulsive, while the left limbs were rigid, contorted, and twitching; the face was at first distorted. By the time the sister of the ward and the resident medical officer reached her the movements of the right side were less violent; the face was equal on the two sides, but the rigidity of the left limbs was the same; the right pupil was greatly dilated, the left much contracted. The patient recognised both, and gave their names, but she could give no account of herself, or of what she felt. Later she was more quiet, but from time to time tossed off the clothes with the right hand or leg, and rubbed her head with this hand, while the left limbs remained rigid and powerless, and were at intervals shaken by a peculiar tremor; the left hand was powerfully pronated, and the fingers and wrist flexed; she resisted when the eyes were opened, seemed to resent disturbance, evidently felt a pinch on the right side, but gave no indications of sensation on the left half of the body; she swallowed fluids pretty well; no signs of consciousness beyond those first mentioned could be elicited. Soon after 3 P.M. convulsive movements again came on, and she died almost instantly. The tongue was bitten.

These details are chiefly interesting in connexion with the seat and extent of the hæmorrhage. The clot lay to the outer side of the right corpus striatum and thalamus, and had torn through the fibres passing from them into the hemisphere. It extended along nearly the whole length of the ganglia, but did not turn round the anterior end of the corpus striatum. Its upper part was on a level with the intra-ventricular surface of these bodies. Inferiorly it was separated by a very thin stratum of nervous matter from the fissure of Sylvius, and, in my opinion, the source of the bleeding was the small branches of the middle cerebral artery here penetrating the brain-substance. Now, my interpretation of rigidity in the paralysed parts at the onset of apopletic hemiplegia is injury near, but not extensively or deeply involving, the motor ganglia or tract. Destructive lesion of the motor apparatus appears to leave the paralysed limbs relaxed; pressure upon, or "irritation" of it, while its cells and fibres are still in continuity, appears to me to render them rigid. This view of mine may, of course, at any time be overthrown by facts inconsistent with it; but, at any rate, it has the support of this case, and I could give you many others.

One question remains: "Could anything have been done to postpone the fatal issue when once the rupture of the vessel had occurred?" I do not know, but had I seen the patient at once I should have bled her, simply on the ground of the antecedently known high arterial tension. We can all see now that, at least, this would have done no harm. You will not, however, from this statement infer that I recommend or practise venesection in apparent apoplexy as a rule; the very contrary is the case, and on some future occasion I hope to show you reasons for this.

Looking once more at the morbid specimens, and taking a backward glance at the history of an attack of hemiplegia in a previous pregnancy, at the comparatively slight evidence of kidney disease offered by the urine and general appearance of the patient, and at the assistance to a correct diagnosis and warning as to the kind of danger to be apprehended afforded by attention to the signs of arterial tension in the pulse and in the modifications of the heart sounds, and remembering also, as I always shall, the existence of pregnancy at the time of the final apoplexy, you will find the case full of interest and instruction.

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ABSTRACT

OF A

Clinical Lecture

ON

GONORRHŒAL RHEUMATISM.

Delivered at the Middlesex Hospital, Oct. 9th, 1871,

By T. W. Nunn, F.R.C.S.,

Surgeon to the Hospital.

Gentlemen,—From time to time during the past session or two you may have observed in the wards cases which were admitted under the name of Gonorrhœal Rheumatism, while perhaps some of you may remember that I expressed in my clinique doubts as to the existence of such a disease as true gonorrhœal rheumatism.

The history of a typical case of so-called gonorrhœal rheumatism is somewhat as follows:—The patient finds himself with a urethral discharge; immediately past circumstances justify a suspicion that he has contracted gonorrhœa; probably three weeks after such discovery, the knee, elbow, or ankle puffs up, and he suffers more or less constitutional disturbance: so lamed, the patient is driven to seek medical advice.

Suppose, now, the same sequence of phenomena to occur three or four times in the same individual, one would naturally say, Here we have cause and effect: in some way or other, clearly, the urethral discharge is the cause of the articular rheumatism. What then is the actual relation between the urethral discharge and the infirmity of the joint? I inquired the other day of a patient, who had been sent to me as laboring under gonorrhœal rheumatism, what he himself took to be the state of affairs. He said, "I imagine that the poison of the gonorrhœa has been absorbed into my blood, and that it is working on my joints." That is a very reasonable idea, for a non-professional person at any rate, to hold, and I dare say is accepted as representing the fact by many practitioners. But against this view it can be urged, firstly, that gonorrhœa is not due to any specific animal poison; and, secondly, that women never, or with very rare exceptions, suffer from gonorrhœal rheumatism, notwithstanding that the extent of mucous surface inflamed is much greater in women than in men in this disease: and there is nothing in the difference of sex that would prevent a specific animal poison from affecting equally either sex. This immunity of the female gives us, I think, the clue to the nature of the disease. In the male, the mucous membrane inflamed is a part of the urinary as well as of the genital system, and is in direct connexion with the kidney; and so there is no necessity for diverging into a discussion on the specificity or non-specificity of gonorrhœal inflammation, since we can rationally infer that the functions of the kidney would be liable to disturbance by a morbid action at the distal segment of the genito-urinary canal, from the known sympathy of the various portions of the same system with each other.

To return to facts. I will give you a history from my private note-book—and I take it that the object of clinical lectures is that the student shall have the advantage of all the experience of the lecturer. It is the history of a patient who applied to me in a state of great trepidation, believing that he had symptoms of incipient gonorrhœa. He had good reason to be anxious, for on three occasions he had been laid up with so-called gonorrhœal rheumatism. At the end of the year 1862 he contrasted gonorrhœa. In January, 1863, three weeks afterwards, he was crippled with rheumatism in the foot and ankle, which disabled him for three months. I wish you to observe that in this case the ankle was the joint affected, because you will find it stated that the knee is the joint most obnoxious to this form of rheumatic inflammation, as though this form of rheumatism had a special gravitation to the knee-joint. In the spring of 1864 this gentleman again had gonorrhœa, and again, after three weeks, had rheumatism in the foot. For a third time he had gonorrhœa in 1866, and again, three weeks after, rheumatism showed itself in the foot, and then in the hip, and, finally, in the knee; thus, at last, the knee-joint participated in the *synovial* disturbance. It fortunately happened, I may mention, that the alarm of 1868, the date of his application to me, was groundless. Now here is an important circumstance to be noted. The brother of this patient was equally liable to synovial inflammation under similar conditions; and we shall presently see why this circumstance is important.

In this case, then, in the course of four or five years the patient had three attacks of gonorrhœa and three attacks of rheumatism, in each instance the rheumatism coming on within about three weeks after the commencement of the urethral discharge. Without question, I think, this is a case of rheumatism depending for its exciting cause upon an inflammatory condition of the urethra, that inflammatory condition being gonorrhœal. However, it is not uncommon to meet with cases of urethritis originating in a rheumatic or a gouty state of the system, the urethral discharge being merely one of the rheumatic or gouty phenomena. For instance, I have before my mind's eye a case in which I have repeatedly observed that the earliest symptom of an attack of gout is a purulent urethral discharge, accompanied by scalding in passing water and chordee. Unless complete and absolute confidence could be placed in the statements of the patient, it would be quite consistent with what we ordinarily see to conclude that he had gonorrhœa. Thus we have an easy path to an erroneous diagnosis; and I have no doubt that it has been owing to the confounding of cases like the first described with cases like this that such indefinite opinions have been promulgated. Mr. Athol Johnson,* for example, says that "synovial inflammation is occasionally met with in connexion with purulent inflammation of the urethra, or even with purulent ophthalmia. This complication appears to have been first described by Sir Benjamin Brodie, and usually now passes under the name of gonorrhœal rheumatism, though it differs from rheumatism, and is not necessarily connected with gonorrhœa."......"All stages of gonorrhœa may present this complication, and usually no material change in the discharge is noticed, though sometimes it declines a little when the synovial affection shows itself." I refer you to the book named for fear I should be doing Mr. Athol Johnson injustice. I cannot, however, omit adding that gonorrhœal rheumatism was described by Swediaur at least twenty years before the pulication of Sir Benjamin Brodie's first book on Affections of the Joints.†

Now I will give you illustrations of the sympathy of the urethral mucous membrane with the rest of the system. It very frequently, almost as a rule, occurs that the first passage of a bougie is followed by faintness. The simple passing of a bougie or catheter may be followed by a rigor; within twelve or twenty-four hours the patient on whom the bougie or catheter has been passed has a shivering fit, which is succeeded by other symptoms of feverishness. This occurrence, by the way, you will find very annoying and perplexing; it however usually leads to nothing more serious, but I have met with instances in which it initiated a troublesome illness.

Here is another almost parallel example. Gonorrhœa, as you are well aware, is liable to be followed by orchitis; inflammation of the urethra from other causes may also induce orchitis, and even the simple passing of a bougie will occasionally do the same; it being remembered, however, that the bougie or other instrument would not have been used unless there had been already existing some indication for its employment in the condition of the genito-urinary track. Besides the faintness at the moment of the passing of a bougie—the intense rigor coming on some hours after the same operation—and the acute inflammatory phenomena in the testis just described, we meet sometimes with instances of urethral irritation giving rise to symptoms of intermittent fever. Several years since my friend, Mr. Henry Smith, of King's College Hospital, published a case in which a small abscess, just external to the urethra somewhere near the bulb, was the cause of a train of symptoms having, one might say, an exact resemblance to those of intermittent fever. I have met with similar cases; one was that of a medical man who had been resident in India, but who applied to me on account of a very bad urethral stricture; he had taken large quantities of quinine without effect. Bearing Mr. Smith's case in my memory, I examined the perineum of this patient, and found a small hard knot, which I punctured, letting out not more than half a drachm of thick pus, but with an immediate cessation of the aguish symptoms.

It is, you will find, stated* that gonorrhœal rheumatism more resembles rheumatic gout than ordinary rheumatic synovitis; on the other hand, Dr. Garrod says that there is no such disease as rheumatic gout. Of gout itself, Dr. Garrod says it is "most common in men after the middle age, and in those who have lived freely," and that rheumatism is "at least as frequent in females," and in the young and those of enfeebled powers. What practically seems to me to be the state of the case is as follows:—Rheumatism is due to some fault in the relations of the skin,—gout to some fault in the relations of the gastro-intestinal mucous membrane,—and so-called gonorrhœal rheumatism to some fault in the relations of the urethral mucous membrane,—with the kidney. You must not, however, imagine from my saying this, that I propose to solve the whole difficulties thus with a wave of the hand: far from it. I propose only to give a definite form and shape to an idea of which you must yourselves by future observation test the accuracy. I offer it to you in place of the doubts of Mr. Athol Johnson (1862), and the despair of Dr. Bumstead (1870),† who says, after discussing the cause of gonorrhœal rheumatism, "It will be seen from the above remarks how imperfect is our knowledge of the etiology of this disease, and it would be useless to enter into any farther speculations upon the subject."

I told you that I attached considerable importance to the fact that the brother of the patient whose history I detailed was as liable as the patient himself to gonorrhœal rheumatism. This is the reason why I attach importance to it—namely, that it is presumptive evidence that liability to gonorrhœal rheumatism is a personal pecularity or idiosyncrasy; that idiosyncrasy consisting in inflammatory disturbance of circulation at the distal extremity of the urinary track (the urethra), being accompanied by functional disturbance at the proximal or primary segment of that system (the kidney). Mr. Berkeley Hill,‡ on the authority of Rollet, says that, "Of 2423 persons with gonorrhœa, one in 25 had also acute arthritis." That is to say, 4 per cent., giving out of the total named 96 cases for observation. Mr. Hill, however, declares that "the causes of rheumatoid arthritis are obscure," and that the arthritic inflammation of gonorrhœa has "certain points of difference from rheumatism excited by ordinary causes." I have already pointed out how easily gouty arthritis might in certain individuals be set down for gonorrhœal rheumatism; there is, however, another class of cases which, by being included in lists of cases of gonorrhœal rheumatism, have served to add to the difficulty of arriving at the pathology of the disorder—namely, the cases in which constitutional syphilis is not only a cause of gleet and stricture, but also of rheumatoid symptoms.

Thus then we shall meet with cases (*a*) in which rheumatism is caused by urethritis, (*b*) in which urethritis is caused by rheumatism or gout, and (*c*) in which constitutional syphilis is the cause of both the urethritis and the rheumatism. I take this opportunity of saying a word or two regarding the connexion between constitutional syphilis and urethritis. I endeavored in a paper read before the Medico-Chirurgical Society in June, 1866, to show that one of the causes of obstinate stricture of the urethra—that is to say, adhesive urethritis—was a state of the system due to constitutional syphilis. Curiously enough, such a cause of stricture of the urethra is not mentioned in special works on syphilis or on stricture. Dr. Oppert, for example, in his book on Visceral Syphilis, while including almost every other mucous canal, omits to allude to the urethra in this point of view.

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* Holmes's Surgery, p. 735, vol. iii., first edition.

† See Swediaur's "Traite complet sur les symptomes des Maladies Syphilitiques." Quatrieme edition. Paris. 1801.

* The Pathology and Treatment of Venereal Diseases, including the results of Recent Investigations upon the subject. By Freeman J. Bumstead, M.D. Third Edition, 1870.

† Bumstead, op. cit.

‡ Syphilis and Local Contagious Disorders. 1868.

In the treatment of any case in which we meet with urethral discharge and synovial inflammation coexistent, it is therefore obvious that we must search into the history of the case before any plan can be rationally determined upon, or rationally modified. At the outset of the complaint you will regulate your treatment according to the conclusion come to as to the exciting cause. Thus, if the urethritis arise from gonorrhœa, you will treat the gonorrhœa, leaving the rheumatism, so to speak, to take care of itself. I will not enter upon the wide field of the treatment of gonorrhœa. The out-patient department will afford you ample opportunity for the study of the treatment of this troublesome disorder. If you are wise, you will not neglect the lessons to be derived from an attentive observation of cases even of gonorrhœa. If the urethritis be caused by a gouty or a rheumatic diathesis, you may, as before suggested, easily fall into an error of diagnosis, and prescribe copaiba or cubebs for a patient who has a kidney already damaged by gout. If the urethritis be an accompaniment of constitutional syphilis, it is tolerably certain that a course of mercury in some shape will be required. But in spite of your utmost endeavors, you will occasionally meet with cases that refuse to mend; in fact, further, show an inclination to slip through your fingers from the simple exhaustion induced by the continual pain in the joints. Under any circumstances, the form of joint affections of which I am speaking is apt to be tedious, and it is more than probable that in private practice you will have to meet the suggestion of one of the patient's "disinterested friends." As a matter of experience, I may tell you that "the disinterested friend" is really a formidable enemy to the patient's welfare, for his or her advice, given with a self-confidence consistent only with the recklessness of ignorance, carries with it too frequently the weight of a revelation. As an example of the kind of advice to which I allude, I was told not long ago by a patient who suffers often from gout, and occasionally from gouty urethritis, that during his experience of gout one hundred and one *certain* remedies for the gout had been recommended to him by various friends. On no subject you will find does the non-professional advisor feel so much at home and rampant as on that of "mineral waters." It is safest, however, for *you* to send a patient to a mineral spring with a qualified promise as to the benefit to be gained. My belief, nevertheless, is that Aix-la-Chapelle is especially serviceable in chronic joint affections allied with urethral trouble. I feel bound to tell you, notwithstanding, that the most striking cures effected by the waters of Aix-la-Chapelle that have fallen under my own notice have been where mercurial infriction was combined with the baths, these cases having had a syphilitic basis. On the other hand, I have known sufferers from so-called rheumatic gout combined with stricture, languish at Aix-la-Chapelle, on going to Wildbad spring into a new life.

Original Papers.

CONTRIBUTIONS ON THE PHYSIOLOGY & THERAPEUTICS OF FOOD.

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No. IV.

INTESTINAL DIGESTION.

The nitrogenous matter which has escaped from the stomach undissolved undergoes a further digestion in the intestine. This may be shown by direct experimental observation. And it is not by a continued action of the gastric juice which passes on with the food in its course, but by an action exerted by the secretions poured into the intestine itself. It has been stated that the presence of an acid is an indispensable factor in gastric digestion. The chyme, as it leaves the stomach, is strongly acid. It contains not only nitrogenous matter which has not yet undergone solution, but also gastric juice whose solvent power (it may be inferred) has not yet become exhausted. So far, there are present the conditions requisite for a continuance of the process carried on in the stomach. It happens, however, that on reaching the small intestine the chyme encounters alkaline secretions. The pancreatic juice is to a marked extent alkaline, and so also is the intestinal juice. The bile likewise contains a quantity of alkali in feeble combination, and easily appropriated by the gastric juice. Thus it happens that the chyme becomes more or less neutralised as the small intestine is being traversed. As the result of observation, for instance, I have noticed that by the time the lower part of the ileum is reached, the intestinal contents may be found to present a neutral or even an alkaline reaction.

Through contact, then, with the secretions poured into the intestine, the energy of the unexhausted gastric juice contained in the chyme is destroyed, and whatever solution of nitrogenous food now occurs must be due to another agency.

Let us, therefore, inquire into the effect which the various secretions, as they become incorporated with the chyme, are capable of producing.

First, as regards the intestinal juice. This fluid, it is manifest, has some solvent influence on nitrogenous matter. Bidder and Schmidt ascertained by experiment that meat and coagulated albumen, placed in a muslin bag and introduced into the empty small intestine in which the bile and pancreatic juice were prevented by a ligature from descending, underwent, in from four to six hours, a considerable amount of digestion. In an experiment performed by myself, in which the hind legs of a frog that had been separated from the body were introduced into the empty small intestine and secured by a ligature from the descent of secretions from above, I found, after an interval of six hours, the legs partially digested; a portion of the skin, for example, having been dissolved away, the muscles underneath it separated, and the bones to some extent exposed.

Next, let us take the pancreatic juice. Besides its other offices in the animal economy, this liquid acts upon and dissolves nitrogenous matters, as appears from the following considerations:—

In 1836, Purkinje and Pappenheim held that the pancreas contained a principle capable of exerting digestive action upon the nitrogenised elements of food. This statement attracted little attention, and soon sank, indeed, into oblivion. More recently, M. Lucien Corvisart, of Paris, having reopened the subject, proved by a series of experiments that the pancreas, as one of its functions, supplements the action of the stomach, and, after a copious meal, contributes to digest those nitrogenous matters which have escaped the stomachic digestion. As far as the result is concerned, the nature of the two digestions is alike, each leading to the production of albuminose. While acidity, however, is a necessary condition to digestion in the case of the gastric juice, the pancreatic secretion, he affirms, possesses the power of acting equally well, whatever the reaction that may prevail, whether acid, neutral, or alkaline.

In support of his doctrine Corvisart adduced three sets of experiments:—

First, he said, let the pancreas of an animal be taken when its active principle is at its maximum of quantity and quality—that is, from the fourth to the seventh hour after digestion has set in. Let it then be finely cut up, and infused for an hour in twice its volume of water at a temperature of 20° cent. (68° Fahr.) If the infusion be at once experimented with, it will be found to possess a power of dissolving the nitrogenised principles and converting them into albuminose; and this with no evidence of putrefaction being perceptible if the experiment be stopped at the end of four or five hours, in which time, under a temperature of about 100° Fahr., the pancreatic principle will have effected all that it is capable of.

Secondly, the pancreatic juice obtained during life from the duct of the gland is likewise found to act as a powerful solvent on the nitrogenised alimentary principles if the experiment be conducted *secundum artem;* that is to say, the secretion must be collected for experiment from the fourth to the seventh hour after the ingestion of food, when it is charged with the pancreatic principle to its maximum degree; and also it must be experimented with immediately after collection. According to Corvisart, the pancreatic juice dissolves fibrine more quickly and more largely than albumen. The heat being maintained between 42° and 45° cent. (108° and 113° Fahr.), a specimen of pancreatic juice of ordinary energy dissolves, he says, if the mixture be agitated every quarter of an hour, all that it is capable of dissolving—of fibrine in two or three hours at the most, of solid albumen in four or five hours; the experiment up to this time being attended with no evidence of ordinary decomposition, while at a subsequent period ordinary decomposition is found to set in.

Thirdly, nitrogenised substances introduced into the duodenum when pancreatic juice is flowing into it, are found to be dissolved, notwithstanding that the gastric juice and bile are precluded from entering by ligature of the pylorus and bile-ducts.

This last experiment must not be taken as absolutely conclusive with regard to the individual action of the pancreatic juice. The bile and the gastric juice may, it is true, have been excluded, but it is impossible to exclude the co-operation of the secretions of Brunner's and the other duodenal glands.

My own experiments with the pancreatic juice at first inclined me to think that the effects producible on nitrogenous matter through the agency of the pancreas were rather like those which result from putrefaction than from true digestion. On re-performing the experiments, however, I obtained results which certainly appeared to indicate that some digestive action had been at work. For example, upon operating with the pancreatic infusion, taken according to the instructions of Corvisart, I have found that frogs' hind legs, which, according to my experience, constitute one of the most, if not the most, sensitive and perspicuous tests of digestive action, have, upon some occasions, been softened, so that the flesh has broken down under exceedingly slight pressure, without evidence of ordinary putrefaction being apparent. The effect, however, was not to be compared with what is observed with artificial gastric juice, and ordinary decomposition is very prone to occur, which is not the case in experiments with gastric juice.

The *bile* forms another secretion which becomes incorporated with the alimentary matter after its exit from the stomach. There is nothing, however, to show that this fluid possesses any solvent power over the nitrogenised principles of food.

As regards the intestinal digestion of food, however, the point of interest to us is not whether this or that section poured into the intestine will dissolve nitrogenous matter, but whether nitrogenous matter really undergoes digestion in the intestine.

I have remarked upon the action of the secretions taken individually; but in relation to food the interesting point to us, as I have mentioned, is what occurs within the intestine when all the secretions are allowed to enter. Experiment shows that there is a very powerful solvent action exerted, and, as I can state from personal investigation, a few hours suffice for nitrogenous matter, introduced directly into the upper part of the small intestine, to undergo marked digestion. With reference, therefore, to the digestion of nitrogenous matter, the intestine may undoubtedly be regarded as performing a part supplementary to that of the stomach. Besides its other functions it serves to complete the digestion of whatever nitrogenous alimentary matter may have escaped the digestive action of the stomach, and it may be remarked that the same result—namely, the production of albuminose or peptone—occurs as when the solution has been effected in the stomach.

We have brought the subject up to this point—namely, the conversion of nitrogenous alimentary matter into albuminose; a principle which, from its diffusible nature, can be readily absorbed, and so reach the circulation.

This process is necessary, not only as preparatory to absorption, but also in order to fit the nitrogenous aliment for subsequent application in the system. It cannot absolutely be affirmed that its absorption, without previous conversion into albuminose, would not to any extent occur; but this much is certain, that the amount so absorbed would be very scanty. Let it, however, be artificially introduced into the circulation and evidence can be given that it is not applied in the normal manner.

Bernard first demonstrated that the albumen of egg, reaching the circulation without having previously undergone digestion, quickly passes from the system into the urine. If introduced into one of the bloodvessels by injection, or even absorbed from injection into the subcutaneous tissue, it rapidly betrays its presence in the urine. This, indeed, I can attest from my own experience. Both after injection into the vein and into the subcutaneous tissue, the albumen of egg, as I have often seen, is soon recognisable in the urine.

It has also been observed that a meal consisting largely of eggs, particularly after prolonged fasting, has been followed by the appearance of albumen in the urine. Here, apparently, it has happened that some albumen has reached the circulation without having undergone the usual conversion, and, as when experimentally injected, been thence discharged with the urine. Hence it may be concluded, not only that egg-albumen and blood-albumen differ strikingly from each other in a physiological point of view, but that egg-albumen, as such, is not fitted for entering into the circulation.

The conversion of albumen, therefore, into albuminose not only bears on the facility of absorption, but on the adaptability for subsequent application in the system. The change is required not only with a view to adaptability for absorption, but to subsequent fitness for utilisation in the system.

Caseine and gelatine I have found* behave in the same manner as albumen. When directly introduced into the circulation, they pass off from the system with the urine. The injection of three ounces of milk into a vein was followed, in an experiment which I some time back performed, by the appearance of caseine in the urine. The injection of one hundred grains of isinglass, dissolved in two and a half ounces of water, also so charged the urine with gelatine as to give rise to the formation of a firm, solid jelly on cooling.

Thrown off as they thus are from the system, albumen, caseine, and gelatine are evidently not adapted for direct introduction into the circulation. Fibrine, on account of its solidity, cannot be similarly experimented with. Digestion, however, in its case is an indispensable condition to its introduction into the circulation. In respect of all these principles, indeed, it may be said that metamorphosis in the digestive system is needed as a preliminary step to their utilisation in the system, or their subservience to the economy of life.

I have thus followed the nitrogenous alimentary principles to the stage of albuminose. The precise nature of the next step is not yet known. There can be little or no doubt as to the progress from albuminose to the albumen of the blood; but as to what next ensues we have no data to show. With the ultimate products which are formed we are familiar; but the steps of metamorphosis are as yet beyond our knowledge. The chain we have hitherto followed now wants one or more links which as yet have baffled experimentalists to bring into view. As regards the seat of metamorphosis we have also nothing of a precise nature to deal with, but we may nevertheless hazard the surmise that the liver is the viscus in which albuminose mainly if not entirely, like other nutritive matters absorbed from the alimentary canal, undergoes metamorphosis.

The various nitrogenous principles of the body must be ultimately produced from albuminose; but whether by direct transformation into them, or by passing through the stage of albumen, we have not the means of deciding. That albumen is susceptible of metamorphosis, however, into the other principles, we know from its forming in the egg the pabulum whence the various nitrogenous principles of the young bird are derived.

Instead of wandering further into the field of conjecture on the subject of metamorphosis, let us turn our attention to the purposes fulfilled by the nitrogenous principles as alimentary matter.

No. V.

THE PURPOSES SUBSERVED BY THE NITROGENOUS PRINCIPLES AS ALIMENTARY MATTER.

First in importance is the office fulfilled by affording a supply of material for the growth primarily, and for the renovation secondarily, of the tissues. Wherever vital operations are going on, there nitrogenous matter is present, forming, so to speak, the spring of vital action. Although non-nitrogenous matter contributes in certain ways towards the maintenance of life, yet it is nitrogenous matter in which arise the molecular changes that determine the phenomena of life. Nitrogenous matter, it may be said, forms the basis without which no life manifests itself. Life is coincident with molecular change. In non-nitrogenous matter the elements of the molecule are not, of themselves, prone to change, as in the nitrogenous molecule, where there exists a greater complexity of grouping among the elements, and where also the elements cohere so loosely or are so feebly combined as to have a tendency to alter or to re-group themselves into simpler combinations. By this change in the nitrogenous, change is started in the contiguous non-nitrogenous molecule, and occurring, as it all does, in a definite or prescribed direction, the phenomena of life are given rise to. Nitrogenous matter, in short, is the instrument of living action, and, as such, it is incessantly being disintegrated—destroyed—changed; becoming effete and inert, and thus necessitating a supply of fresh nitrogenous matter to replace that which has been used up. The primary object, therefore, of nitrogenous alimentary matter may be said to be the development and renovation of the living tissues.

We have seen that nitrogenous matter forms an essential part of living structures. It acts in the same manner in the case of secretions. These owe the active properties with which they are endowed, always in part, if not always entirely, to a nitrogenous constituent. This is drawn from the blood by the glands just as it is drawn by the tissues; and in passing from the blood it is modified or converted by the agency of the gland into the special secretory principle encountered. Nitrogenous matter is thus as essential to the constitution of the active secretions as it is to the tissues; and as the amount of the secretions required is in relation to the general vital activity, a corresponding demand for nitrogenous matter is created.

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* Gulstonian Lectures (1862) on Assimilation and the Influence of Defects on the Urine.

I now come to treat of

NITROGENOUS MATTER IN RELATION TO FORCE-PRODUCTION.

The dependence of muscular and nervous action upon oxidation of the respective tissues is one of the many doctrines which have sprung from the theorising intellect of Liebig. According to this view, nitrogenous matter alone constitutes the source of muscular and nervous power. The tissues being worn out by the exercise of their functions or in the manifestation of their dynamic properties, fresh nitrogenous matter is needed to replace that which has gone to the production of power. It is thus inferred that nitrogenous matter is not only applied to nutrition and to the evolution of the nitrogenous constituents of the active secretions, but also to the restitution of the loss incurred by the production of power. What wonder, then, if, with all these purposes to fulfil, the nutritive value of food should have been reckoned by the amount of nitrogenous matter it contains.

Like many other of its author's views, this doctrine appeared so plausible that for some time it reigned unquestioned. Experimental inquiry, however, gradually began to invalidate it, until Traube arrived at a directly opposite conclusion as to the source of muscular and nervous power. According to this observer, the organised or nitrogenous part of a muscle is not used up in its action, and the resulting force is due to the oxidation of non-nitrogenous matter, the muscle merely serving as a machine to convert the liberated force into motor power. The problem has attracted much attention in several quarters. Let us see how the researches conducted of late with so much elaboration affect its solution.

This solution will be contained in the answer given to the following question: Does the force evolved by muscular action proceed from the destruction of muscular tissue? If so, nitrogenous matter will be needed to repair the loss incurred, and it is equivalent to nitrogenous matter through the medium of muscle being applied to the production of motor power. Now, if muscular action is coincident with the destruction of muscular tissue there must, as a product of the destruction, be a nitrogen-containing principle eliminated. The elements of the compounds that have served their purpose in the economy do not accumulate, but are eliminated from the system in some form of combination or other. The nitrogen, therefore, belonging to a consumed nitrogenous structure, should be recognisable in the effete matters thrown off from the body. Nay, as the force developed by muscular action cannot arise spontaneously—as it can be produced only by transmutation from another force—the destruction of muscular tissue (which implies chemical action as the source of power) should be in proportion to the amount of muscular work performed, and the nitrogen contained in the excreta should be in proportion to the amount of muscular tissue destroyed.

It happens that the quantity of outgoing nitrogen is susceptible of easy and exact determination, and it is thus in our power to arrive at a solution of the question by appeal to experiment.

First of all, as to the channels by which the effete nitrogen passes out of the system.

According to the latest researches, no appreciable amount escapes in either the cutaneous or the pulmonary transpiration.

There remain the alimentary canal and the kidneys as the only channels of exit. Nitrogenous matter is found in the discharges from both these passages; but more particularly from the kidney. And, although discordant results were formerly obtained, it may now be looked upon as established that an amount of nitrogen equivalent to that which enters with the food can be recovered from the urine and alvine dejections.

Lehmann found, while subsisting on a purely animal diet (eggs), that a daily average of 30·3 grammes (467 grains) of nitrogen entered his system, and that a daily average of 24·4 grammes (376 grains) was discharged by the urine; hence a quantity equal to five-sixths of the ingested nitrogen escaped by the kidneys.

Dr. Parkes, in a series of very carefully conducted experiments upon two soldiers,* obtained the following results in relation to the ingestion and egestion of nitrogen. Two men for sixteen days were made to consume exactly the same allowance of food every day. During that period the nitrogen taken amounted to 313·76 grammes; from the urine of one man were recovered 303·660 grammes, and from that of the other 307·257 grammes, being 10 grammes of nitrogen less in the former, and 6 less in the latter, than that taken with the food. An analysis of the alvine evacuations was made only upon three occasions, and, taking the mean of the results given as representing the daily average of nitrogen discharged from the alimentary canal, it amounted for the sixteen days in the case of one man to 25·8, and in that of the other to 17·2 grammes of nitrogen, thus somewhat exceeding the deficiency apparent between the ingested nitrogen and that excreted in the urine, or giving rather more nitrogen discharged than nitrogen ingested.

Let it be taken, then, that the outgoing nitrogen is to be found in the products discharged from the kidneys and bowels. The nitrogen eliminated by the latter, owing its origin to the nitrogen in the undigested food on the one hand, and that in the unabsorbed intestinal secretions on the other, has no bearing on the question in view. It is with the urine that the nitrogen resulting from the metamorphosis of nitrogenous matter in the system passes out, and observation has shown that in the human subject the form chiefly assumed is that of urea.

Now what is the state of the urine in relation to rest and exercise? If muscular disintegration forms the source of muscular work, the quantity of urinary nitrogen ought to increase in proportion to the muscular work performed. Lehmann, imbued with the doctrines of Liebig, as his writings show, speaks of there being an actual increase in the elimination of urea in proportion to muscular exercise, and yet he gives it as the result of observation upon himself that, while under ordinary circumstances he passed about 32 grammes (498 grains) of urea in the twenty-four hours, the quantity passed after severe bodily exercise was upon one occasion 36 grammes (555 grains), and upon another 37·4 grammes (577 grains)—only this moderate deviation to represent the difference in the amount of muscular work performed.

Voit experimented on a dog, and determined the amount of urea voided during rest and the performance of mechanical work in association both with abstinence and a regulated diet of meat. The work imposed on the dog was running in a treadmill. The results, alike during abstinence and feeding, exhibited no material excess in the urea voided during work over that voided during rest.

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* Proceedings of the Royal Society, June, 20th, 1867.

Dr. E. Smith also, in his observations on the elimation of carbonic acid and urea during rest and exercise, found, in the case of the prisoners at Coldbath-fields, that, in the absence of food, the labor of the treadwheel did not materially increase the nitrogen discharged under the form of urea. Like others have done, he noticed a distinct relation between the urea discharged and the food ingested. At the same time he suggested—and, be it remembered, this was as far back as ten years ago—that the relation between urea and muscular work was far less established then than it had been considered to be some years previously.

The theory that muscular work is dependent on, and proportioned to, the destruction of muscular tissue by oxidation, received its decisive blow from the now celebrated observations of Fick and Wislicenus, professors of physiology and chemistry respectively at Zurich.* These experimentalists subjected themselves to a measurable amount of work by ascending a mountain of an ascertained height. They argued that if the work performed be due to destruction of muscular tissue, then (the urine containing the nitrogenous product of destruction) the collection of the urine, and the determination of its nitrogenous contents, ought to show the amount of nitrogenous matter destroyed. Again, as the mechanical work to be performed must be represented by an equivalent of chemical action to produce it, the destruction of nitrogenous matter, as measured by the nitrogen appearing in the urine, ought to balance the amount of the work performed. To simplify the experiment, the food consumed by the experimentalists consisted wholly of non-nitrogenous matter; so that the nitrogen appearing in the urine might be derived exclusively from the system.

Drs. Fick and Wislicenus chose, for ascent, the Faulhorn near the Lake of Brienz in the Bernese Oberland—a steep mountain of about 2000 metres (6561 feet) above the level of the lake, and furnished with hotel accomodation on the summit, enabling them to rest over night and make the descent next day.

On the 30th of August, between ten minutes past five in the morning and twenty minutes past one in the afternoon, the ascent was made. From the noon of the 29th no. nitrogenous food had been eaten by the experimentalists—their diet consisting solely of starch and fat taken in the form of small cakes, sugar; and tea, beer, and wine as fluids. After ascending the mountain Drs. Fick and Wislicenus rested, and took no other kind of food till seven in the evening, when they partook of a plentiful repast of meat and its usual accompaniments.

They began to collect their urine for examination at 6 P.M. of the 29th—that is, six hours after the commencement of their non-nitrogenous diet. The urine secreted from this time till ten minutes past five A.M. of the 30th, when the ascent began, was called the "before-work" urine. The urine secreted during the ascent was called the "work" urine; and that from 1·20 P.M. to 7 P.M. the "after-work" urine. Finally, the urine secreted during the night spent on the Faulhorn up to half-past five A.M. was also collected and termed the "night" urine.

Each specimen of urine was measured, and both the quantity of urea and the absolute amount of nitrogen determined. For the demonstration before us, it will suffice to take the nitrogen.

The quantity of this element secreted per hour, as deduced from the amounts contained in the respective specimens and from the time occupied in secretion, stood thus for the several periods:—

Quantity of nitrogen excreted per hour.

	FICK. Grs.	WISLICENUS. Grs.
Before-work	0·63	0·61
During-work	0·41	0·39
After-work	0·40	0·40
Night	0·45	0·51

A glance at these figures shows the agreement existing between the two cases. The result proved that, while the nitrogenous excretion was related to the food ingested, it was not so to muscular action. The less nitrogen produced during the "work"—and "after-work" periods as compared with the "before-work" period—was plainly attributable to the absence of nitrogenous food from the diet. During the night, after the meal of mixed food, there was an increase, greater in Wislicenus's case than in Fick's; but the one meal did not bring the amount of nitrogen up to the point at which it stood shortly after the commencement of their abstinence from nitrogenous food.

First, then, the conclusion may be drawn from this experiment that muscular work is not accompanied by an increased elimination of nitrogen, such as might be looked for if the work resulted from the oxidation of muscle. But let us inquire whether the disintegration of nitrogenous matter which actually occurred during the "work" and "after-work" periods, as measured by the nitrogen excreted, would correspond with the generation of force equivalent to that expended in the work performed.

The nitrogenous matter of muscle containing, say, in round numbers, 15 per cent. of nitrogen, it is easy to calculate to how much muscular tissue the excreted nitrogen was equivalent. Then, taking this muscular tissue, an approximate if not an absolute estimate can next be given of the amount of mechanical work which its oxidation would be capable of producing.

As regards the other side of the question, the height of the ascended mountain being known, the amount of muscular force expended in raising the weight of the body to the summit can be definitely stated.

We have before us—

1. From the nitrogen excreted the amount of nitrogenous matter oxidised.
2. The amount of force that this oxidation would generate; and
3. The expenditure of force required to raise the bodies of the experimentalists to the height they reached.

If the work performed were due to the destruction of the muscle, then the second factor ought to equal the third; that is to say, the force producible from the muscle oxidised ought to equal the force expended. The results of the calculation, however, show, as will be presently seen, that the force expended considerably exceeded what could be accounted for by the nitrogenous matter consumed.

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* On the Origin of Muscular Power. By Drs. Fick and Wislicenus. *Philosophical Magazine* (Supplement), vol. xxxi., 1866.

Before completing the calculation, there is a further point that requires to be taken into account. Besides the force expended in simply raising the body weights of the two men to the elevation reached, there would also be occurring during the performance of the work an expenditure of muscular power in keeping up the circulation, in respiratory action, and other life processes. The calculations on these points have been carefully worked out by Fick and Wislicenus; and though the data for calculating can scarcely be regarded as precise enough to warrant our looking upon the results given as scientifically exact, still they may be admitted as affording a safe general expression of the matter. We are also told that, in dealing with doubtful data, figures were taken as favorable as was permissible to the old hypothesis of the source of power in muscular oxidation.

Now, summarily stated, the result of the experiment was that the measured work performed during the ascent exceeded by about one-half in Fick's case, and more than three-fourths in that of Wislicenus, the amount which it would be theoretically possible to realise from the amount of nitrogenous matter consumed.

THE

BOWEL LESION OF TYPHOID FEVER; ITS NATURE AND TREATMENT.

By T. J. Maclagan, M.D.

In April, 1871, I published in the *Edinburgh Medical Journal* a paper "On the Nature of the Intestinal Lesion of Enteric Fever," in which expression was given to views considerably at variance with those usually entertained on that subject, but which were the result of years of careful and minute observation of the symptoms and course of that malady. Subsequent observation has tended only to confirm me in the opinions then expressed.

To the general features of the disease I shall not specially refer. I wish only to call attention to the bowel lesion—to the ulceration of the small intestine which accompanies typhoid fever so invariably that it is regarded as the "anatomical sign" of the disease.

This lesion consists in inflammation and ulceration of the agminated and solitary glands of the small intestine. These glands are scattered over the last few feet of that portion of the bowel, increasing in number as we pass downwards, until at the extremity of the ileum they are so numerous and closely aggregated together that but a small part of the mucous membrane remains free from them. They are imbedded in the submucous tissue, and covered over by the mucous membrane; they have no external opening, but are blind, ductless glands. The solitary ones are also found sparsely distributed over the large intestine, diminishing in number the further we pass down from the cæcum.

The nature and function of these glands in a state of health have been matter of some difference of opinion: some have supposed that they are secreting cells, which periodically burst and discharge their contents into the intestine; others regard them as absorbent glands, which have no share in the production of the intestinal contents, but are somehow engaged in the elaboration of the fluid which is ultimately carried away by the lacteals. The latter view is the more generally adopted, and is, no doubt, the correct one. The appearance which these glands present under the microscope is very similar to that presented by the medullary cords of the lymphatic glands; and Brücke succeeded in injecting them through the lacteals—a proof of their connexion therewith. Their position in the intestine also supports this view; for if they were secreting cells, and if their function consisted in supplying to the intestine something requisite for the digestion of its contents, they would be situated high up in the gut (as are Brunner's glands and the bile and pancreatic ducts), where their secretions would be brought into play and utilised at an early period of the process of digestion. The sphincter ilei, or band of muscular fibres at the extremity of the ileum, probably has for its function the narrowing or temporary occlusion of the outlet of the ileum, and consequent detention of the chyle in contact with these glands.

The period of life at which they are most active is adolescence and early manhood. By the time that middle life is reached they begin to diminish, and continue to do so until in old age they quite disappear. This fact would indicate that their special function is to absorb from the chyle and send into the system through the lacteals something which is requisite for the building up and strengthening of the frame—something which is necessary for maintaining the vigor and elasticity of youth, but which is not so much required after the frame begins to undergo those slow and gradual changes which ultimately terminate in the feebleness and sterility of old age. Suppose that the function of these glands were to absorb from the chyle and supply to the blood some substance which the reproductive organs appropriate to themselves, and which is necessary to enable them to perform their special functions, we should at once have a sufficient reason why these intestinal glands, coming into notice in childhood, should be particularly abundant and active in adolescence and early manhood, and should gradually dwindle away after the vigor of life has passed. We find that such is their history; that their period of activity is also the period of activity of the reproductive organs; and we not unreasonably infer that there may be some connexion between the two.

The bowel lesion of typhoid fever consists in inflammation, sloughing, and ulceration of these agminated and solitary glands. What is the connexion between this local lesion and the febrile symptoms? Is the constitutional disturbance a mere symptom of the bowel disease, or is this a result of the circulation in the blood of the poison of the malady?

The generally entertained opinion is that the bowel lesion is the result of nature's efforts to eliminate the poison from the system; that the deposit which takes place in and around the glands is specific in character; and that its deposition in and elimination by these glands is the means of ridding the system of the poison which produced the febrile symptoms. The sole facts in favor of this view are—(1) that inflammation of these glands is an invariable accompaniment of typhoid fever, and (2) that the discharges from the bowels largely contain the poison of that disease.

Such a view of the nature of this important lesion I believe to be altogether erroneous, and that for the following reasons:—

1. The glands are possessed of absorbent functions only, and are in no sense eliminatory.

2. Even if they did possess eliminatory functions, it is very improbable that the work of elimination would be thrown to so large an extent on the glands at the lower extremity of the ileum, while those high up escaped altogether.

3. Were they eliminatory, they would not probably be destroyed (as they are in typhoid fever) while performing a duty so necessary to the well-being of the body.

4. Were the elimination of the poison by these glands nature's way of relieving the system, the deposition in them of the morbid material ought to be followed by an amelioration of the general symptoms of the disease. We know that the contrary holds good, and that the bowel lesion increases rather than diminishes the general febrile disturbance.

5. The opinion that the poison is thus eliminated is at variance with the fact that mild cases of typhoid fever do occur in which the glandular lesion terminates in resolution, and in which the so-called morbid deposit is reabsorbed into the system.

For the above reasons I reject the views generally entertained regarding the nature of this lesion, and hold that it is a specific inflammation of the agminated and solitary glands consequent on the action of the poison of typhoid fever on these glands, and bearing to that disease the same kind of relationship that inflammation of the tonsils does to scarlatina, and inflammation of the mucous membrane of the respiratory passages to measles.

I believe also that the sloughs and discharges from the ulcerated glands carry the poison of typhoid fever, and are capable of conveying that disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina and measles are capable of transmitting their peculiar poison. Nay, I go a step farther, and maintain that the contagious nature of these discharges is not done full justice to by a simple recognition of their power to convey typhoid fever from one person to another, but that *their propensity to evil is coincident with their existence, and that they can, and often do, impart the specific inflammation of which they are the product to the unaffected glands with which they come in contact in their course down the intestine.* I hold, in short, that in typhoid fever there are two forms of bowel lesion, a *primary* and a *secondary:* the primary lesions commence coincidently with the onset of the febrile sympsoms; the secondary result from the inoculation of healthy glands by the discharges from those primarily involved.

The grounds of this belief are various, and may be briefly stated as follows:—

1. It is consistent with all pathological analogy that the discharge from a specifically inflamed organ applied to a like organ in a state of health should there reproduce that specific inflammation to which it owes its origin.

2. Examination of the individual lesions shows that they are of two kinds—one in which the deposit is scanty and the mucous membrane granular in aspect, and another in which the deposit is more abundant and the mucous membrane smooth: to the former Louis gave the name of *plagues molles*, to the latter that of *plagues dures.* Besides this difference in appearence and in amount of deposit, there is also a difference in the mode in which they ulcerate. In the *plagues dures* the process commences in the interior, and the mass sloughs out bodily; in the *plagues molles* the process of destruction commences by a superficial ulceration which eats its way inwards through the mucous and sub-mucous coats, and may even penetrate the muscular and peritoneal. No explanation of this difference has ever been given. The real distinction between them I believe to be, that the *plagues dures* are the primary lesions, the *plagues molles* the secondary. The explanation of the difference in their external characters is, that in the primary lesions (*plagues dures*) the inflammatory product is slowly deposited in the glands and surrounding submucous tissue, and continues to be deposited for eight or ten days before sloughing takes place; while in the secondary lesions (*plagues molles*) the direct application of the virus and putrid discharges sets agoing a smart inflammatory action which terminates in ulceration before there is time for the deposition of sufficient inflammatory products to give them the external appearance of the primary lesions.

3. The glands of the lower part of the ileum suffer more than those higher up. When considering the normal physiological action of these glands, it was noted that they became more numerous the further down the ileum we went. This peculiar distribution has for its object to secure that no waste takes place; while the contraction of the sphincter ilei muscle retains the chyle in the lower part of the small intestine, the presence of so many absorbent glands in that locality provides against the passage into the large intestine of any quantity of the nutrient material which ought to be taken up in the small. The contents of the ileum in a state of health consist of such nutritious ingredients of the chyle as have not yet been absorbed, epithelium, and the excretory matter which goes to form the fæces in the large intestine. In typhoid fever they consist of the same materials *plus* the sloughs and discharges from the affected glands situated higher up. In a state of health the numerous glands in the lower part of the ileum, in the exercise of their absorbent functions, take up the nutrient material remaining in the chyle, and the rest of the iliac contents pass into the colon. In typhoid fever they take up the same material, but along with it they absorb the virulent discharges proceeding from the affected glands situated higher up. The discharges from the secondary lesions are as potent for evil as those from the primary. Hence it is that the lower part of the ileum suffers so much more than the upper; for the further down the small intestine we go below the highest affected gland, the greater is the amount of virus in a given quantity of the contents of the bowel, and the less the likelihood of any gland escaping. On careful examination of the diseased intestine after death, it is found that "the transition between the diseased and healthy glands is usually rather abrupt, and proceeding downwards after the first diseased patch all are usually diseased" (Murchison). This fact can be satisfactorily accounted for in no other way than that which I have

indicated. The greater abundance of these glands at the lower extremity of the ileum doubtless leads to a correspondingly greater frequency of primary lesions in that locality; but for the explanation of the fact that a healthy patch is rarely, if ever, found below a diseased one, it seems to me absolutely necessary to recognise the influence of an agency which acts from above downwards, and which comes into play at the upper margin of the disease. Such an agency can be no other than the sloughs and discharges from the affected glands, which, in their passage down the gut, tend to inoculate those below, but cannot affect those above the highest point at which they are formed. An apparent objection to this view, that the ulcers near the ileo-cæcal valve are as far advanced as those higher up, is explained away by the fact that the secondary lesions ulcerate much more rapidly than the primary, and are in a week as far advanced in their pathological course as those primarily affected a fortnight earlier.

4. The extent of the intestinal lesion, as revealed by examination after death, bears no relation to the severity of the abdominal symptoms during life. The severity of the abdominal symptoms is judged of very much by the amount of diarrhœa. In cases in which the bowels are moved several times a day the sloughs and discharges are carried off as quickly as they separate, and thus do their minimum of harm. Where constipation exists they are retained, and so have an increased chance of inoculating healthy glands. Where constipation exists there may thus be produced an amount of local disease greater than would have existed had the abdominal symptoms been more marked, and the discharges carried off more speedily.

When considering the difference between the primary and secondary lesions it was noted, *inter alia*, that in the primary lesions the process of destruction commenced in the interior, the diseased gland forming a slough which separated bodily,* while in the secondary the destructive process commenced on the surface, and proceeded inwards. As a consequence of this mode of formation of the ulcers, the primary lesions seldom extend beyond the base of the glands—that is to say, they involve the mucous and submucous coats, reaching, but not penetrating the muscular. In the secondary lesions the ulcerative process commences on the surface, and proceeds inwards, rapidly destroying the mucous and submucous coats, but does not have the same reason for stopping there, and hence is more apt to invade the muscular and peritoneal coats. In this way is to be explained the fact that perforation most frequently occurs in cases in which up to the moment of its occurrence, the abdominal symptoms have been by no means marked. This accident, indeed, often happens in cases in which such symptoms have been altogether awanting. How is this? It seems very like a paradox to say that perforation, the acme of all that is severe in the bowel lesion, is most common in cases in which the evidence of that lesion is almost or altogether awanting. Paradoxical as it appears, it is nevertheless true. The explanation of this fact is that the perforation is the result, not of a primary, but of a secondary lesion; for if in these mild cases the number of glands primarily involved is comparatively small (as I believe it is) there remains a large number of sound glands liable to inoculation; and as the risk of perforation is directly as the extent and number of the secondary lesions, it follows that the probability of that accident occuring as a consequence of such a lesion bears some relation to the mildness of the attack. It is to be noted, too, that perforation generally occurs low down in the ileum, where we have seen that secondary ulceration is most abundant and most active.

Hæmorrhage, too, when it occurs to any extent, usually proceeds from a secondary ulcer. So long as the mucous and submucous coats only are involved profuse hæmorrhage is not likely to occur, there being in them no vessel sufficiently large for the production or such a result. When the muscular coat is invaded, however, the risk is much increased, for between the transverse and longitudinal fibres there run vessels of considerably larger size than those found in the submucous tissue. These vessels run a great risk of being opened into by an ulcer which eats into the muscular coat. This it is which imparts to even a slight hæmorrhage occurring late in the case an importance which would not attach to it at an earlier period. It indicates a still progressive secondary ulcer, and there is no saying where such an ulcer may stop. It may open into other vessels, or involve the peritoneum in its destructive course.

The time when the sloughs are separating is generally, I had almost said universally, regarded as the period of greatest danger, so far as the bowel lesion is concerned. Experience shows that the danger from that lesion increases the longer the fever continues, and that from the beginning of the fourth week onwards it is at its greatest. Now it can scarcely be supposed that a slough, which is formed during the second week (as most if not all of them are), which is necessarily of limited size, and which is seated in a very vascular structure, should take a fortnight or three weeks to separate. Yet such a conclusion is the inevitable outcome of the generally entertained opinion. The position seems to me quite untenable. I believe that the whole process of inflammation and destruction of the glands primarily involved, and separation of the resulting sloughs, never occupies more than three week, and that the prolongation of the disease beyond that period, and all the consequent risk and dangers, are attributable to the formation of secondary lesions, the distinguishing characteristic of which are the rapidity with which they are formed and the greater tendency which they show to extend beyond the submucous coat. I believe that hæmorrhage seldom, if ever, occurs before the primary sloughs begin to separate, and that the bleeding which occasionally takes place at that time is rarely a source of danger. It is generally very slight. Dangerous hæmorrhage, as already explained, comes on late in the case, when the primary sloughs are detached, and when the secondary ulcers are still pursuing their destructive course, threatening the larger vessels of the muscular coat.

In the vast majority of cases in which death is attributable to the intestinal complication, the fatal event is the result of the destructive ulceration of the secondary lesions.

When a patient dies of typhoid fever within the first fortnight, death is due to the intensity of the poison, and not to the abdominal mischief. In most cases in which the bowel lesion is the source of danger, the urgent symptoms are developed late in the case—seldom before the middle or end of the third week, generally during the fourth or fifth, by which time it may be presumed that the sloughs have separated from the primary lesions; the exacerbation of the local mischief being due to inoculation of fresh glands by the poisonous discharges of those primarily affected.

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* This is a result of the mere mechanical pressure of the effusion on the vessels which supply the glands. The vessels are so pressed upon that the glands die for want of nourishment. A consideration of their mode of vascular supply will readily explain this.

If the discharges are so virulent, why, it may be asked, do they not effect the solitary glands of the large intestine? In the majority of cases these glands do escape. This is attributable chiefly to their small size and sparse distribution, it being evident that the risk of a drop of virus bearing discharge* lodging over a small solitary gland in the capacious large intestine is very much less than the chance of its lodging over a good-size patch in the attenuated small intestine; in the latter, too, the discharges are retained for a time in contact with the glands by the action of the sphincter ilei, while in the former they are in constant motion.

The successive inoculation of individual glands by the discharges from those already affected offers a ready explanation of the protracted duration of many cases of typhoid fever. The simultaneous inoculation of a number of accounts for those sudden and often alarming exacerbation of the local and general symptoms which so frequently convert an apparently mild into a severe attack. No matter whether the aggravation of the febrile symptoms takes place before the cessation of the primary fever, or some days afterwards (constituting a true relapse), it is due to one and the same cause, inoculation of healthy glands by the discharges from those already affected. The alarming symptoms which are often developed in the fourth and fifth weeks of typhoid fever are the result either of the absorption by glands which had hitherto escaped of the putrid discharges coming from above them, or of the destructive ulceration of the secondary lesions. Occurring while the primary fever still exists, such inoculation gives rise to more alarming symptoms than are observed in a true relapse, in which the second seizure is generally mild, probably in consequence of the diminished scope for intestinal mischief resulting from the destruction of so many glands during the primary seizure.

The age and mode of life of the patient have a material influence on the severity and course of typhoid fever.

Age.—As already explained, typhoid fever increases in frequency and severity from the earliest period at which it occurs up to the time at which the vigor and elasticity of the frame are greatest; when that period is passed it begins to descend, and becomes less and less common as years advance, till we find that after fifty it is a rare and not very fatal disease. In this respect it presents a marked contrast to typhus, which so increases in severity and fatality with advancing years that recovery is far from common after fifty, and is scarcely to be looked for at sixty and seventy. The period of life during which typhoid is most prevalent and fatal is that during which the intestinal glands which are specifically affected in that disease are most prominent and active, and that, in its turn, may be said to be the period of activity of the reproductive organs. Typhoid fever is therefore most fatal in early manhood.

The mode of life of the patient has a very important influence on the severity of the attack. All who have seen much of the disease in both hospital and private practice will agree with me that it is more fatal among the better classes than among the poor. Why is this? Hospital patients are not so well nourished, are often the victims of considerable privations, and seldom come under notice till they have been ill a week or ten days; one would therefore be disposed to think that they would be less able to bear up against the disease. I have seen it stated (I do not remember where) that people in the higher ranks do not bear up against such a malady so well as those in a humble sphere because, being in the daily habit of using a more generous diet and a considerable quantity of wine and other stimulant, they do not so readily respond to stimulants when these become necessary to counteract the depressing influence of the disease. Such an impression I believe to be quite erroneous. A drunkard of course suffers more severely from fever or any other ailment than a temperate man, and is less easily kept up; but such patients are more common in hospital than in private practice. The greater fatality of typhoid fever among the better classes is due to quite another cause. It is due, not to any difficulty in getting them to respond to the action of stimulants, but to a really greater severity of the disease; the malady takes a greater hold of them, and they consequently suffer more from it, independently of all treatment.

We have seen that the glands which suffer in typhoid fever are absorbent, and that their function probably consists in taking up from the chyle something which the system requires for the maintenance of the vigor and elasticity of youth. Now it may be taken for granted that the richer and more abundant the chyle the greater will be the work which these glands have to do. Of course the chyle will be richest in those who live best (*cœteris paribus*), and hence it must happen that the absorbent glands of the better classes of society will be more actively and constantly employed than the same glands in the poorer classes.

It may be regarded as a physiological law that an organ's capacity for work increases with the demands made upon it: frequent exercise increases the size and strength of the muscles; obstruction to the flow of blood from the heart produces hypertrophy of that organ; stricture of the urethra produces thickening of the muscular coat of the bladder; when one lung is in abeyance the other does increased work; cessation of the function of one kidney results in increase in the size and efficiency of the other. So it is with these glands: the richer chyle which passes along the alimentary canal of a man who constantly uses a highly nourishing and stimulating diet supplies more of that substance which it is their function to absorb than does the chyle of a man whose diet is sparing. The continued use of such a diet for a length of time must lead to increase in the size and activity of these glands. The condition thus produced is much more favorable to the development of the poison of typhoid fever than that which obtains in the poorly-fed hospital patient. Should this poison be received into the system of one whose glands are so highly developed there inevitably results a severer attack of the fever than the same poison would produce in one whose glands were smaller and less active. The greater fatality, then, of typhoid fever in the higher ranks of life is the result, not of any difficulties in the way of treatment, but simply of the greater severity of the disease consequent on the more highly developed state of the intestinal glands and the necessarily increased facilities afforded for the growth and reproduction of the contagium in the system, as well as the exaggerated dangers of the local mischief.

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* I regard it as proved by the observations of M. Chauveau and Dr. Sanderson that contagium is particulate.

Chomel and Forget have both expressed the opinion that debility from destitution is a favorable circumstance as regards prognosis in typhoid fever. According to the views generally entertained regarding the nature of that fever, this statement is little better than a paradox: to say that debility from destitution positively favors recovery from a disease which kills by asthenia is to make a statement which requires the high sanction of such eminent names to prevent us receiving it with scepticism, if not actual incredulity. The explanation just given of the greater fatality of the disease in the upper ranks of society supplies also a reason for its being less fatal among the poor and destitute, and affords evidence of the accuracy of the observation made by Chomel and Forget.

Treatment.—It may readily be supposed that these peculiar views regarding the bowel lesion of typhoid fever have some influence on the treatment which I adopt. They do influence it very materially.

The opinion generally entertained by medical men in this country regarding the treatment of typhoid fever is that it consists in keeping up the strength of the patient, in procuring sleep, and in checking every tendency to diarrhœa. Regarding the desirability of keeping up the strength and of procuring sleep there can be no doubt. To the general treatment I shall not further refer. I wish only to direct attention to the treatment of the bowel lesion. There are two methods of treating this. One is that recommended by Bretonneau and Louis, and adopted by most French physicians: it consists in encouraging rather than checking the action of the bowels. The other is almost universally adopted in this country, and consists in checking every tendency to diarrhœa. Dr. Todd thus summed up the treatment:—"Restrain diarrhœa and hæmorrhage in typhoid fever; and when you have fairly locked up the bowels, keep them so. Patients will go for four or six days, or even longer, without suffering inconvenience from this state of constipation." Such is the treatment adopted in ninety-nine per cent. of the cases of typhoid fever which occur in this country. Dr. Todd's opinion is that of the profession in general.

The treatment proper for the bowel lesion of typhoid fever varies in different cases, and in the same case at different times. To know what is best for the patient, we must know the probable condition of that portion of the bowel which is the seat of disease. The general opinion is, that inflammation and irritation of the mucous membrane, by causing an increased flow of the secretions, produce a tendency to diarrhœa which it is well to check, and if possible prevent. Granting the soundness of the pathology, the treatment is right enough. I hold, however, that the pathology is unsound, and that the practice of "locking up" the bowels is one which leads to a prolongation of the disease, and increases the risk to the patient.

If all the lesions were primary, and if there were no chance of the secondary inoculation of other glands, constipation might be regarded as a favorable sign. The recognition of the occurrence of secondary lesions alters the whole question, and is the true pathological foundation of the treatment which I have to recommend: it supplies, too, a more satisfactory reason than has hitherto been advanced for the success which has attended the French practice.

In the early stages of typhoid fever it must ever be borne in mind that in many cases resolution of the intestinal lesion is possible, and that judicious management during the first ten days may cut short and render comparatively trivial an ailment which the patient's own indiscretion, or injudicious treatment on the part of the physician, may convert into a long and dangerous illness. So long as such resolution is possible, the patient must be treated for that result. He must be confined to bed, and absolute rest and quiet enjoined. It must be borne in mind that some of the glands of the small intestine are inflamed, and consequently more than usually prominent and tender. Under these circumstances, every possible source of irritation to them must be avoided: for this reason no article of diet should be taken which contains much indigestible matter; for the contact of this with the glands on its passage down the gut would only serve to irritate them more. ***Physiological rest must also be obtained as far as possible.*** For this reason too much nourishment should not be given. The object in view is rest to the affected glands: to attain this object we must give only such articles and such quantities of food as will be digested and utilised in the upper part of the alimentary canal, leaving little or nothing to be done by the inflamed glands. The diet therefore should be mainly composed of milk, given, not *ad libitum*, but in stated quantities and at stated times.

The bowels should be kept as nearly as possible in their natural state. Any tendency to diarrhœa should be counteracted by the addition of lime-water to the milk, and frequently repeated small doses of Dover's powder. Constipation (which is more common in mild cases) is to be obviated by some gentle laxative. I prefer the phosphate of soda to any other; it is not at all irritating, acts very gently, and, having very little taste, is readily taken dissolved in a little warm milk or gruel: from half a drachm to two drachms may be given once or twice a day as required. At this stage of the illness ipecacuanha is very useful: given in small doses—one-fourth to one-sixth of a grain—every two hours, it acts beneficially, not only on the skin, but on the mucous membrane of the bowel. When there is much heat of skin it is best given in the form of wine combined with liquor ammoniæ acetatis. In administering laxatives, it is best to give those which act on the upper part of the alimentary canal.

Such are the means calculated to favor resolution of the bowel lesion.

In all cases after the disease has continued for a fortnight without signs of improvement, and in severe cases at an earlier period of the illness, the hope of attaining such a favorable result may be abandoned. Treatment must now be directed, not to the prevention of sloughing of the glands already involved, but to the prevention, as far as possible, of secondary lesions by the inoculation of glands which had hitherto escaped.

It must be remembered that we have to deal with an intestine in which there are a number of sloughing glands and a number of healthy ones. The liability of the latter to inoculation by discharges from the former evidently depends upon the relative position of the two sets; the higher up the bowel the inflamed glands are situate the greater will be the risk of secondary lesions. It is impossible to tell the exact position of the affected glands. We must act on the supposition, I had almost said the certainty, that there are some glands exposed to inoculation, and do what we can to save them. How is this to be done? Certainly not by shutting up in the intestine the noxious sloughs and discharges. For what is taking place during this quiescent state of the bowels? The sloughs and discharges are being constantly separated from the primary lesions, and the healthy glands situated further down are being exposed to the influence of a poison which has a specific action on them, and which is hourly increasing in strength and quantity the longer constipation exists. By encouraging and keeping up this condition of the bowels we positively increase what we fancy we are guarding against—extension of the bowel lesion. To prevent altogether the formation of secondary lesions is impossible, but it is evident that no means which tend to keep the discharges from the primary lesions in the neighborhood of healthy glands can have other than a deleterious effect, and that any means calculating to ward off this danger must have for its object the carrying off of the sloughs and discharges as soon as they are separated. Hence the objection to the practice advocated by Todd, and acted up to by most physicians in this country; and hence the reason for adopting the opposite line of treatment. After the inflammation of the primary lesions has gone on to sloughing, the patient's welfare demands that the sloughs should be got rid of as soon as possible; this end can only be attained by keeping the bowels open. If constipation exists it should be overcome by small doses of castor oil, phosphate of soda, or other laxative. Looseness should not be checked unless the stools are so frequent or copious as to affect injuriously the patient's strength. It may be stated generally that so long as there are not more than three stools a day astringents should be withheld. The diarrhœa is not entirely due to the actual lesion of the bowel, but is in part due to the irritation caused by the fetid discharges, which it tends to carry off. It is therefore salutary so long as it is not so severe as to prostrate the sufferer.

Space forbids me to enter on the consideration of the question whether or not the typhoid symptoms of enteric fever are not at all due to the absorption of pus or putrid discharges from the bowel lesions. I would only remark that, while they are not due to that cause in every case, it is quite possible for such an accident to occur, and that the recognition of such a possibility is an additional reason for preventing their undue retention within the bowel.

When diarrhœa is excessive it must be restrained, not stopped. For this purpose, acetate of lead, dilute sulphuric acid, liquor ferri pernitratis, or other astringent, with or without opium, may be administered. The liquor ferri pernitratis is particularly serviceable; it is possible that some of its beneficial action may be due to a corrective action on the fetid alkaline contents of the bowel.

The opinions which I have expressed are at variance with those generally entertained; but they are the result of years of careful study of the subject, and of a large experience in the treatment of typhoid fever.

Dundee, January, 1872.

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ON AN

IMPROVED FORM OF DEMONSTRATING OPHTHALMOSCOPE.

By Robert Brudenell Carter, F.R.C.S.,

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The elements which enter into the formation of the ophthalmoscope are so well known, their action is so well understood, and so many combinations of them have been devised by various persons, that it would be extremely hazardous to claim novelty for the arrangement which I am about to describe, and which differs from others only in points of detail, and not at all in principle. I have, however, ventured to call it an improved arrangement, because, whether as regards the simplicity of its construction and adjustment, or the magnitude, brilliancy, and clearness of the image which it affords, it is not equalled by any other with which I am acquainted.

The skilled ophthalmoscopic observer, when he wishes to scrutinise carefully the optic disc or other object in the fundus oculi, uses the direct method of examination. In this the mirror is brought close to the eye of the patient, and the fundus is seen magnified by its own crystalline lens. For many purposes this method is the best that can be adopted; for some purposes it is indispensable. But there are various difficulties in the way of its being generally employed; and the indirect method, or the examination of an aërial inverted image formed by placing a convex lens before the eye, must always be that which will find most favor with practitioners.

The chief disadvantage of the indirect method, as usually pursued, is that it affords only an inconsiderable degree of enlargement; while, at the same time, the highest degree of enlargement easily obtainable is only gained at a considerable sacrifice of illumination.

The reason of this is, that the lamp is placed at the side of the patient's head, and that the convex lens, which should be held at rather more than its own focal length from the eye, forms, at its focal distance on the side of the spectator, an image which increases as the focal length increases, and so becomes larger as the magnifying power of the lens is less. Let the capital letters below represent the relative position of things when the lens used is of two-inch focal length. E represents the eye of the patient, L the lens, I the image, and O M the eye of the observer, with the mirror in close proximity to it. In this case it is supposed that the distances L E and L I are each two inches, and the distance from I to O about fifteen inches, which is generally as little as will be found compatible with clear vision. The resulting image is well lighted, but small, and fine details cannot be

O M   I   L   E

seen with the degree of distinctness that is often required. If now we take a four-inch lens, we obtain an arrangement in which the distances L E and L I become each four inches; and hence O is removed four inches further from E, or to twenty-three inches instead of nineteen. The image is larger, but the illumination seriously diminished. In the first place, as the quantity of light radiating from any source to fall on a given area diminishes as the square of the distance, the absolute amount falling upon the mirror will be lessened by very nearly one-third, and hence less light will be thrown into, and returned from, the examined eye. This lesser quantity of light will in its turn have to form an image of about four times the superficial extent of that first considered, and the large image will therefore be so dimly lighted that it will often be impossible to make out its details with any certainty. Moreover, an object lens of even four inches focal length requires to be held so far from the patient that the observer cannot steady his hand by resting a finger upon the forehead or cheek; and anything beyond four inches soon requires a distance between the observer and the object lens that is beyond the comfortable, or even possible, range of the arm. Practically, a three-inch lens is the lowest power available for hand instruments.

Attempts have been made to overcome these difficulties in various ways. Ulrich, Hasner, Liebreich, Follin, Galezowski, and others have mounted the mirror and the object-glass at the opposite extremities of a tube, blackened inside, so as to cut off external light, and thus to increase the apparent brightness of the image. In most of these instruments, and by many observers using the hand-mirror, a second convex lens has been placed behind the mirror, and used to magnify the image. I myself constantly employ a lens of 7″ behind my hand mirror, in conjunction with an object-glass of 3″.

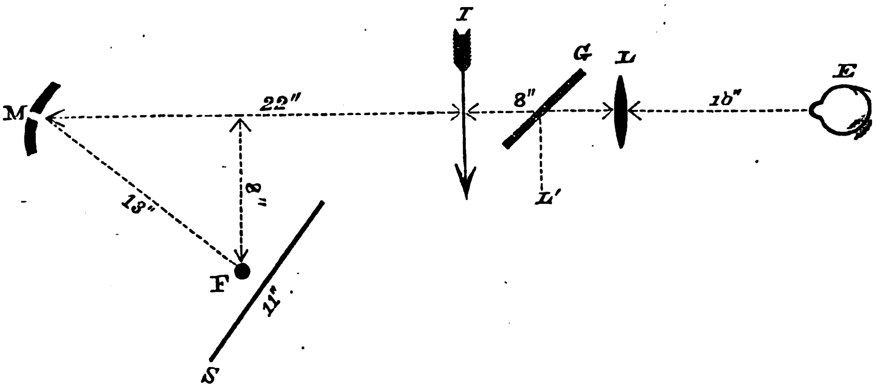
The objection to all kinds of tube instruments is that they render impossible slight independent movements of the lens, by which it is easy to correct, or rather to compensate for, slight movements of the eye. Even a moderately steady, or what may be called a good patient, does not keep the eye perfectly still. Its slight lateral movements constantly displace the optic disc or other point of the image; and the skilled observer follows them, and keeps his object in view, by similar slight movements of his lens hand, movements of which he is scarcely conscious, and in the performance of which he is guided solely by sight. The inverted image appears to move in the same direction with the lens; and, therefore, if the optic disc strays away to the right of the observer, a little movement of the lens to the left will recall it to a central position in the field, and so on with other changes. Moreover, lateral movements of the lens are important aids to diagnosis in cases of depression or projection of any part of the fundus, as in excavation of the nerve-disc, or in the early stages of morbid growths; because the image of a part that is in a plane relatively anterior moves further with the lens than the image of a part relatively posterior to the former. As the lens is moved from side to side, the apex of a projection, or the margin of a cupped disc, appears to move also in front of the general level of the retina or of the floor of the excavation. In tube instruments, of course, the lens can only be moved laterally by moving the mirror also, and thus by changing the incidence of the light and otherwise disturbing the observation. Where any part of the apparatus touches the patient, the difficulty is much increased; for the instrument cannot be moved, as a whole, to follow the eye, and is liable to constant disturbance from slight movements of the head. The latter are of frequent occurrence during ophthalmoscopic observations; because patients, when told to look to the right or left, almost always turn the face, and not the eyes only, in the direction indicated to them.

The use of a magnifying lens behind the mirror soon reaches its limit, and is attended with the disadvantage of narrowing the field of view in a degree corresponding with the enlargement. If in this way we magnify the optic disc, we see nothing but the disc, and cannot at the same time obtain a general view of the fundus.

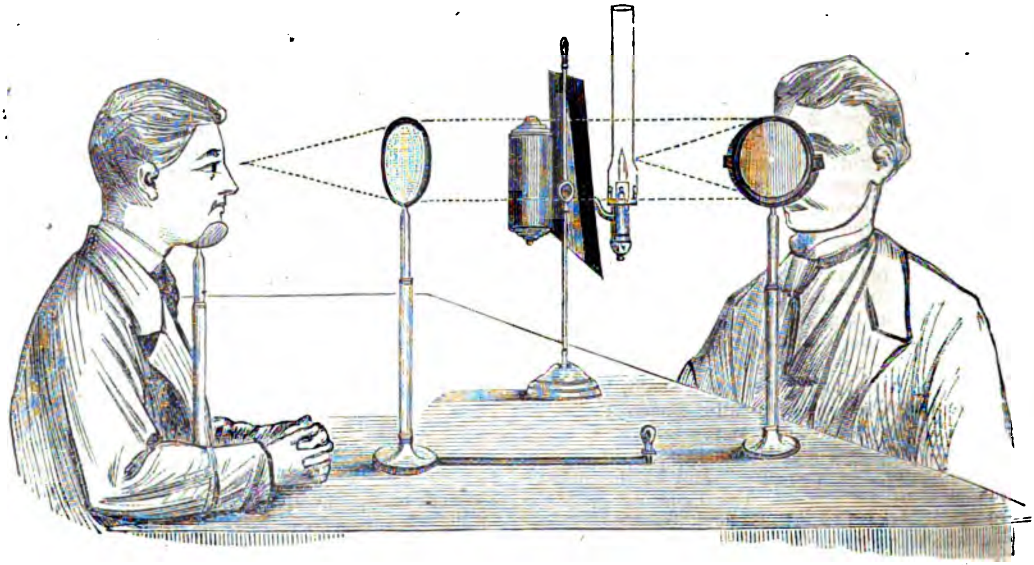
The first successful attempt to overcome these difficulties was made by Dr. Burke, of Paris, whose "reflecting ophthalmoscope" was perfected shortly before the commencement of the Franco-German war. Dr. Burke was good enough to bring me one of his instruments during a hasty visit to England; but at first I failed in using it satisfactorily. It was only when he came again to this country, after the peace, that I learned from him how to overcome my difficulties; and I then exhibited his ophthalmoscope to several medical assemblages. For size of image and brilliancy of illumination it stood quite alone; but it was attended by several minor inconveniences which limited its practical application.

In principle, Dr. Burke's instrument is not entirely original, but is a modification of what had been suggested by Klaunig. It consists of two concave mirrors, each four inches in diameter, placed facing each other, about a yard apart, and about six inches from the edge of a table. One of these mirrors has a central perforation, and a focal length of thirty-three centimetres. The other has no perforation, and a focal length of nineteen centimetres. A lamp is placed on the table, a little to one side of the line between the mirrors, and at the focal distance of the first or perforated one, which is then so turned as to reflect the lamp-light upon the second mirror, from which direct rays are cut off by the interposition of a screen. The patient sits against the edge of the table, looking at the second mirror, and with his face just beyond its focal distance. This mirror is then turned so as to throw the light it receives from its fellow into the eye to be examined, and it forms the rays returned to it into a large and brilliant inverted image—large, on account of the long focal distance of the inverting mirror; brilliant, on account of the flood of light furnished by the illuminating mirror. This inverted image is seen through the perforation of the illuminating mirror. It is necessary to a good result that the two mirrors, the eye observed, and the lamp, should all be precisely on a level, otherwise the image will be obscured by glare, or direct reflections of the flame; and the inverting mirror must not be placed too obliquely, otherwise the pupillary aperture will be distorted. The precise distance between the inverting mirror and the patient requires to be very nicely adjusted; and the observer, who is seated with the patient's back turned to him, cannot make these adjustments himself without rising and going to the mirror and then going back to observe the effect. In the meantime, a very slight movement may take the eye out of view altogether, and practically the instrument is of little use unless the observer has the aid of a skilled assistant.

In using Burke's ophthalmoscope for purposes of demonstration, I was constantly annoyed by these defects; and, in considering how they might be oviated, I was struck by the fact that the chief peculiarity of the instrument was in the brilliancy of its illumination,—and that this, which was solely due to the relative positions of the lamp and the perforated mirror, might equally be obtained if the inverting mirror were removed and replaced by the convex lens of the ordinary ophthalmoscope. The result has surpassed my expectations, and has not only afforded an image fully equal to that of Dr. Burke's ophthalmoscope under the best conditions, but also an instrument requiring no assistant, and perfectly simple and easy of management.



In the above diagram,—E, represents the eye of the patient; L, the lens; I, the image; M, the mirror; F, the flame; S, the screen; and G, the slip of plate-glass to produce a second image at I'.



The apparatus consists of three pieces—a mirror, a lens, and a chin-rest,—each of which is supported on a vertical tube, which slides up and down within an outer tube, and can be fixed by a screw-collar at any desired height. The outer tube is screwed into a circular foot of sufficient weight. The mirror is hung in a gimbal by horizontal screws, and can be turned on its vertical axis by rotating its stand. It is the illuminating mirror of Burke's instrument, four inches in diameter, thirteen inches in focal length, centrally perforated, and furnished at the back with a lens clip, to hold a concave lens if used by a short-sighted observer. The mirror is of silvered glass; and the old tin and mercury silvering affords most light. The amalgam only is removed at the centre, leaving quite a small opening for the passage of light; but the opening in the metal back is somewhat larger, to get its margins out of the way of oblique rays. The lens is biconvex, four inches in diameter, eight inches in focal length, and is fixed in a simple collar, as it requires no movement on a horizontal axis. It can be raised or depressed or turned on its vertical axis by means of the inner sliding-tube of its stand. To one side of the base of the latter is attached a stout horizontal brass rod, fifteen inches long, with a vertical wooden handle at its free extremity. The chin-rest is simply a plate of metal, concave from side to side, rather more than two inches long and one broad, rounded at the corners, and covered with cloth or velvet.

The arrangement of the apparatus is shown, both in ground-plan and in perspective, in the accompanying diagram and sketch, the former of which gives also the relative distances between the patient, the observer, and the several parts of the apparatus. It will be seen that the patient and the observer are placed exactly facing each other, on opposite sides of a table, which (as the distance from the mirror to the patient's cornea should be forty inches) cannot be less than four, nor conveniently more than five feet wide. The lamp is thirteen inches from the mirror, and eight inches to either side (preferably to the right of the observer) of the base line. The lens is thirty inches from the observer, and ten inches from the patient; and its horizontal handle stretches directly towards the former.

The first thing to be done is to seat the patient comfortably, with his chin well on the rest, and this fixed at such a height as to render the general plane of the face vertical. The mirror is then set with its central opening on a level with the patient's eye, and the lens at the same height as the mirror; and these two, and the lamp and screen, are placed in the positions shown in the diagram. The mirror is turned on its vertical axis until the rays reflected from it fall full on the lens, which should then throw a circular patch of brilliant light, about the size of a shilling, full upon the eye of the patient. If the patch be much larger than this, the lens is too distant; if it lose its circularity, and approach the shape of an inverted image of the flame, the lens is too near. Until perfect certainty of adjustment has been obtained, it is well to protect the eye by a screen until the right distance is attained. If the light be of the proper size and shape, but not central on the eye, the lens must be raised or lowered, or moved laterally, until the proper position is obtained. The observer then takes his seat behind the mirror, and looks through the perforation at the image. By means of the horizontal handle, which comes well within his reach, he impresses upon the lens any to and fro or lateral movements he may desire, and is able to follow those of the patient's eye. The patient should be instructed, before the light is thrown on, to look a little across the median line with the eye examined, so as to bring the optic disc into the centre of the field of view.

The best light for the purpose is that afforded by the common German (or so-called Queen's) reading-lamp, not only on account of the ease with which it may be handled, and the burner set at any required height, but also on account of the perfect steadiness of the flame. The flame of even the best gas burners is apt to flicker; and this flickering, which is much intensified in the concentrated beam of the lens, produces disturbance by distorting the outline of the pupil, and by causing broken images of the iris to be projected into the field of view, and partially to conceal the image of the fundus.

The German lamp, moreover, conveniently carries a simple screen, to shield the patient from direct light, in the socket intended to support the ring which sustains the shade, this ring being of course removed. My screen is a piece of stout white cardboard, eleven inches broad and nine deep, blackened on the side that is turned away from the light. A strip half an inch wide is cut out of its lower half, to allow it to fall over the tube that carries oil from the reservoir; and a sort of central leg enters the socket already mentioned. The whole arrangement is shown in the sketch, and the degree of inclination of the screen in the diagram. With any other lamp, a detached screen may be employed.

If the plane of the lens be placed, as shown in the diagram, at right angles to the line between patient and observer, the view will be more or less obscured by two reflected images of the mirror. To obviate this, the *inner* tube of the lens-holder must be turned a little on its axis, so as to render the lens oblique, by which the two images will be laterally displaced. The corresponding manœuvre is perfectly familiar to all who use the hand instrument; but it is necessary to turn the *inner* tube only, so as not to alter the position of the horizontal handle.

The line G in the diagram indicates the position of a valuable accessory in using the instrument for demonstrating purposes. A slip of thin plate glass, placed thus obliquely in the track of the rays, will reflect some of them in the direction of I′, and will form there a faint reduplication or "ghost" of the image, which will be sufficiently visible, in the shadow of the screen, to an observer properly placed, who may thus control the lens by being himself able to see dimly the appearances that are seen brilliantly by anyone looking through the mirror opening. The slip of glass may be held in the hand, or placed in a clip supported by a horizontal arm projecting from the lens-holder. I believe it will be possible to intensify this ghost, and even to render it visible to many spectators at once; but this part of the question must be reserved for a future communication.

The ophthalmoscope is now coming into constantly increasing prominence as an instrument of diagnosis in many maladies wholly unconnected with the eye, or, at least, having only a secondary influence upon it; and a good instrument is nearly as valuable to the general physician as to the ophthalmic specialist. I bring this one before the profession with great confidence, feeling that, for the consulting-room at least, it will remove many of the difficulties that have hitherto impeded the general employment of ophthalmoscopic research. Its image, which is brilliantly illuminated, which embraces a large field, and which, with a normal eye, is magnified at least twelve diameters, leaves nothing to be desired, and its construction and management are so simple that I cannot conceive that anyone, even on a first attempt, would fail to obtain a satisfactory view of the fundus oculi. The use of atropine is often desirable, but is not essential except in the case of very contracted pupils. It is best to exclude daylight, or a second artificial light on the same table; but artificial light overhead may be entirely disregarded.

Messrs. Weiss and Son and Mr. Hawksley are both acquainted with all details of the instrument; and from both I have received valuable suggestions in the course of the experiments that have led to its construction.

Wimpole-street, Jan. 1872.

RINGWORM IN SCHOOLS.

By Tilbury Fox, M.D. Lond., F.R.C.P.,

Physician to the Department for Diseases of the Skin at University College Hospital.

Troublesome and disappointed as the management of ringworm in ordinary practice is, it is infinitely more so in schools and public institutions. The consequences of error in the latter cases are in some points of much more serious account. A mistake, for example, in deciding when complete recovery is established is not only often most injurious to the reputation of the practitioner, and of course disappointing to parents, but cruel to the principal of the school concerned, whom it involves in great annoyance and sometimes serious pecuniary loss. I see a constantly increasing number of cases of ringworm in children at school, and I must add, many instances of relapsed disease among them. One of the chief questions upon which a decision, however, is requested is the fitness or unfitness of those who are supposed to be convalescent to go back to school, and to remingle with their playmates. This is a signally important matter in the case of those who are to be returned to schools in which the young are very closely associated, and emphatically so in regard to public institutions of a charitable nature.

The whole question of ringworm in public institutions has recently been prominently forced upon my attention in the case of a remarkable outbreak of the disease, comprising some 300 cases in all, in a public institution near London, which epidemic I was requested a few weeks since to investigate and report upon. With the facts fresh in my mind I shall perhaps, be doing good service to the readers of The Lancet if I discuss briefly the general subject of ringworm in schools, under the heads of—

1. Its origin and dissemination.
2. The treatment of the disease when introduced, as regards (*a*) the actually diseased; (*b*) the surroundings and belongings of the attacked.
3. The preventive treatment as regards (*a*) re-importation through the apparently convalescent; (*b*) rekindling from other causes.

I. ORIGIN AND DISSEMINATION.

Origin.—Firstly, I do not understand that a school is properly managed unless every child admitted is shown to be free from ringworm of the head (tinea tonsurans) and ringworm of the body (tinea circinata), either as certified by a medical practitioner, or by a careful examination at the time of admission by some competent person. A matron or good nurse can have no difficulty in preventing the introduction of ringworm as the rule. Every child with ringworm (tinea tonsurans) has certain "scurfy patches" or spots where the hair looks shrivelled or unhealthy. Such appearances and red scurfy patches on the body are readily detected by anyone who has a pair of eyes, and they should suffice to excite suspicion and to lead to medical examination. Secondly, every week at least a careful inspection of heads should be made in schools. The heads of girls should especially be searched on account of their long hair. In this way the earliest signs of disease must be detected. Ringworm of the body should be recognised more decidedly than it is as the frequent source of ringworm of the head, and dealt with accordingly.

Dissemination.—Well, once introduced into schools, how is ringworm spread? (*a*) By neglect, of course; (*b*) by actual contact of the healthy with the diseased; (*c*) by the common use of towels and brushes by the diseased and healthy; (*d*) by the air of the institution, which, under certain circumstances, is loaded with the germs of the fungus—trichophyton tonsurans.

I want specially to notice the last point. When I came to collect the dust deposited from the air in the wards of the institution in which the outbreak of ringworm before referred to occurred, I found that it contained fungus elements in abundance. This observation I believe to be a novel one. The achorion has been detected in the air passing over children effected with favus, I know; but I speak of the existence of the trichophyton in the air when no artificial means have been adopted to disseminate it there. Fig. 1 gives the appearance seen with a one-fifth inch object-glass at four p. m., in the dust which had collected upon it between that time on a certain day and the evening before. Fig. 2

Fig. 1.

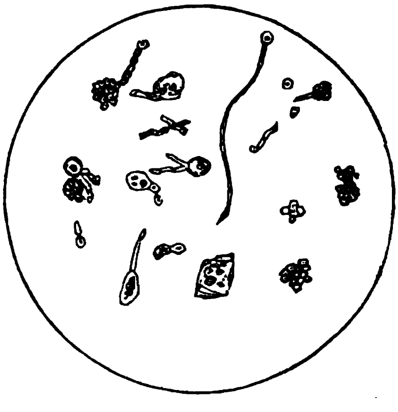
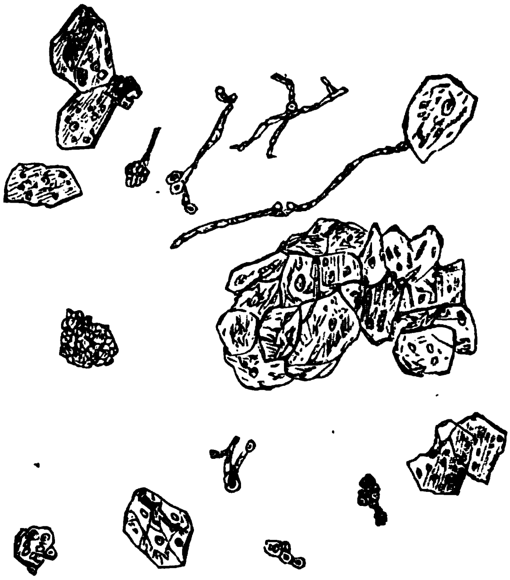


Fig. 2.



shows the fungus developing into mycelium two days subsequently. I need not say care was taken to avoid all fallacies. It will be noticed that epithelial scales were found in the dust. No doubt the scratching of diseased places practised by the patients explains the presence of those several elements in the dust. I presume it was the fact of so many children being diseased—I saw 121 together at the time of my visit—at one and the same time that gave rise to such a plentiful supply of fungus; for I imagine that we should have a difficulty in detecting the fungus in the dust of a room where a few cases of ringworm only were present. I cannot doubt that where ringworm of the body (tinea circinata) only is present, particles of cuticle and fungus may be thrown off by scratching, and so give rise, by the development of germs which fall on the head, to tinea tonsurans. In all cases, therefore, we must endeavor to neutralise the causes of dissemination, to particularly enforce the isolation of the infected, *including those who suffer only from ringworm of the body*, and to disinfect the air where there is reason to think fungus germs exist in it.

II. TREATMENT.

(*a*) *As regards the actually diseased.*—Isolation at all hazards is the first thing to do. I will only say on this point that cases of ringworm of the body must be isolated. I think this of essential importance in the case of schools. It is not, however, thought of any moment as the rule. Where a number of cases occur, it is better to separate instances of very bad and extensive diseases again from slight *new* cases and convalescents, for the simple reason that active treatment may at once annihilate the disease in the former, and in the new cases and convalescents fresh implantations over the, in the main, healthy area of the scalp may be taking place from contact with bad cases of tinea. I would, of course, only adopt this plan where the cases of disease are very numerous—say thirty, forty, and fifty or more.

There are, next, certainly general considerations to be taken account of. Attention to the dietary is one; for the underfed, and ill-nourished, and ill-kept furnish the most appropriate nidus for ringworm. All deficiency in meat should be rectified, and in case the attacked or the non-infected look sickly or pallid, the allowance of meat and fresh vegetables should be increased and supplemented by iron and cod-liver oil. So, again, the cubic space allotted to each child should be ample, ventilation free, and cleanliness enforced with exceptional strictness. One word more as regards the general health of children. If with a rigorous system of inspection in constant operation many cases rapidly appear, and, in spite of hygienic measures, spread; the children furnish clearly a very suitable soil, and the dietary of the children should be looked to. If ringworm becomes epidemic, with a *bad system of inspection*, it implies simply neglect, of course. Here isolation is the main thing needed to protect the healthy, and not feeding up.

The treatment must be very briefly referred to. In all cases in schools the hair should be cut short, close to the scalp. Recent cases are at once checked and often cured by simple blistering. The disease, not having reached the bottom of the hair-follicles, is at once accessible to remedies. Cases of ringworm may be dealt with in the same way. The use of strong acetic acid is perhaps as good as the blistering fluid. If the case is not very recent, epilation of diseased hairs, after the Paris fashion, should be practised. It is generally "too much trouble to do this." I next enforce the use, every few days, of Coster's paste to the extent of some five or six applications, and the subsequent use, night and morning, of some parasiticide ointment, diluted citrine ointment, or sulphur, creasote, and ammonio-chloride of mercury. The head should be washed each day and well greased. The latter prevents the escape and dissemination of fungus germs. If preferred the head may be kept soaked in diluted sulphurous acid; of course a proper cap of silk should be worn.

(*b*) *As regards the surroundings and belongings of the attacked.*—It is scarcely necessary to do more than refer to the necessity of thoroughly cleansing the brushes, combs, and towels of the diseased, and seeing that these are not used in common by the healthy and the infected. Towels should be well boiled. To one novel point I must direct special attention. It is the disinfection of the air of the wards in which a large number of cases of ringworm have been. My recent observations show that the fungus germs are floating in the air, and though I had until lately no experience to go upon, because the observation is a novel one, yet I have no hesitation in saying that the air of the wards should be disinfected by burning sulphur if, after complete isolation has been practised where many cases of ringworm have occurred, other instances of disease still continue to appear amongst the previously healthy.

III. THE PREVENTION OF NEW OUTBREAKS.

As regards the reimportation by those apparently convalescent.—No more puzzling problem is presented to the practitioner than that of saying when a child "is well of ringworm," and "fit to go back to school." I err on the side of caution if there is the least doubt, and advise that the same course be taken by others. When a child is well—that is to say, incapable of reimporting or redisseminating the disease amongst his fellows—there will be present certain naked-eye characters and microscopic appearances. The hair will be growing vigorously and naturally in the original sites of the disease; there will be no scurfiness, no broken-off hairs, and the structure of the hair and its sheaths will be probably developed and free from fungus elements. If the hair is dull and dry, suspicion should be excited; and if the suspected surface is *studded over with short broken-off hairs* (*readily overlooked*), there is still disease present. The fungus will be formed in abundance in the short broken-off hairs. As a rough guide, this is the best. Condemn a child any portion of whose scalp is studded with the little dark points of short broken-off hairs. This is the rule I observed. But no one can be certain in any case without a microscopical examination. If the root is well formed, and the hair sheaths likewise; if fenestrated membrane can be seen, and no fungus detected, then all is right. Of course, fungus in any abundance is at once discovered. The doubtful cases are those in which the root seems healthy, but the shaft of the pulled-out hair is observed to be surrounded at its follicular portion above the root with epithelial and exudation matter. This may be an indication that irritation is being set up by the remedies, the ringworm itself being well. I see many cases of this kind, and in them the roots and surrounding structures, and hair-shaft, are healthily formed, whilst no fungus elements are to be detected in the material surrounding the hair. The scalp in these cases is tender, more or less swollen, and reddened, the hair at the same time growing well and vigorously. Perhaps the plainest and easiest guide to disease still existing, is the presence of short broken-off hairs.

Rekindling of the disease from special causes.—In order that ringworm may not "break out afresh" in schools, the non-infected must observe all those directions which were referred to under the head of "dissemination of the disease." Especially it is important to keep heads perfectly clean by frequent washings, and to keep them fairly greased or oiled. To this latter point I attach much importance.

This, in short, is a sketch of the means to be followed in managing ringworm in schools. There are those who think the use of a weak parasiticide to the healthy is advisable. Well, there can be no objection to sponging the heads, even daily, of the healthy with diluted sulphurous acid, one part to six of water, or, better, with diluted acetic acid, one part to four or six of water.

Sackville-street, W., Jan. 1872.

◆

POLYPUS OF THE EAR.

By W. B. Dalby, F.R.C.S., M.B. Cantab.,

Lecturer on Aural Surgery at St. George's Hospital.

From a tumour of considerable size, which occupies the whole of the external auditory meatus and projects for some distance from the external orifice, to a growth no larger than a small seed, and which can only by careful examination with reflected light be discovered lying within the tympanum behind a perforation of the tympanic membrane, polypi of the ear present every variety of form and shape. It will generally be found that they have been preceded by disease of the middle ear, which has resulted in a perforation of the tympanic membrane, purulent catarrh with scarlet fever being the commonest of all forms. The immediately exciting cause of the growth seems to be the purulent discharge which attends this condition. The only variations to this course of events are the very exceptional cases in which they have been discovered after death as a commencing growth in the cavity of the tympanum with the membrane entire, and the perhaps hardly more frequent occasions where they appear in the external auditory meatus independently of any previous tympanic disease.

No purpose is served by an artificial classification of these polypi. In structure they are all fibro-cellular, and have the appearance of fibrous tissue at different stages of development, the fibrous element predominating at one time and the cellular at another; the proportion which either element bears to the other appearing to depend on the age of the tumours rather than on their relative denseness or size, or even on the part of the ear from which they arise.

Dr. Whipham, curator of the museum at St. George's Hospital, has on various occasions made sections of polypi which I have removed, and reported on them. I here give two examples.

No. 1 was a polypus arising from the roof of the tympanitic cavity, projecting into the meatus, and had been, as far as I could judge from the history, of about twelve months' growth. "A fibro-cellular growth, resembling the early form of fibrous tissue. Delicate fibrillated stroma, the fibrillæ having a tendency to arrange themselves in parallel lines. In some parts the stroma is finely granular; cells not very numerous—some round, some oval in shape, beginning to become elongated. Here and there in the section were scattered oil-globules."

No. 2 was a small polypus of recent origin, arising from the meatus; probably of two or three months' growth. "Numerous small nuclei, scattered without much definite arrangement in a fibrous stroma of scarcely so delicate structure as in the former case; here and there cells of considerable size, many distinctly elongated where the fibrous tissue is more advanced."

They are at times very vascular, and when they are so there is a good deal of bleeding after their removal. When they do not attain any considerable size, a very common appearance for them to present is a red, fleshy, globular mass projecting through a perforation in the membrane. In such a case they grow from the cavity of the tympanum, and from their situation, by preventing the escape of discharge, sometimes thus become the indirect cause of death in those fatal cases where the lateral sinus and brain become involved.

The reasons for which their removal generally is demanded are the continuous offensive discharge from the ear and deafness, both of which symptoms are their invariable accompaniments. The most usual position from which aural polypi arise is some part of the walls or roof of the tympanum; they also spring from the external meatus and from the tympanic membrane itself. After one polypus has been removed from the cavity of the tympanum, it not unfrequently happens that one or even two more are brought into view. In looking down a speculum where the membrane has been completely destroyed it is not always easy to decide at first sight whether the red mass at the bottom of the meatus is a small polypus or simply granulations on the lining membrane of tympanum. This, however, is ascertained by examination with a probe, when a polypus will be found to move under the touch. Indeed, this should always be practised so as to determine, as nearly as possible, the point from which the growth arises.

The indications for treatment of aural polypi are three-fold. Firstly, to remove them; secondly, to apply caustics to their root regularly and for a sufficiently long time, to ensure against their reproduction; and, thirdly, when they exist with a perforation, to keep the tympanum clear of secretion, and to induce a more healthy condition of the lining membrane of this cavity and the Eustachian tube; in short, in addition to directing attention to the polypus, the case must be treated as an ordinary perforation of the tympanic membrane.

It is most desirable that the tediousness of these cases should be recognised. Although, after removal, a few applications of some caustic will occasionally eradicate them, and this especially when they arise from the external meatus, the treatment will often extend over several weeks, or even months; the time occupied depending upon the tendency to reproduction which the growth manifests.

Judging from the cases which present themselves where polypi were taken away some years ago and have recurred, only to be again taken away and again to reappear, my conviction is that until late years the perseverance necessary for the successful termination of this class of cases was but imperfectly estimated.

With regard to removing the polypus. If it be very large, the best instrument is a Wild's snare armed with fine gimp. If of moderate size or small, simple rectangular forceps, meeting at the points by two rings, are the most convenient. The lever ring forceps of Toynbee are not often necessary, and at the best will only grasp a very small growth. While using any kind, or in applying caustics, the meatus should be illuminated with diffused daylight through the largest speculum that it will hold, the reflector being worn on the forehead and fastened with a band round the head; both hands will thus be quite free. The most effective caustic I find to be chloro-acetic acid, applied on a very small camel's-hair brush. In using this, care should be taken not to touch the meatus; and provided this is done, very little pain is felt; but if this should happen, it is relieved at once by syringing with warm water. Potassa fusa and chloride of zinc are not so manageable, and the former is liable to spread on to more parts than it is necessary should be touched. Nitrate of silver is not sufficiently powerful, but it is a very useful caustic to apply to granulations on the membrane or on the surface of the cavity of the tympanum. The liquor plumbi is useful for the same purpose, and may be applied with a camel'-hair brush, or in the form of a piece of cotton-wool soaked in it and worn for a few hours every day pressed on to the part. It is well, before any of these applications, to dry the part carefully with cotton-wool.

A plan of cleansing the middle ear of secretion and applying solutions to the whole extent of the lining membrane in cases of perforation, by forcing them through the tympanum and Eustachian tube by means of a syringe with the nozzle guarded by india-rubber, and fitting the external meatus, I have described elsewhere; and in all cases where there is a perforation of the membrane and a pervious Eustachian tube, it is most useful. The patient may also be instructed to practise a plan which I first saw Dr. Joseph Gruber at Vienna teach at his clinique to patients with perforations —viz., to incline the head to the affected side; and, after a drachm or so of the lotion to be used has been passed into the inferior nasal meatus of the same side with a small glass syringe, to close the nostrils firmly with the fingers, and the mouth also being closed, to blow vigorously, when the fluid will run through the Eustachian tube, and out of the perforation; thus cleansing the lining membrane in its course. When neither of these plans succeed—and occasionally they fail on account of some temporary obstruction,—the Eustachian catheter may be employed with an india-rubber bag fitting into its open end; and thus fluids may be made to pass directly through the Eustachian tube into the tympanum, and through the perforation. Speaking generally, however, I should like to be understood to imply that no pains should be spared to keep the cavity of the tympanum thoroughly cleansed; and, in case of an adult, no trouble which has this in view should be grudged by the patient. He should be provided with a syringe which he can use without fear of hurting himself, and with this he should wash the ear twice a day. Lotions (a very good one is sulphate of zinc and solution of opium, four grains and ten minims respectively to the ounce) should be used in the following way:—The meatus is filled with the lotion, previously warmed, and with the affected side uppermost and horizontal, let the patient blow through the perforation; this will cause the fluid to bubble in the meatus, and, on ceasing to blow, some little of it will pass into the throat, and thus it will come in contact with all parts of the lining membrane of the middle ear. Observation teaches me to be more hopeful as to the results of treatment in cases of perforation when they are accompanied by polypi than when they are not. I cannot in any way account for this, any more than I am able to do—judging simply by appearances as regards size, shape, &c.—for the immense variations in improvements to hearing that are met with after treatment in all cases of perforation. When the lining membrane of the tympana and Eustachian tubes becomes more healthy, the improvements that take place, with regard to the hearing power, are with some very great indeed, while with others they are scarcely perceptible. With these latter a careful examination with the tuning-fork, the history, and some subjective symptoms—tinnitus, worse hearing after fatigue, &c.—will frequently detect a nervous lesion accompanying the tympanic disease.

In the following very short abstract of seven cases it was not found necessary in any to use either form of artificial tympanic membrane—viz., Toynbee's, or the preferable one of cotton wool. They are chosen from my notes because, from the fact of the treatment extending over a very long period with some compared with others, the time and trouble which are sometimes demanded in order to treat an aural polypus with lasting success are shown.

The method of examination now universally employed is diffused daylight reflected from a concave mirror down a tubular speculum; and in passing air into the tympana, or through them in case of a perforation, Politzer's plan, of sending a stream of air through the inferior meatus of the nose during the act of swallowing, is the simplest, the Eustachian catheter being only occasionally required.

CASE 1.—June 22nd, 1870. G. P——, a boy aged sixteen. Discharge from right ear from infancy. Watch heard at three inches from the ear, and very deaf to conversation. The natural position of the membrane occupied by a polypus. This was removed with forceps, and he attended three times a week till July 25th, and three times between then and August 14th, when he was dismissed, the discharge having ceased, and the hearing for conversation being very good. The membrane on the other side was perforated; but, although the discharge from the ear ceased, the hearing improved very little from treatment.

CASE 2.—July 20th, 1870. J. G——, aged twenty-two. Discharge from right ear after measles in childhood. Watch heard at two inches from the ear. A fleshy-looking polypus filled up the meatus. It was removed, and he was under treatment till August 17th, when a second small polypus appeared within the tympanum. This was taken away, and he attended once a week till Sept. 29th, the discharge having ceased, and the watch being heard at eight feet, and conversation very well. In this case, too, the membrane on the other side was perforated, but the hearing did not improve under treatment.

Case 3.—April 19th, 1870. C. F. P——, male, aged eighteen. Discharge from right ear from a child. A large polypus projecting from the external meatus. Watch not head on contact. He attended, after the polypus was removed, three times a week till May 10th; aften then, sometimes once and at others twice a week till June 27th; from that date, occasionally at intervals of five or six weeks for six months. The perforation was very small, and the cavity of the tympanum became filled with discharge and made him deaf for the time; this caused the case to be so tedious. When dismissed he could hear conversation very well.

Case 4.—M. M——, female, aged twenty-two. After scarlet fever. A perforation, and a polypus filling the meatus on the right side. Watch not heard on contact. Polypus removed. She attended once a week for fourteen weeks, when she was dismissed, the discharge having ceased. Although she heard the watch at five feet, the improvement for conversation was not proportionately increased.

Case 5.—W. H——, male, aged nineteen. A perforation and polypus after five years of discharge from the right ear. Watch heard on close contact. Attended twice a week for two months, and was dismissed, the discharge having ceased, and hearing increased to six feet with the watch, and conversation very well.

When a polypus grows from the meatus without a perforation of the membrane (a rare state of things), it is just as easy to prevent its regrowth as it is difficult when it arises from the tympanic cavity with a perforation. This occurred in

Case 6, where Mr. T——, aged twenty-six, who could hear well four weeks before I saw him, could then only hear a watch at one inch from the ear, and had a polypus growing from the meatus, and blocking up the orifice. The polypus was removed, and showed no disposition to grow again, the temporary deafness being only due to closure of the meatus. In ten days no traces of it were to be seen. The membrane was quite healthy.

Case 7.—E. L——, aged twenty-six. After catarrh of the tympanum without a perforation of the membrane. In this example a small polypus could be seen in close apposition to the membrane, and arising from the meatus. This was taken away. Caustic applied five times, and the growth was quite eradicated. The hearing, however, did not improve, the deafness depending on the condition of the cavity of the tympanum remaining after the catarrh.

Grosvenor-street, November, 1871.

ON THE

DIFFERENTIAL DIAGNOSIS OF CHRONIC INFLAMMATIONS OF THE LARYNX.

By Morell Mackenzie, M.D. Lond.,

Physician to the Hospital for Diseases of the Throat, and Senior Assistant-Physician to the London Hospital.

Although the removal of growths from the larynx, and the restoration of the voice in cases of functional aphonia by faradisation of the vocal cords, are, perhaps, the most striking results which the invention of the laryngoscope has brought about, the accurate diagnosis of chronic affections is really the most important annexation which that instrument has accomplished. New formations of the larynx, in the circumscribed sense of the word, are necessarily rare, and their treatment, *per vias naturales*, must of necessity remain in the hands of the few. Functional aphonia also may often be successfully treated by general measures; but the prospects of thousands of patients depend on whether a chronic disease of the larynx be simple or benign, whether a thickening be catarrhal or scrofulous, whether an ulcer be syphilitic or cancerous. With the aid of the laryngeal mirror alone, and often when other guides are altogether wanting, the practitioner may be able to promise his patient a speedy and complete recovery; or he may be obliged to inform him that a loss of vocal function will necessarily result from the disease; or he may even be forced to acknowledge that medical art cannot arrest the fatal malady. Any well-educated practitioner who can give a few weeks' study to diseases of the larynx, as seen with the laryngoscope, can easily acquire sufficient dexterity to form a reliable prognosis. Of course treatment may be of great service in some of the affections referred to, and the accurate application of remedies may be effected after a little practice; but the reputation of a professional man so often depends on prognosis, that, for that purpose alone, every practitioner engaged in the general treatment of disease should learn to use the laryngoscope.

The affections which have to be considered are: simple chronic inflammation, laryngeal phthisis, syphilis, and cancer.

In dealing with these affections the following features will be taken into consideration:—

First. The part of the larynx most liable to each kind of the disease.

Secondly. The organic character of the disease. Under this head will be considered—(*a*) the kind of tumefaction; (*b*) the coloration; (*c*) the character of the ulceration (when present); (*d*) the amount of displacement; and (*e*) the condition of the contiguous parts.

Thirdly. The functional impairment—i. e., alteration of voice, presence of cough, embarrassment of respiration, and difficulty of swallowing.

Simple Chronic Inflammation.

1st. The vocal cords are most frequently affected, next the ventricular bands, then the epiglottis, and occasionally, but rarely, the arytenoid cartilages and inter-artenoid fold. The posterior or cartilaginous portions of the vocal cords are rather more often attacked with simple inflammation than the anterior portions.

2nd. (*a*) The amount of tumefaction depends on the precise seat of the inflammation. As regards the vocal cords, there is generally nothing more than a little irregularity of the surface. The ventricular bands, being more lax in tissue, are more likely to be swollen. The epiglottis is generally but slightly tumefied, though there is often considerable capillary engorgement. (*b*) The condition being one of simple hyperæmia, the color of the mucous membrane is more or less bright-red, according to the activity of the process. (*c*) The ulceration is generally very slight, and partakes rather of the character of an erosion. When the vocal cords are thus affected, the ulcers appear as small white specks. This peculiarity is due to the fact that the mucous surface is destroyed, and the white fibrous structure of the cord is exposed. (*d*) There is no displacement. (*e*) The pharynx is frequently congested and relaxed, and there is often some elongation of the uvula.

3rd. There is generally hoarseness and fatigue of the voice. There is seldom cough; but when present it is of an irritative character. The respiration is never embarrassed, nor is there often dysphagia. When this last symptom occurs, it generally indicates that the inter-arytenoid fold is inflamed.

Laryngeal Phthisis.

1st. The ary-epiglottic folds are, above all parts, the most liable to scrofulous deposits. The epiglottis stands next in order of frequency. The posterior extremity of the ventricular bands and the vocal cords are also not unfrequently diseased.

2nd. (*a*) The tumefaction is generally of a diffused character; that is to say, there is no distinct line of demarcation. When the ary-epiglottic folds are affected, a swelling first takes place around the cartilage of Wrisberg and capitulum Santorini. (Fig. 1.) As this tumefaction increases, the projection of the cartilages is lost sight of, and a pyriform swelling is produced. (Fig. 2.) The epiglottis, when attacked, is invariably considerably thickened, and, in an advanced stage, has a turban-like appearance. (Fig. 3.) This woodcut is very characteristic of an advanced case of laryngeal phthisis, in which the interior of the larynx is entirely hidden by the thickened valve, and nothing is seen but a portion of the pyriform swellings. The ventricular bands are irregularly swollen, and often merge into the ary-epiglottic folds. The vocal cords when thickened have a rugged, fleshy appearance. (*b*) The characteristic of the color of laryngeal phthisis is the extreme paleness of the mucous membrane of all the parts, except when the disease affects the vocal cords, and in that case these structures are purplish-red. (*c*) Ulceration in this disease is one of the later phenomena, and is almost invariably preceded by thickening. The ulcers are generally small and comparatively superficial. When the glandulæ are affected, a very characteristic worm-eaten appearance is produced. (*d*) The contiguous parts are not generally implicated, except that there is often some ulceration of the trachea; and it will be understood that in nearly all cases, sooner or later, the lungs become diseased, if they have not been primarily affected.

FIG. 1.

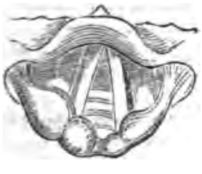
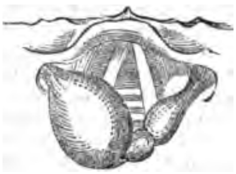
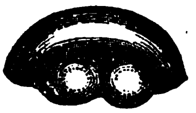


FIG. 2.



[For the sake of comparison, the disease is represented in the woodcuts as unilateral; it more often happens, however, that both sides are more or less affected.]

FIG. 3.



3rd. The voice is generally hoarse in the early stage, aphonic in advanced disease; but cases occasionally occur where the epiglottis is alone affected, in which there is little or no alteration in the voice. Cough, accompanied with a slight amount of expectoration, is one of the most frequent and troublesome symptoms. The respiration is sometimes seriously embarrassed. Dysphagia is a most distressing characteristic of laryngeal phthisis in all instances in which the epiglottis is attacked. In such cases fluid food frequently passes into the larynx, and not only gives rise to convulsive paroxysms of cough, but it is also often ejected through the nostrils.

Syphilis.

The so-called *secondary* phenomena of syphilis are essentially exanthematous in character, and tend to spontaneous subsidence; they need not, therefore, be considered in this article.

In *tertiary syphilitic* disease,—

1st. The epiglottis is the most frequent seat. Next in order of frequency the vocal cords and cavity of the larynx are attacked, and more rarely the mucous membrane covering the arytenoid cartilages.

2nd. (*a*) There is seldom much tumefaction in the early stage; but when present, it is generally considerable, and prone to take on an œdematous character. Considerable ulceration may, however, take place without any previous swelling. (*b*) There is almost always active hyperæmia of the mucous membrane. The epiglottis often loses its buff cartilaginous color, and its normal configuration, and even assumes the appearance of a fleshy tumour. (*c*) Not only is ulceration the most characteristic feature of tertiary syphilis in the larynx, but the ulceration is itself very distinctive. It is rapid, deep, and extensive; it not only destroys the mucous and submucous tissues, but often attacks the muscles and perichondrium. The ulcers are generally covered with mucus, and thus have a yellowish appearance; but when this coating is removed, the surface is seen to be more or less red in color. There is not, as a rule, any elevation of the edges, nor any increased vascularity of the margin of the ulcer. (*d*) Displacement is a frequent *sequel* of syphilitic inflammation, and is invariably the result of cicatrisation. (*e*) There is occasionally ulceration of the walls of the pharynx, but generally the pharyngeal ulceration has occurred at an earlier period of the disease, and has undergone the healing process.

3rd. The voice is very often impaired. There is frequently difficulty in respiration, but seldom any cough when the epiglottis is ulcerated, dysphagia is a very prominent symptom, but after the ulceration is healed, even through the valve has been almost entirely destroyed, the power of swallowing is perfectly restored.

Cancer of the Larynx.

This disease is extremely uncommon, as compared with either the simple or the specific affections already described.

1st. When present as a primary disease, it occurs in one of two forms—either as an epithelial neoplasm projecting into the cavity of the larynx, or as a rodent ulcer attacking the arytenoid cartilages, their posterior surface, the hyoid fossa, or the epiglottis. The neoplasms are altogether different from chronic inflammation, and do not require elucidation in this article.

2nd. (*a*) In the ulcerative cases there is generally but little tumefaction, except when accidental œdema occurs. (*b*, *c*) The ulcers are small, sharply circumscribed, and, though exhibiting little disposition to spread, rarely undergo a healing process. They are of an ashy grey, or greenish color, but when attacking the cartilages, the base of the ulcer is almost black from caries of the subjacent parts. There is but scanty secretion of mucous. The edges have an angry-red color, and are generally much thickened. (*d*) Displacement is a very common and a rather early symptom of malignant ulceration of the larynx, owing to disease of the adjacent parts, and to glandular enlargements. (*e*) Even though cancer in the larynx be primary, the pharynx and other contiguous parts are frequently similarly affected.

3rd. As the ulcerative form of the disease generally attacks the frame work of the larynx, the vocal function is seldom impaired, except in the later stages. The same observation explains the absence of cough. The respiration is only affected when considerable tumefaction has taken place. The very reasons which account for the voice being spared explain the early embarrassment of deglutition.

In analysing the foregoing remarks, it will be seen that the functional signs are uncertain, but that the organic changes as reflected in the laryngeal mirror may be implicitly relied upon. In *simple chronic inflammation* the appearances are superficial and general. In *phthisis* pale solid pyriform swelling of the ary-epiglottic folds are typical, whilst turban-like tumefaction of the epiglottis is not less characteristic. The other varieties herein referred to are less common and less characteristic; but it may be accepted as an axiom, that in phthisis thickening invariably precedes ulceration. In *syphilis* extensive rapid ulceration is the distinctive manifestation, and the epiglottis is its most common seat. Here ulceration most frequently occurs without any previous thickening, but subsequent narrowing of the laryngeal canal and displacement are not uncommon. In *cancer* the framework of the larynx is most commonly attacked, and the character of the disease is its slow intractable ulceration, and the disposition of the contiguous parts to become affected.

December, 1871.

ON A

CASE OF DEFECT OF SPEECH FOLLOWING A RIGHT-SIDED CONVULSION.

By J. Hughlings Jackson, M.D., F.R.C.P.,

Physician to the London Hospital, and to the Hospital for the Epileptic and Paralysed.

In the "Mirror" of The Lancet, for June, 1871, is a brief note of remarks I made on cases of temporary loss and defect of speech with certain epileptiform seizures (epileptic aphasia). The following case, there referred to, was reported by Mr. Stephen Mackenzie. It is not often that one has the chance of observing a patient who talks badly soon after an epileptiform seizure, and therefore this case has great value. In most cases the defect after the fit, so far as one can learn from the patient's friends, is slight, or the accounts we receive are very vague. The great merit of Mr. Mackenzie's report is that it is a faithful record of what actually took place, and possesses infinitely more value than if he had contented himself with mere statements as to the nature of the defect. I have, however, one critical remark to make on it. The patient was not asked to write during the continuance of the defect of speech; but notwithstanding this omission, the report is the most valuable one I possess of the manner of talking after a one-sided convulsion. Hence it may seem most ungracious of me to draw attention to the omission; but I need not hesitate, for the reporter, knowing thoroughly well the importance of investigating the writing powers of patients who have defect of speech, would himself, were he publishing the case, be certain to draw attention to his omission. As mentioned in the "Mirror," those fits in which speech is much affected usually start in the right cheek, but it must be admitted that occasionally there is temporary defect of speech when the fit begins in the hand. In the following case it was only ascertainable that the fit affected the right side.

Martha Faughty, aged seventeen. Her name is given because it is possible that the frequent use of the word "forty" may, so to speak, have been determined by the fact that the similarly sounding word, Faughty, would be one of the most automatic in her vocabulary. Mr. Mackenzie begins his report by alluding to trouble she had had from a wrongful accusation. The patient was a healthy-looking woman, and, as will be seen later in the report, no evidence of organic disease was discovered in the heart or kidneys. The rest of the report is in Mr. Stephen Mackenzie's words.

"Nov. 18th, 1870.—Her uncle says that the patient visited him to-day. During or after tea she had what he calls a fainting fit—that is to say, she turned very pale, and seemed to lose, at least partially, her consciousness, not answering when spoken to, and being unable to swallow water. She was laid down, and when a little recovered her uncle proposed to her to go out, thinking the open air would revive her. He got her into the street by supporting her, and whilst walking along she had a fit. He says that the head was drawn down to the right shoulder, the mouth was drawn to the right, and that her right arm was drawn up towards her head and stiffened. He spoke to her, but she could not answer him. He got her into a cab and brought her to the hospital. On her way there she vomited. The nurse in the receiving-room noticed that 'her mouth was drawn,' and that she kept her right arm semi-flexed. When I saw her she was able to walk into the women's dressing-room. She kept her right arm bent so that the hand came to about the waist, whilst the left arm she carried naturally. Her face was very pale, but there was no difference between its two sides. Eyes natural; pupils large, probably not abnormal. I asked her to put out her tongue. She did so readily. It was clean, and not bitten. I then asked her if she was in pain. She said, 'Yes, sir.' When I asked her where, she again said, 'Yes, sir.' When asked what was her name, she said 'No.' When desired to place either of her hands in mine, she did so with each equally well. I then asked, 'Where do you live?' She replied, 'Quite Chris.' I said, interrogatively, 'Quite Chris?' and she replied, 'Twenty Chris.' I then again asked her her name. She replied, 'Thank you, sir.' 'How old are you?' 'Thank you, sir.' 'Are you cold?' 'Yes, sir.' 'Have you had your dinner?' 'Yes, sir.' 'Have you been sick?' 'Yes, sir, having tea-c.' 'Have you any brothers or sisters?' 'N-e-c-k' (spelled rapidly). 'What is to-day?' 'N-e-c-k.' 'Is it Sunday?' 'N-e-c-k.' What was yesterday?' 'C-k.' 'What did you have for dinner?' 'Oh, n-e-c-k.' 'How did you get to your uncle's to-day?' 'Oh, any length.' 'When did you leave your situation?' 'Oh, when you like'—(correcting herself) 'Oh, when you will.' 'What is your mistress's name? 'Oh-n-c.' 'Has she any children?' 'No-c.' 'What is your master's employment?' 'Oh-n-c.' 'Have you been to church to-day?' 'Oh-n-c.' 'What church?' 'That's all.' 'How long have you left it?' 'Oh, altogether yet.'

"Her articulation was clear. She did not seem annoyed at being asked questions, nor at her own replies; and, with very few exceptions, she did not attempt to correct herself. She laid herself down on the couch at full length. She was sent up to bed.

"When visited about half an hour after in the ward, I said, 'What is your name?' She replied 'Martha Faughty.' 'How long have you been in service?' 'Forty years.' 'Where does your master live?' '40, Clerkenwell-green.' 'Where does your father live?' '40, Clerkenwell-green.' '40, Clerkenwell-green?' '41, Clerkenwell-green.' 'What had you for dinner?' 'Forty.' She then answered 'Yes' to several consecutive questions, and then 'No.' 'What are your wages?' 'Forty pounds.' 'How much is that a month?' 'Four pounds.' 'Count twelve.' She did so readily without mistake. 'What is twice twelve?' 'Twenty-four.' 'What is twice forty?' 'Twenty-two.' 'How many pennies are there in a shilling?' 'Twenty.' 'How many threepenny-pieces in a shilling?' 'Sixpence.'

"Her upper extremities were very cold, but the trunk was warm, and the skin dry. She had perfect sensibility and power in both legs, and knew right from left when touched. Feet warm. Perfect sensibility of upper extremities, and could place either hand in mine when asked to do so. Tongue clean, and protruded in middle line. Left pupil slightly larger than right. The difference was extremely slight. Pulse 104, excessively small and feeble, so much so that it was with difficulty found. Respiration 18, almost entirely abdominal, but not at all labored. Temperature: right axilla, 97·8°. Heart and lung sounds normal.

"Louis Mackenzie held up his fingers, and asked how many there were. Sometimes she answered correctly, and sometimes incorrectly. She did not seem able to calculate, for when he held up seven fingers she said 'eight'; and when he asked her how she made that out, she said, 'five before, and two, that's eight.' Though generally right as to the number, she made mistakes as to the addition, as 'five and one is nine.'

"Nov. 14th.—(The following notes are by Mr. Louis Mackenzie).—She recognises me on coming to her bedside, and laughs on my asking her several questions. She answers with perfect ease and accuracy. She says that she woke up this morning about 4 o'clock, and wondered where she was. She states that she went to her uncle's yesterday about 4.30 P.M., having dined at 3. She had a cup of tea at her uncle's, and a small slice of bread and butter. She had swallowed three or four damson stones at her dinner with her pie. She was sick soon after tea, and they took her into the wash-house. Soon afterwards she felt sleepy, and then she remembers nothing more, except that her uncle put his hat on the table. She remembers that we asked her many questions last night, but she does not remember answering at all. She 'remembers listening to all we had to say,' but she said very little herself. She remembers my saying, 'You see she does not know when I hold up my fingers.' Pulse 96; respiration 18; temperature, right axilla 99° F., left 98·8°. Has no pains in the head. No abnormal sounds to be heard at the heart. Urine: specific gravity 1030, acid; no albumen, no sugar.—Evening: Got up for a short time this afternoon, but had to lie down again.

"15th.—Slept well. Tongue clean; appetite good; bowels freely open after an aperient. When she got up to-day she felt giddy, and had a shivering, although she was warm. No nausea nor vomiting. Has no pain in the head, but has pain in her back of an aching character. This is probably connected with menstruation, as she has 'seen nothing' the last five or six weeks. There is a crop of herpes labialis. She cannot see distinctly, as atropine has been put into her eyes. This will perhaps account for her giddiness when up. There are no abnormal ophthalmoscopical appearances. Pulse 92. Respiration 28. Temperature: right axilla, 98·6°; left axilla, 99·4°.—Evening: She states that about mid-day she gradually became very hot, and then broke out into a sweat. There had been slight shivering in the morning; but no distinct rigor nor cardiac murmur. Pulse 96, small but regular. Respiration 18; vesicular murmur is heard all over the chest. Temperature: right axilla, 97·4°; left axilla, 98·5°. Nothing wrong can be felt in the rectum, uterus, or vagina.

"16th.—Says she has an 'awful pain' in the back. Has been perspiring very freely through the night. Pulse 96. Respiration 22. Temperature: left axilla, 98·8°; right axilla, 98·4°.—Evening: She states that she is very hot, and perspires more than ever. Pain in her back worse than ever. Some vesicular spots about the left nostril, which are painful. No sickness. Feels thirsty; tongue clean and moist. Pulse 92. Respiration 20. Temperature in left axilla, 97·2°.

"17th.—Evening: Pulse 104. Respiration 20. Temperature: right axilla, 99°; left axilla, 98·8°. In less pain and feels stronger.

"18th.—Says she feels much better. Began to menstruate this morning, when the pain left her back. When she gets out of bed she feels cold, and has 'pins and needles' in both legs. Pulse 84. Respiration 20. Temperature: left axilla, 98·2°; right axilla, 97·6°.—Evening: Pulse 84. Respiration 20. Temperature: left axilla, 98·2°; right axilla, 97·2°.

"19th.—'Pins and needles' gone. Extremities cold. Pulse 100. Respiration 18. Temperature: right axilla, 99·6°; left axilla, 99·9°.

"21st.—Menstruation ceasing. Went out."

In March, 1871, she was again seen by Mr. Stephen Mackenzie; she had remained well.

What the illness she had on recovery from the nervous seizure was I do not know. It is possible that the case may be considered by some as hysterical or emotional, especially as the poor woman had very good reason to be depressed in spirits from the unjust treatment she had received; but the convulsive seizure, so far as the description enabled me to judge, is not such as occurs in hysterical women, and is like those which occur from disease of the brain. A convulsion which effects one side of the body must, I presume depend on local changes of some kind; and I infer that the location is the region of the corpus striatum. What those changes are there is no evidence to show. Granting the reality of the seizure, it is obvious that there was a sudden discharge of nerve-tissue; but we cannot say by what *pathological* process the local change permitting that discharge was brought about. It may be supposed to depend on embolism or thrombosis; but there is no evidence to support this conclusion. In most cases of convulsion beginning unilaterally there is no evidence to show the nature of the pathological process from which the "discharging lesion" results.

Manchester-square Dec 1871.

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CASES OF EXCISION OF THE KNEE-JOINT AND REMOVAL OF THE OS CALCIS.

By John N. Terry,

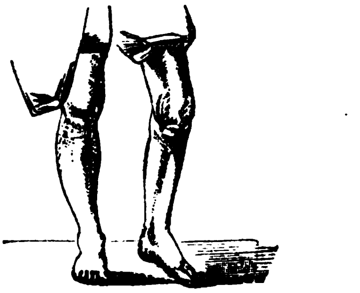
Surgeon to the Bradford Infirmary.

In order that excision of the knee-joint may prove successful, it becomes imperative that the cases should be carefully selected. It is not applicable, for obvious reasons, where the bones entering into the articulation are very extensively diseased; for though I admit a better chance of preserving life would be secured by excision than by amputation, yet the limb would be faulty—as a means of progression, it would be short and almost useless. I do not lay stress upon the integuments being extremely disorganised, nor think that such a state militates with any force against excision, for immediately after the removal of dead bone the soft parts set up a healthy action and recover themselves rapidly.

Excision of the Knee-joint.—D. D——, aged twenty-six years, was admitted into the Bradford Infirmary on the 15th October, 1864, with serious and long-standing disease of the left knee-joint. When quite a child disease of the knee set in, and he continued to be laid up at intervals on account of it. About twelve months previous to his admission into the hospital the symptoms became more aggravated; and when admitted, the joint was much swollen, and the structures around thickened and partially disorganised. Below the knee there was great wasting of the limb; it was considerably bent at the knee, and the slightest attempt at altering its angle caused severe pain.

On the 30th October the patient was put under chloroform, and resection of the joint performed, the horseshoe-shaped incision being adopted. About an inch and a half of the end of the femur, and a quarter of an inch of the head of the tibia, were sawn off. The section was made from behind forwards, and the cut surfaces were found quite healthy. A large quantity of softened gelatinous material was cut away by the scissors, and the patella removed, though not diseased. No vessels required ligaturing. The flaps were brought together by several wire sutures, and the limb put up as recommended by Sir William Fergusson in his work on Operative Surgery.

On January 21st, 1865, eighty-three days after the operation, the bones at the knee being firmly anchylosed, he left the hospital, requiring no other assistance in walking than that of a stick. There has been no great constitutional disturbance, and the result of the operation has proved most satisfactory. I have an opportunity of seeing him daily, and he now leads a very active life. He has perfect use of the limb, and can walk and run with freedom.

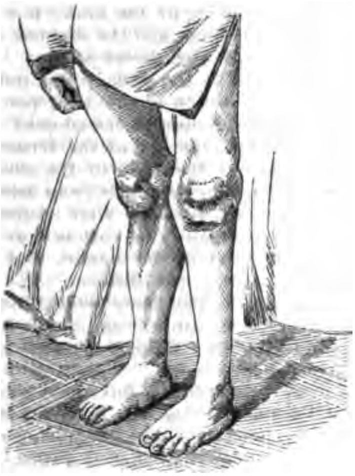


From a photograph taken six years after the operation.

Excision of the Knee-joint.—S. A. S——, twelve years of age, was admitted into the Bradford Infirmary on Jan. 7th, 1868, laboring under strumous disease of the left knee-joint. At the time of his admission the leg was bent at nearly a right angle with the thigh; there was a great deal of swelling round the joint, and several sinuses, extending considerable distances, discharging purulent matter. Pressure or slight movements occasioned acute pain.

As the boy's health suffered in consequence of the irritation of the joint, I recommended its excision. Accordingly that operation was performed on the 24th January. The horseshoe-shaped incision was made; the joint opened into, and found largely disorganised. I removed about an inch of the femur, half an inch of the tibia, also the patella, and carefully got away from around the joint all the tissues that appeared to be in a disorganised condition. Several of the articular vessels required ligaturing. The limb was put up as in the previous case.

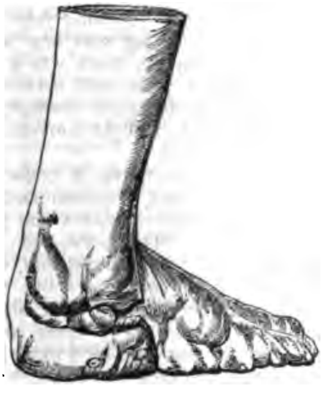
Recovery in this case went on uninterruptedly, and unattended with any marked febrile disturbance. Fourteen weeks after the operation he was able to walk about the wards, having perfect use of the limb, without the aid of either crutch or stick; and a week later he was sent to the Convalescent Hospital. This boy lives near me, and his gait is so slightly altered that it is difficult to detect any imperfection in his walking. He has perfect use of the limb, and can walk and run with freedom.



From a photograph taken two years after the operation.

Removal of the Os Calcis.—W. D——, aged nineteen, was admitted into the Bradford Infirmary for caries of the os calcis, which had been going on for several years. He was quite unable to walk except by the aid of a stick and crutch, and his general health was so much impaired that I recommended his removal for a short period to the Convalescent Hospital.

On his readmission I advised him to have the diseased bone taken away, as he was then in a more favorable condition for undergoing the operation. This was done on the 20th of September, 1863. An incision was begun at the inner edge of the tendo Achillis, dividing it, and drawn forwards, along the outer side of the foot, to a point somewhat in front of the calcaneo-cuboid articulation. A second incision was made across the sole of the foot, commencing at the anterior extremity of the former incision, and continued as far as the inner border of the os calcis. The knife and Fergusson's lion forceps were used freely in the separation of the bone; and I took care to have as much as possible of the periosteum separated with the soft parts. There was a little general oozing of blood, which was arrested by exposing the flaps and keeping ice applied for nearly twenty-four hours.



From a photograph taken six years after the operation.

The patient made a rapid recovery, and he now follows the occupation of traveller to a grocery business. There is nearly entire regeneration of the os calcis. He walks with ease, except that he puts his foot down as if flat-footed.

Bradford, Nov. 1871.

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NOTES OF A CASE OF FATAL POST-PARTUM HÆMORRHAGE, IN WHICH TRANSFUSION OF BLOOD WAS PRACTISED.

By W. S. Playfair, M.D., F.R.C.P.,

Assistant Obstetric Physician to King's College Hospital, and Physician to the Evelina Hospital for Sick Children.

Cases of severe post-partum hæmorrhage in which transfusion of blood is called for are fortunately so rare that our experience of that operation must always be limited. It seems to me of importance that every case in which it is practised should be recorded for the benefit of others, even when, as in the one I am about to relate, this last expedient may not have proved successful. I believe it to be of the utmost importance that our failures should be recorded as faithfully as our successes, and I therefore make no apology for bringing the history of this case under the notice of the profession. It is fair to say, however, that so far as the immediate results of the transfusion were concerned it answered my expectations, if not fully, at least to a sufficient degree to make me think that it would have had the desired effect of rallying the patient had it not been for the unfortunate recurrence of the hæmorrhage, which resisted every method of treatment, even (for the first time in my experience) the repeated injection of a solution of perchloride of iron.

On the 2nd of December, at 11 A.M., I was summoned to Mrs. ——, a patient under the care of my friend Mr. Lattey, of Kensington. She was thirty-three years of age, and the mother of three children, the youngest being seven years of age. She was a lady of delicate health, and had been for long more or less of an invalid. Her labour had been easy, the pains having commenced at 5 A.M., a living and healthy child having been born at 9.45 A.M. The uterus apparently contracted well, although there was some little difficulty about the removal of the placenta. The usual pad and binder were applied. About an hour after delivery, Mr. Lattey being fortunately still in the house, a sudden gush of hæmorrhage commenced. On removing the binder, the uterus was found to be distended with clots. These were removed, and the usual means for arresting the flow were at once resorted to, in the application of which Dr. Pearson, of Phillimore-gardens, lent his aid. Ice-cold water was injected into the uterus, ice in lumps was placed in the cavity of the uterus, and applied externally. Firm pressure was adopted. Ergot was administered by the mouth and rectum, brandy and beef-tea were freely given, and, in fact, every means usually employed were resorted to before my arrival, except the intra-uterine injection of the perchloride of iron, of which there was none in the house, but it was sent for as soon as possible. In the meantime, however, the patient had passed into a state of the utmost danger. When I saw her she was cold and almost pulseless, or rather her pulse was a mere thread, which every now and then could not be felt at all. She had the usual sighing respiration and hiccough of extreme exhaustion. The hæmorrhage had ceased, and the uterus was fairly contracted, but felt spongy and soft as if it would again relax whenever pressure was removed from it; and I thought it best to provide against the recurrence of the hæmorrhage by injecting the uterine cavity freely with the perchloride of iron. It was now about 11.30 o'clock, and the patient went on from bad to worse, although there was no fresh flooding, until 2 P.M., when she seemed on the point of death.

Mr. Lattey, Dr. Pearson, and I, came to the conclusion that her only hope lay in transfusion, which we determined at once to practise. I had read with much interest Dr. Beattie's remarkable case of successful transfusion recently published in the *Dublin Quarterly Journal*, and I had provided myself with the apparatus used by him. It consists, as may be remembered, of a large pipette holding some eight ounces of fluid, to one extremity of which is attached a fine caoutchouc tube, at the end of which was the tube for inserting into the vein, which in my case was that recommended by Dr. Richardson as most suitable for transfusion. The atmospheric pressure is sufficient of itself to drive the fluid through in a steady stream. This seemed to me to be the most simple and satisfactory apparatus we possess, and it answered to perfection in Dr. Beattie's case. I anticipated the best results from it, and on trying it with water the stream was full and abundant; but, as will presently be seen, I was eventually obliged to use an ordinary syringe.

The steps of the operation were precisely those recommended by Dr. Beattie. Mr. Lattey was good enough to supply the blood, which was drawn from his arm in a full stream, and rapidly stirred as it flowed. As soon as the fibrine was separated, it was strained through a piece of fine muslin, previously dipped in boiling water, into a finger-glass floating in a bowl of water at the temperature of 106°. From this it was transferred into the pipette, which, with its attached tubing, was placed in water at the same temperature. All this was done leisurely in an adjoining room, and thus all fuss and flurry were avoided, the advantage of which, under such painful circumstances, can hardly be over-estimated.

We now proceeded to search for a suitable vein in the patient. A fold of skin at the bend of the left elbow was pinched up and divided by a lancet, and at the bottom of the incision thus formed a vein was found, but with considerable difficulty. Underneath this a needle was passed, but this, unfortunately, got displaced by a movement of the patient, and all our endeavors to find it again were fruitless. The difficulty was due to the quantity of adipose tissue, and the entire absence of blood in the veins. Eventually we determined to look for a vein in the right arm rather than waste time in further search. In this we succeeded perfectly, and after passing a probe under the vein, and making an opening into it, we easily introduced the nozzle of the injecting apparatus. To our great surprise, however, but little blood flowed through the instrument. Fancying it might be blocked up by a piece of fibrine in the tube, I removed it, and found it worked perfectly out of the vein, but on reintroducing it a second time met with the same failure. The only explanation I can give of this is that the wall of the vein fell over and blocked up the opening of the nozzle, which is bevelled off the upper surface of the tube, and that the propulsive power was not sufficient to overcome the obstruction. After repeated endeavors I was obliged to desist. I now employed an ordinary ear syringe with a fine nozzle, and with this the transfusion was effected readily. From five to six ounces of defibrinated blood were injected.

A decided improvement now commmenced. Our patient became warmer, breathed more naturally, opened her eyes, and her pulse became distinctly perceptible. She was able to take Brande's essence of beef and brandy. For about an hour the improved state continued, and we were becoming sanguine as to the results. At this time, unhappily, a fresh gush of hæmorrhage took place from the uterus, not to any great extent, but sufficient to prove very serious in the patient's condition. I lost no time in again injecting the perchloride, with the effect of immediately stopping the flow. But the pulse disappeared, and the livid coldness of the extremities previously felt again showed itself. We discussed the advisability of repeating the transfusion; but, as the patient was able to swallow, and had shown some signs of rallying, we determined to postpone it. For about an hour she remained much in the same state, neither losing nor gaining ground, but able to take the beef essence and brandy in small quantities, so that we hoped for the best. I was now obliged to leave her for a short time, Mr. Lattey remaining in attendance. On my return, in about an hour, I found that, during my absence, a fresh flow had taken place, and that a third injection of the perchloride had been made. In the interval there had been no loss at all, the uterus was firmly contracted, and the discharge was sudden and unexpected. It sufficed, however, to turn the scale, and the patient had died a few minutes before my return, three hours after the transfusion and seven hours after delivery.

In considering the lessons to be learnt from this melancholy case, the most important seems to me to be the farther evidence it gives of the importance of defibrinating the blood before commencing the operation. My experience on this point fully corroborates that of Dr. Beattie in the case already alluded to. The process by which this is effected is extremely simple, and the fact of not being hurried by the necessity of completing the transfusion before coagulation has begun is of immense importance in an operation every step of which should be conducted leisurely and calmly. It is certain that the removal of the fibrine does not in any way deteriorate the blood for the purposes of transfusion, and it is an expedient the adoption of which I should strongly recommend. I do not look upon the failure of the operation as any argument against its adoption in suitable cases. On the contrary, it answered the purpose of rallying the patient when at the point of death, and of keeping her alive for several hours. The sudden and unexpected gush of hæmorrhage, occurring after the injection of the perchloride, was the turning point of the case, an occurrence of great rarity after that invaluable hæmostatic has been used in post-partum flooding. The case was also in other respects a most unfavorable one, the patient being very weakly, and much debilitated by previous illness.

I regret that Dr. Beattie's simple and inexpensive instrument should have failed to act satisfactorily. A transfusion instrument that can be purchased for a few shillings and is within the reach of all is a great desideratum, since many lives have been lost because practitioners have not the means of transfusion at hand. In some of the successful cases an ordinary syringe with a fine nozzle was used, and this I found to answer perfectly. The objections to this disappear when defibrinated blood is used, and such an instrument is in the armamentarium of every practitioner, or could always be procured at the nearest druggist's shop.

Curzon-street, Mayfair, December, 1871.

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A CASE OF

ABDOMINAL PREGNANCY; RUPTURE OF THE GREAT INTESTINE; CÆSAREAN OPERATION AND CURE.

By J. H. Adams, M.D.,

Surgeon to the Sorocaba Hospital, Brazils.

The following is such a rare case (if ever it has been met with by any of my surgical brethren) that I cannot refrain from communicating it to the widely-circulated Lancet, being sure that it will be of interest to the medical profession.

Blandina de Barros, a mulatta, aged thirty, married, to all appearance of a sound and well-constructed frame, was admitted into the hospital 18th October last, being, as she herself thought, between the seventh and ninth month of pregnancy. She complained of great bodily suffering and pains through the whole of her system, particularly at the pit of the stomach and loins. When she completed the ninth month and expected to be delivered, her pains ceased, and the whole of her body became œdematous to an alarming degree.

Previous to her admission into the hospital she had consulted two Brazilian doctors, who diagnosticated her disease to be ovaritis and metritis, and treated her with iodine and mercury, &c., and externally with leeches, blisters, and embrocations for nearly four months, until the poor creature became quite a skeleton, without, however, any reduction in the supposed tumour. At last she had such a bad smell from the body that no one would attend her. She had also an ulceration and fistula near the navel, which discharged matter and fæces; likewise, for about a month a discharge of pus from the rectum in great quantity.

In this state she was sent to the hospital. I examined the swelling or tumour, and it appeared to me that the woman was right about her pregnancy. I explored the uterus and found it empty. I made an injection through the fistula near the navel into the abdomen, and with light pressure the liquid returned immediately, bringing with it matter and fæces. I thought I could detect some foreign body at the mouth of the fistula. I took a pair of small forceps and extracted it, and, to my surprise, the radius of the child in a few seconds appeared. I extracted also the ulna. Another slight compression brought nothing but matter. The greatest volume of the tumour being more than four inches to the right further down, I proposed to the patient to operate, and she consented.

At 2 P.M. the same day I administered chloroform with some reluctance, considering her weak state (pulse 50), and performed the Cæsarean operation. Having made an incision six inches long, and cut through the walls of the abdomen and peritoneum, I found there was a complete adherence with the walls and no cavity. I tried to pass my finger into the opening, but found a resistance every way. I cleaned away with a sponge the clots of blood and some liquid, and made a minute examination. I found quite a fabric of cells and foreign membranes attached to the walls of the peritoneum, cellular membranes and a fibrous network with vessels forming a second diaphragm, enveloping the intestines and also keeping the fœtus from view. The ovaries were quite shrivelled up. I could not attempt such an enormous dissection, so I determined to separate what I could by the finger to get at the fœtus. Having done this, I extracted the child in a state of complete decomposition or putrefaction. The body was intact, but it had parted from the head, hands, and feet. I found the head, the bones of which were complete. I could not find the hands or feet in the cavity. I concluded that, as the radius and ulna had been discharged, so the tarsus and metacarpus and phalanges might have passed by the rectum, as she had for upwards of a month discharged a great quantity of matter before being brought into hospital. Having cleaned out the cavity with a sponge dipped in chlorinated soda, camphorated oil and compound tincture of benzoin, I made an exploration by the uterus, rectum, and incision. The uterus was in its normal state, but did not communicate with the abdomen except through the Fallopian and ovarian tubes. Both ovaries were shrivelled up and almost atrophied. The rectum contained three fistulæ, cummunicating with the cavity of the abdomen, through which the matter and fæces were discharged. The large intestine had ruptured, ulcerated, and separated. I could not detect the end, being buried in the mass of cells. The whole of the intestines were completely hidden in new formed membranes. The sphincter appeared to have lost its contractile power, the anus being quite shrivelled up. The intestine appeared to have parted near the anal orifice. On the left side of the ovary was a fistulous opening, which afterwards proved to be a point of the intestine. The state of the uterus, the vessels, and foreign membranes that I met with in the peritoneum, observed through the incision, proves to me that the pregnancy had been abdominal. Having closed the wound and put on the bandages, I ordered the patient three grains of opium and one ounce of aromatic infusion for the night.

The next morning she told me that she had not slept so well and passed such a good night for many months.

Oct. 14th.—Pulse 55, very weak. Has had chills; complains of great exhaustion. Ordered port wine and quinine. Great discharge of matter, mixed with fæces, from the incision.

From this date she gradually improved, till the 10th of November, when the incision had almost cicatrised, and the fæces were discharged through the rectum by the anus. She complained of a little pain in the canal, but walked about and wished to leave the hospital.

Sorocaba, Brazils, 15th November, 1871.

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A NEW MIDWIFERY FORCEPS.

By John Barclay, M.D.,

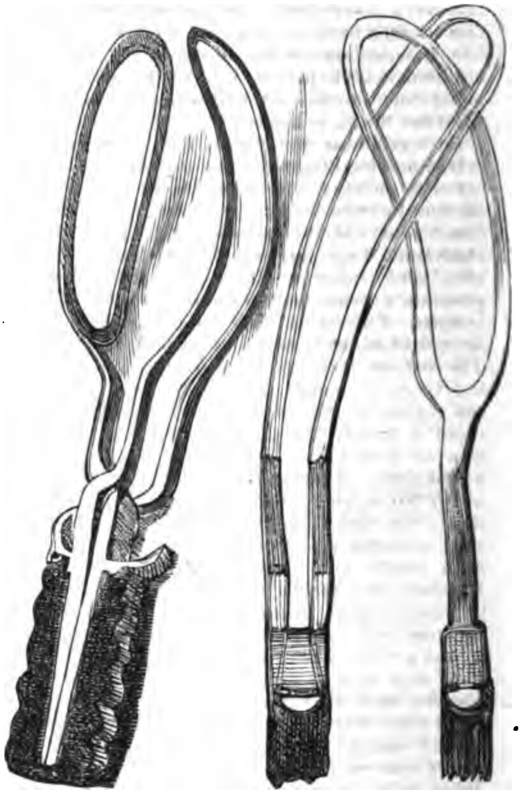
Lately one of the Assistant-Professors in the Aberdeen University.

For the first year or two after commencing practice I followed the advice given to his class by the late Dr. Dyce, Professor of Midwifery in the University of Aberdeen, and used Ziegler's forceps, the distinguishing characteristic of which consists in the male and female blades. But I was not long in finding out that, though the facility of the application of this instrument was very great, it was nevertheless very inefficient in all but very easy cases. In fact, the extreme readiness and facility with which Ziegler's forceps can be applied constitutes its only recommendation. Its disadvantages are the following:—It is not quite long enough for long forceps cases; the blades are too weak, being exceedingly apt to slip; the double curve is wanting, which of course is a great objection; and it does not possess sufficient means for powerful traction.

Now Simpson's long forceps has none of these objections; but then, though men of some experience may be able to apply it quickly and lock it without any difficulty, I have known many beginners, and my own experience at first with it was the same, considerably annoyed by the two halves twisting backwards, thereby rendering the locking of them a very tedious matter. And if there is one thing more than another that shakes the confidence of the patient's friends in the attendant, it is repeated attempts and failures in the application of the forceps.

I accordingly explained to Mr. Young, surgical instrument maker, Edinburgh, that I wanted a forceps that should combine the principle of Ziegler's male and female blades with the length, strength, curves of blades and cross-stops of Simpson's. He accordingly made for me a beautiful instrument, which, I think, combines all the advantages of the two. The handles are precisely the same as Simpson's; the cross-stops are the same in form also as in his, except that they are made to rise and fall at will, like the guard of a carving fork; the shank and lock are Ziegler's, only much stronger; and the blades are male and female, as in Zeigler's, only they are of the same strength as Simpson's, and have his double curve.

For more than two years, in all sorts of cases suitable for the forceps, both long and short, I have used this alone, and can only say that it has fulfilled all my anticipations. It is quite as easily applied and locked as Ziegler's, and, indeed, it would be rather a difficult matter to avoid locking it. The male blade is introduced first, and above; then the fenestrum of the other is slipped over the handle of the first, and as the latter glides along the curve of the sacrum upon the child's head, it is so guided by the first as to lock with absolute certainty and precision. The stops which till now are lying against the shank are pulled up, and traction made at the proper time. During the application the instrument is Zeigler's, *when* applied it is Simpson's.



The measurements of this forceps are as follows:—Length of handle, 4 in.; from the handle to the lock, 1¼ in.; from the lock to the beginning of the curve of the blade, 2 in.; and from the last-mentioned point to the end of the blade, 6¾ in.—in all, 14 in.; distance between the points of the blades, 1 in.

Banff, December, 1871.

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CASE OF SPASMODIC STRICTURE OF THE ŒSOPHAGUS.

By John Eaton, M.D., C.M.

On the evening of December 11th, 1871, Mrs. A. F——, aged thirty-four years, called to consult me regarding her throat. She presented a careworn, enfeebled aspect, and her complexion was anæmic, waxy, almost cachectic; her pulse was 96 and weak; tongue flabby and slightly coated; skin cool. She complained of a burning pain, constant, but not always of the same severity, situated in the region of the submaxillary gland; and, whenever she attempted deglutition, a pain also at the left side of the gullet, behind the cricoid cartilage. Patient has for nine years experienced symptoms of atonic dyspepsia attended with much flatulence, and it is seven years since she began to be affected with a sensation of choking in the throat; the latter got gradually worse till three years ago, when she became totally unable to swallow anything except liquids, or very soft, well-masticated food. Patient is much alarmed about her case, and very despondent; the slightest noise startles and excites her; in short, she is hysterical. She has, however, no uterine disorder, has borne seven children at full period, all healthy, the youngest being four months old, and with the above exceptions her health is normal.

On examining the fauces and pharynx I could detect nothing abnormal, but, on passing a probang into the gullet, found an obstruction when the end of the instrument had passed six inches and a quarter beyond the incisor teeth. I prescribed a chalybeate tonic, and on the evening of the 14th ult., after using a No. 8 gum-elastic catheter, passed a softened and well-oiled No. 1 œsophagus bougie. The latter, after being obstructed for a second or so, went suddenly through the stricture. The patient then complained of pain at the part, but there no longer existed any such sensation in the region of the left submaxillary gland. Next night I passed No. 2, with slight irritation, and now all pain was referred to the right side of the œsophagus. A larger bougie was used every night up to No. 4, each successive one with less irritation. On first attempting to pass No. 5, the tube was apparently contracted, for the bougie, even with gentle force applied, would not penetrate. I waited two nights ere I tried it again, and then it passed easily, showing that in the interval the stricture had spontaneously dilated, and hence that it was a spasmodic one. From this time the patient was able to swallow nearly as well as ever she did, and there was slight soreness only for a short time after passing a bougie. Next night Nos. 6 and 7 passed; two nights thereafter, inadvertently, No. 9 was used instead of No. 8, and with a little difficulty it also passed; three nights thereafter, on December 25th, I passed easily Nos. 10, 11, and 12.

Dec. 27th.—Passed No. 12 again. The œsophagus is now of nearly the same calibre throughout, and it requires a little care to detect the position of the stricture; there is no pain after passing an instrument; the function of deglutition is performed with ease, and patient is taking plenty of nourishment.

At date of writing (9th inst.) the gullet remains freely pervious to No. 12 bougie, and painless. Latterly patient has been taking quinine, iron, &c.; the flatulence is nearly gone; her pulse is stronger and less frequent, and she is much improved in appearance.

This case clearly shows the importance, in certain cases of dysphagia, of using an œsophagus bougie to ascertain whether stricture exists. Such a process done earlier would have saved this patient years of discomfort.

Airdrie, Jan. 1872.

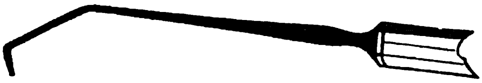
AIDS IN OPHTHALMIC PRACTICE.

By George Cowell, F.R.C.S.,

Senior Assistant-Surgeon to the Westminster Hospital, Assistant-Surgeon to the Royal Westminster Ophthalmic Hospital, and Surgeon to the Victoria Hospital for Children.

NEW STRABISMUS-HOOK.

The strabismus-hook, of which the accompanying woodcut is a representation (exact size), is a modification of Von Graefe's hook. The end from the extremity to the bend is much shortened, the bend is more abrupt, and a second bend at a very much larger angle is placed at about five lines from the former. To place the tendon of the muscle to be divided on the stretch, the portion of the hook between the two bends is held at right angles to it, and the end very effectually prevents the escape of a portion of the fibres before the tendon is completely divided, an occurrence which is the cause of a large percentage of the failures in operations for squint. The second bend enables the operator to get the handle of the instrument well out of the way of any interference with the play of the scissors. I may add that the hook has been made for me by Messrs. Weiss.



TEST-TYPES.

Messrs. T. Brettell and Co., of 51, Rupert-street, Haymarket, have printed for me a set of test-types for determining the acuteness of vision. They are drawn up to correspond as nearly as possible with those of Dr. Snellen of Utrecht, and, as supplemental to his well-known book of test-types, will tend to prevent the disproportionate wear and tear of its first two pages. The limbs are in diameter about one-fifth of the height of the letters, and the figures over each size express the number of feet at which the letters are seen by a standard eye. Words of one syllable are chosen, and verses are used, as facilitating the testing of the eyes of those who read imperfectly; and a line of small capitals is introduced, where practicable, for similar reasons, There is also a greater variety of the smaller types, commencing with Brilliant (1). Should these types be favorably received, it is proposed to print a piece of *prose* composed of small words to correspond. Copies of the above can be obtained of the printers.

Belgrave-road, December, 1871.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

GUY'S HOSPITAL.

(Cases under the care of Dr. Wilks.)

On the 4th January, we paid a visit to the clinical wards of this hospital, and accompanied Dr.

Wilks (who is at present in charge of the wards) while he made the round of his patients.

The Relative Advantages of Stimulants and Bloodletting; the tricuspid bruit.—Dr. Wilks discussed at some length the treatment indicated for a case of *emphysema and bronchitis* which had just been admitted. The patient was a thin old woman of about sixty years. She was intensely cyanotic; the small veins of the face were enlarged and distended, the jugulars standing out in bold relief. She was lying supported by pillows, with her head high, gasping for breath. There was next to no expansion of the chest. The feet were swollen, the abdomen was large and contained fluid, and the pulse was small and weak. She complained of feeling cold and exhausted, and begged for a little wine. The majority of practitioners, said Dr. Wilks, would at once accede to her request, and, because her pulse was feeble, would order her a considerable quantity of alcohol. This was not his idea of scientific treatment. The pulse was weak because the blood, being unable to pass the right side of the heart, entered the left ventricle in only small quantities. The blood was dammed back in the veins, and throughout the body there was a condition of extreme venous engorgement, which no amount of alcohol would relieve. The real scientific treatment for the case would be, he believed, to relieve the venous system by taking some blood. The jugular vein was by its swollen condition asking to be opened. Ultimately the patient was ordered to be bled to five or six ounces. There was no doubt, from the general condition of the patient, and especially from the state of her jugular veins, that there was considerable dilatation of the right side of the heart. There was undoubtedly regurgitation through the tricuspid orifice, and yet no murmur was audible. This was quite in accordance with Dr. Wilks's experience, that no amount of regurgitation through this orifice gave rise to a bruit unless there was disease of the valves as well. There was a considerable quantity of fluid in the peritoneal cavity, and there was little doubt that there was some cirrhosis of the liver. Very many of the patients had cirrhosis, and it was highly probable that the stimulants taken to relieve the breath were often answerable for this. It was most important to give alcohol with precisely the same care and exactness as other drugs. Dr. Wilks had a patient up stairs (a boy) to whom he was giving spirits of wine in drachm doses, and he was doing very well upon it. It was highly probable that the plan of universal bloodletting pursued by our forefathers was, on the whole, productive of less evil than the modern plan of universal stimulation.

We have since learnt, through the kindness of Mr. C. J. Oldham, that this patient, as well as another in a similar condition, experienced immediate and marked relief after six ounces of blood had been taken from the arm.

The Diagnosis of Thoracic Tumours; the assistance to be derived from the laryngoscope. Another case which presented itself for diagnosis was that of a man past the prime of life with a frequent *laryngeal cough.* There was considerable expectoration of watery fluid with some mucus. There was dulness in the right infra-clavicular region, with some doubtful pulsation in the second right interspace. The air did not enter the left lung so freely as the right. There was some difficulty of swallowing, but the mouth and pharynx were to all appearance healthy. Auscultation gave no decided information. The second sound of the heart was accentuated. The great probability here was that we had to deal with a thoracic tumour, possibly an aneurism; but the facts were hardly sufficient to warrant a certain diagnosis. We could learn something from the matter expectorated. Secretion from the bronchial tubes or larynx would cause cough; and, on the other hand, repeated cough would cause secretion. In the former case we should expect to find the expectoration purulent, and in the latter we should expect it to be watery, with a slight admixture of mucus, as was the case here. The character of the cough and the expectorated matter would not lead one to suppose that there was any organic change in the lungs or larynx. The laryngoscope would be of considerable assistance. If the larynx were found healthy, this would be in favor of thoracic tumour; and if one of the vocal cords were found paralysed, this probability would be greatly increased. The sphygmograph might also give assistance. We should possibly find the force of the pulse on the two sides different, or we might find it delayed upon one side. The accentuation of the second sound of the heart was in favor of aneurism.

The Administration of Alcohol.—Dr. Wilks, for the purpose of watching more closely the effects of alcohol on the system, has for many years prescribed a medicine which he calls "mistura alcoholica," composed of rectified spirit, tincture of cardamoms, and water, to be given at intervals during the day. But it is only in a particular class of cases that it is applicable. For the various ordinary acute and chronic disorders he has used wine and brandy when he has considered them necessary. It has been more especially in cases of atrophy in children and in tuberculosis that he has used this form with the most beneficial results. He believes that he was indebted many years ago to Dr. Sutton (now of the London Hospital) for this method of administering alcohol, and he has continued to use it ever since with very often marked success. When physician to the Royal Infirmary for Children, Dr. Wilks often prescribed small doses of rectified spirit, according to the above formula; but, unfortunately, there is no record of the results. He remembers one case, of a little boy who was said to have marasmus. He lay in bed almost a skeleton, and had taken all kinds of nourishment, with cod-liver oil, in vain. He was then ordered small doses of alcohol four times a day, and rapidly improved, so that in a few weeks he was fat and strong.

Dr. Wilks says he must leave it to the physiologists to determine the action of alcohol in the system; but from the fact of its not being eliminated, and from persons growing fat who partake of it, he has never discarded Liebig's views; certain it is that wasted children often directly improve under its use.

At the present time there is a little boy, aged five years and a half, in Guy's Hospital, under Dr. Wilks's care, who is on this plan. He was admitted in an extreme state of emaciation on Oct. 25th. No disease could be found in him, and he was handed over to the sister of the ward to take charge of, as it was thought his ailments might be due merely to starvation. In spite, however, of good living, with a little wine, he did not improve, and therefore, after being in for about six weeks, on Dec. 15th he was ordered one drachm of rectified spirit four times a day. In a few days he was better. He was soon able to leave his bed, and he has been growing fatter and stronger ever since.

We have been favored with the following interesting account of a post-mortem by Dr. Moxon:—

Polypoid Tumour of the Larynx, stopping the œsophagus; senile organs.—A man, aged sixty-five, a cabman, was in Guy's Hospital, suffering from difficulty in breathing and swallowing, with progressive emaciation. He had to be fed by the œsophagus tube. Laryngoscopic examination showed a growth, apparently occupying both pharynx and larynx. The dyspnœa became intense, and tracheotomy was performed. He lingered a month after this to reach the last stage of emaciation, and slowly sank.

The post-mortem examination, by Dr. Moxon, showed a growth, of the size of a chesnut, attached by a narrow root to the right aryteno-epiglottidean fold, and contriving so to place itself that it lay over the larynx during the effort to breathe, when the pharynx was at rest, while it stopped up the œsophagus during the attempt to swallow. In this way:—We need not say that the second stage of deglutition includes two movements for the protection of the larynx—first, the larynx itself is carried upward and forward, and, second, the root of the tongue is carried backward, the two motions combining to place the larynx under the root of the tongue, while the food passes back and down on its course. It will easily be seen that the movable mass attached loosely between the tongue and larynx had its root drawn up with the larynx, while its body was pushed back by the tongue so as to stop the passage of the food into the pharynx. The growth was an adenoid epithelioma. It was interesting to notice that, emaciated as the man was, his death had been determined by an attack of pneumonia. He had not died out simply. We usually find, both in very aged and very wasted persons, the full tale of morbid products, as though it still took as much as ever to kill them. The pneumonia was a purulent infiltration, with patches of gangrene—an intense pneumonia, the severity of its results being partly due to the extreme reduction of the patient; but another cause showed itself, for a piece of one ring of the trachea in the wound was ossified and dead, so that it separated at a touch. The ichor from this necrosis, no doubt, had conveyed down the trachea a like severe necrotic change. One was struck with the fact that his heart still weighed ten ounces and a half, whereas to correspond with such emaciation it should not have weighed more than five and a half or six ounces. The cause for this anomaly was found in the state of the kidneys. These were granular to a severe degree, being much wasted, so that the largeness of the heart was, in fact, a relative hypertrophy of the sort common in this renal disease. In the middle of the thoracic aorta (its hinder side) were several firmly-attached smooth masses of fibrine, shaped like shrunk-up garden slugs, quite decolorised, and evidently of old date—a point worth attention, as proving that the equivalent of cancer of the œsophagus is the proper cure for aneurism, seeing that it can produce firm fibrine in the open channel of the aorta. That the stomach was ready to use food, if it could get it, was proved by its having dissolved its own mucous coat, and containing highly acid mucus. The liver was like an old man's liver in one interesting particular, which has not been much noticed, we believe. This was that its anterior part in contact with the ribs was very hard, while its posterior part, which rests against the diaphragm and lung, was comparatively quite soft and natural. This hardness of the front edge of the liver in older people surely represents the effect of the exposure of this part of the organ to trifling mischances of various kinds—hard breathing, casual pressure, tight trowser-bands, &c. Often one sees on the left lobe, under and corresponding to the heart, a roundish patch, where the capsule is thickened and depressed. This patch registers in a similar way the extent of direct mechanical injury which the liver has sustained from the life-long beating of the heart. No gout was present, and the wasted muscles had the usual dark-brownish tinge.

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ST. MARY'S HOSPITAL.

ANEURISM AT THE ROOT OF THE NECK; LIGATURE OF RIGHT CAROTID AND SUBCLAVIAN ARTERIES; RECOVERY FROM THE OPERATION, BUT WITHOUT PERMANENT ARREST OF THE PROGRESS OF THE ANEURISM.

(Under the care of Mr. James Lane.)

The number of cases on record of the simultaneous ligature of the carotid and subclavian arteries is so small that we feel that we are justified in exceeding the usual limits allotted to a single case, in order to lay before our readers at length the following details with which Mr. Lane has kindly furnished us.

Jane W——, aged forty, married, was admitted under Dr. Broadbent on the 31st of July, 1871, with a pulsating swelling, about the size of a hen's egg, at the lower part of the neck on the right side. She was a slight, spare woman, otherwise apparently in good health. She said she had first noticed the swelling about three months previously, when it was not larger than a small walnut, and that it had gradually increased to its present size. For some time before its appearance she had suffered from severe cough, with which she was still troubled, though in a slighter degree. She had also had some dyspnœa and difficulty in deglutition; but these symptoms had almost entirely disappeared. She complained of pain in the back of the neck, in the right shoulder, and down the right arm. The tumour was situated over the lower part of the right common carotid artery; it was oblong in shape, with its lower end extending slightly downwards into the chest, behind the sterno-clavicular joint, but as yet there was no alteration in the appearance of the bones, and no indication of erosion having commenced. The pulsation in the tumour was very distinct and *expansive;* but there was only a slightly-marked bruit. No difference could be detected in the pulse in the radial and temporal arteries on the right and left sides. Pressure on the right carotid and subclavian arteries caused no perceptible alteration in the tumour. She was kept in bed, on beef-tea and milk diet, and was ordered iodide of potassium, in five-grain doses, thrice daily. This medicine was continued in gradually-increasing doses for six weeks; at first with some apparent benefit, but towards the end of that time a perceptible enlargement in the tumour had taken place, especially in its transverse dimension. It was nearly circular instead of oval in shape, reaching internally across the trachea to the inner edge of the left sterno-mastoid muscle, and externally beneath the right sterno-mastoid nearly to its outer border.

Under these circumstances she was transferred to Mr. James Lane, who determined, after consultation, to ligature the right carotid and subclavian arteries, which operations he performed on Sept. 20th. In some remarks previous to the operation he dwelt on the great difficulty of forming a correct judgment as to the exact point of origin of aneurisms at the root of the neck, and alluded to the recorded cases which had been subjected to operative treatment by ligature of one or both arteries on the distal side, especially those of Mr. C. Heath (who was present) and Mr. Maunder, in which the double ligature had been simultaneously applied. In both of these the aneurism was believed to be of the innominate artery, but in both the arch of the aorta was subsequently found to be involved. In the present case, he thought the fact that only a very small portion of the tumour was situated within the chest, together with the absence of bruit and of any abnormal dulness on percussion over the upper part of the sternum, rendered it probable that the aneurism was connected with the innominate artery, rather than with the aorta; but the point was one on which it was impossible to arrive at a positive conclusion.

The patient having been placed under chloroform, the operation on the carotid was undertaken first. The artery was exposed in the usual manner, and a silk ligature applied to it just above the omohyoid muscle. No unusual difficulty was met with, but the vessel was somewhat deeply seated in consequence of the projection of the tumour below, while it was not thought advisable to place the head and neck in a position of forced extension, which would have brought the artery nearer to the surface, as any attempt to do this seemed to cause a strain upon the aneurism, and had previously been found to cause great pain and distress to the patient. During the operation the artery was observed to have no connexion with the upper part of the aneurism, but to pass down deeply behind it. Pulsation in the temporal artery ceased when the ligature was tied. The subclavian artery was then exposed, external to the scalenus muscle, by the usual incisions. A vein crossing the neck from the acromial side to enter the external jugular was divided, and required a ligature; but the jugular itself was easily pushed inwards out of the way, and gave no trouble. Some difficulty was experienced in bringing the artery into view and in passing the ligature beneath it, from its being overlapped and concealed more than usual by the lower nerves of the brachial plexus. The pulsation in the radial artery was completely arrested by the ligature. The wounds were closed by silver-wire sutures, and covered with lint dipped in carbolic-acid lotion.

On the following day some febrile excitement was observed; the pulse was 120, the face was flushed, and the patient was restless and excited. These symptoms, however, soon subsided, and nothing worthy of record of an unfavorable nature was further noticed as a result of the operation. No alteration in the pupil, or other symptom indicating disturbance in the cerebral circulation, was at any time observed. Pulsation in the right temporal artery could be felt on the day after the operation, though less distinctly than on the opposite side. In a few days, however, there was hardly any perceptible difference in this respect between the two sides. After a time, also, pulsation could be distinguished in the carotids above the ligature, showing that there was a free retrograde circulation through the circle of Willis down to the bifurcation, and so round into the branches of the external carotid.

There was complete absence of pulsation in the brachial and radial arteries for some weeks, but at length a feeble thrill could be distinguished in them. She suffered a good deal from numbness and tingling in the limb, and sometimes from severe neuralgic pains. It was kept perfectly quiet and wrapped in cotton wool. The ligatures separated from the carotid on the fourteenth, and from the subclavian on the nineteenth day. There was some slight suppuration in both wounds, but they both healed, on the whole, favorably.

Effect of the operation on the aneurism.—No alteration was observed in the tumour immediately after the application of the ligatures, nor could any difference be detected on the following day; but in two or three days it was perceptibly less prominent, the pulsation was less violent, and it was thought the walls of the aneurism were more solid. This improvement continued steadily, and at the time of the separation of the second ligature the aneurism had diminished by fully one-third in transverse measurement, and projected only slightly from the surface, but in longitudinal extent no difference could be noticed. At the end of a month the pulsation was recorded as *very feeble*, a marked difference having been observed in this respect within a few days, and solidification in the walls of the aneurism appeared to be taking place. She had all along (after the first two or three days) expressed herself as much relieved, especially by the diminished feeling of pulsation.

She was allowed to get up and move about the ward, but did not leave the hospital till Nov. 6th, forty-seven days from the date of the operation. Before this, however, it had been observed that any slight exertion increased the pulsation and caused enlargement of the tumour. Before leaving her bed in the morning it was perceptibly smaller than after she had been up a few hours. It appeared questionable, therefore, whether any real advantage had been obtained, or whether the apparent improvement had not been rather the result of the absolute rest in which she was kept after the operation than of the operation itself. Since she quitted the hospital she has been occasionally visited. The aneurism has been steadily increasing in size; it is now twice as large as when she left, and extends upwards as high as the bifurcation of the carotid. The pulsation in it is very strong, and its walls appear to be thinning. The upper end of the sternum and adjacent portion of the clavicle are becoming eroded. She has been strongly urged to return to the hospital, when it was intended to practice electro-puncture on the plan suggested by Dr. Althaus, of placing the negative pole of the battery within the sac and the positive pole outside; but she has hitherto obstinately refused to submit to any further treatment.

UNIVERSITY COLLEGE HOSPITAL.

A CASE OF PHTHISIS FOLLOWING PNEUMONIA OF THE RIGHT LUNG, AND BRONCHITIS OF THE LEFT.

(Under the care of Dr. WILSON FOX.)

IN the "Mirror" of last month, we briefly recorded some clinical observations by Dr. Wilson Fox on the diagnosis and treatment of a patient in whom phthisis appeared to have supervened on pneumonia of the right lung, and on bronchitis of the left. There seemed to be reason to hope that, under careful treatment, his condition might be brought to undergo some improvement. As, however, death ensued three days after our visit, we are enabled now to add an account of the condition which was revealed at the post-mortem examination.

Death occurred, apparently from suffocation, ten minutes after a fit of coughing, in the course of which a large quantity of blood flowed from the mouth and nostrils.

The left lung completely overlapped the heart, which encroached to the right of the sternum; it extended upwards, on the right side, as far as the second cartilage, and was more or less emphysematous throughout, especially at the anterior margin and base. No distinction could be accurately traced between the lobes of the right lung. The bronchi, especially those of the right lung, were filled with frothy fluid; blood-stained mucus was found in those of the left lung, but no clots were found in any of the larger bronchi.

The upper lobe of the right lung was occupied by an extensive cavity, of which the parietes were changed into pigmented fibrous tissue, and completely consolidated. The middle lobe was the only portion of the right lung which contained any remaining lung tissue; it was emphysematous at the upper part, studded with tubercle, and with indurated and caseous granulations, some of which were arranged in racemose groups, interspersed with fibrous bands, of which some appeared to be pleural and ran indefinitely into the lung. The lower lobe was occupied by a very large cavity, in which were numerous bronchi considerably dilated; it was traversed by fibrous bands, some of which contained pervious vessels. The active source of the hæmorrhage could not be discovered. The bronchi of the two lower lobes of this lung presented numerous fusiform dilatations which passed into ulcerated cavities.

In the left lung, the bronchi presented similar fusiform dilatations; those of the upper lobe ended abruptly in a few ulcerated cavities. The posterior part of the upper lobe was occupied by a cavity lined with caseous matter, and the emphysematous tissue contained pigmented, indurating, semi-cartilaginous tubercle. In the lower part of the upper lobe numerous masses of fibrous thickening surrounded the excavations, and ran irregularly through the lobe, intercommunicating. There was no disease of the lining membrane of the heart; but the right ventricle was hypertrophied. The mucous membrane of the stomach was covered with thick blood-stained mucus; it was thickened, indurated, and finely injected; no ulceration or erosion could be discovered in the intestines.

ENCEPHALOID CANCER OF THE TESTICLE; CASTRATION; UNOBLITERATED PROCESSUS VAGINALIS TESTIS; PERITONITIS; DEATH.

(Under the care of Mr. B. HILL.)

The following case, to which we have been unable to find a parallel, illustrates a danger which may attend the operation of castration—a danger which has heretofore been ascribed only to operations upon testicles retained in the inguinal canal.

H. C——, aged two, was admitted into University College Hospital on Nov. 14th, 1871, with a tumour of the right testicle. His friends stated that he had had a swelling in this region ever since he was three weeks old. He wore a truss for the swelling till he was three months old, when it is said to have disappeared until seven months before his admission to the hospital, when it returned again in the same place. It was supposed by the surgeon who attended him to be an undescended testis. Subsequently the diagnosis of hydrocele was made, and it was accordingly tapped. After this the swelling increased in size.

On admission, the swelling was found to be slightly translucent, and the testicle could be felt behind it. The swelling was globular and elastic; the cord could be felt above it, apparently healthy, and the skin, of a normal color, was perfectly freely movable over the tumour. A trocar was thrust into the tumour, but only two or three drops of blood escaped.

On Nov. 23rd Mr. Hill (having determined to explore the diseased part, and, if necessary, to remove it) performed the operation of castration. The operation offers no points for particular comment. The spermatic cord, which was ligatured *en masse*, was perfectly healthy at the part where it was ligatured. On examination after removal, the tumour was found to present all the naked-eye and microscopic characters of encephaloid cancer. The chief part of the tumour was formed by the body of the testis, the epidydymis being only recognisable as an irregular lump on the back of the tumour. On the day following the operation the patient's temperature was found to be very high (104·3° F.), and on the 25th it was 105·1° F. On the 26th a rash like scarlet fever appeared, and he was separated from his fellows. This lasted till the 28th, when the stools were noted as "offensive"; there was a "pyæmic odor of the breath," and the parts about the wound were much swollen.

On the 30th copious diarrhœa set in, the swelling round the wound increased, and the abdomen was swollen. On the next day frequent vomiting set in, and continued till death, which occurred on December 2nd, nine days after the operation.

Autopsy thirteen hours after death.—The wound was covered with a thick layer of foul yellow slough. The abdomen was rather fully distended and tympanitic in front; it was dull in the flanks. On opening the belly about half a pint of dark straw-colored fluid mixed with flakes of lymph escaped; the intestines were but slightly congested; the congestion was most marked where the folds met; they were not glued together by lymph; they were fully distended by flatus. The pelvis and the posterior part of the abdominal cavity contained large quantities of soft yellow lymph. On sponging out the cavity of the abdomen an opening was seen in the situation of the end of the vaginal process of peritoneum. On putting a probe into this it was found to pass directly into the wound. The canal was then slit up and found to be the vaginal process unobliterated. It had been included in the ligature, but the ligature had cut it through. Its end was perfectly unclosed by lymph and was completely open into the wound, thus opening the cavity of the peritoneum to the air, and into the midst of the sloughy tissue in the wound. The tissues forming the process were thin, like healthy peritoneum. The tube was of uniform size, and presented no appearance of a neck as it would if it were an old hernial sac. At the opening into the abdomen there was a little very soft puriform lymph, but there had been no attempt to close the opening either by firm lymph or by adhesion of a coil of intestine. There was no other feature of interest in the post-mortem examination, excepting that one of the lumbar glands was found infiltrated with cancer which was breaking down.

The processus vaginalis of the opposite side was completely closed.

For the notes of this interesting case we are indebted to Mr. H. C. Lang, ward-clerk. The post-mortem was made by Mr. Marcus Beck.

In remarking on the above case, Mr. Hill said:—"This appears to be a unique case of cancer developing in a testis the serous covering of which still freely communicated with the cavity of the peritoneum—an arrest of development that obtains perhaps more often than is generally supposed. In this instance its presence was the cause of the fatal peritonitis, by permitting the inclusion of a prolongation of the peritoneum in the ligature applied to the cord—a procedure that was, nevertheless, not necessarily fatal, as it was exactly analogous to the old operation for radical cure of hernia by ligaturing the neck of the sac—one sometimes successful, though of course attended by great danger. The post-mortem investigation showed the importance of speedily removing malignant tumours of the testis. During life no change in the iliac or lumbar lymphatics could be detected, yet one gland had already undergone cancerous infiltration. Though the result of the operation was unfortunate from the accidental co-existence of a serous covering to the cord common to the testis and the abdominal cavity, which could not be diagnosed during life, the child's life could not have been greatly prolonged by withholding the operation, and his sufferings would have been more severe than they actually proved. The post-mortem examination helps also to clear up the obscurity concerning the nature of the tumour, which was detected soon after birth, and which disappeared after a truss was worn. Instead of a hernia, it was probably the incompletely descended testis, which, as often happens, only reached its proper position in the first year after birth."

TESTIS IN THE PERINEUM.

An example of this rare deformity occurred lately among Mr. Berkeley Hill's out-patients, and may be added to the foregoing case, where arrest of development had an important influence on the result of the operation. The patient was seven months old, born at full term, and in other respects well formed. His father, and, so far as was known, his male relations are naturally developed. The child's condition was observed by his mother at three weeks after birth, and she believes no change in the position of the testis has taken place since then. The scrotum is well formed, and almost as large on the left as on the right side of the raphe, an unusual circumstance in these cases. Examination with the finger reveals the presence of only one testicle, the right, in the purse. On the left side, the cord passed down from the external abdominal ring to the testis, which formed a slight projection under the skin, a little in front of and close to the ischial tuberosity. The testis, apparently as large as that in the scrotum, was very movable in its unusual position, and could be pushed upwards about one inch towards the ring, but was restrained from going further by a tight band attached below, possibly the gubernaculum still remaining. The freedom of the gland in its abnormal position, which would allow it to slip aside when exposed to pressure, and the firmness of this restraining band, were decisive against any attempt to put the testis into the scrotum, the more so as among the few recorded instances of this peculiarity attempts to do this have failed. Again, this position of the gland does not prevent the growth of the gland, for Ledwich, who dissected an example, found spermatozoids in the displaced testis; in another case, Godard's, the patient had been a soldier, was able to ride and march without fatigue, and was at the time of the observation a father and a grandfather. This position of the testis is natural in certain pachydermata—the swine, for example, where they are always lodged in the perineum.

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MIDDLESEX HOSPITAL.

CASES UNDER THE CARE OF MR. HULKE.

Stricture of Urethra.—Two interesting cases of this disease were lying side by side in adjoining beds. The first was a middle-aged man, with a dense cartilaginous "bridle" stricture, affecting only a very short length of the urethra. With great difficulty a No. 1 catheter had been passed, and the stricture gradually dilated till a No. 3 was got through; but beyond this no farther dilatation was found practicable. Recourse was therefore had to Holt's dilator, after the use of which a full-sized catheter was passed with ease. In the second case (an elderly man) a considerable length of the urethra was the seat of dense horny thickening. At first only a smallest-sized catheter could be passed; but the case proved readily amenable to the treatment by gradual dilatation, and now a No. 8 was passed with little difficulty. It is always Mr. Hulke's custom to attempt the cure of stricture, in the first instance, by gradual dilatation; and not until this has failed does he resort to more sudden and violent methods of dilatation. Those cases in which a short section of the urethra only is affected are the most suitable, in his opinion, for treatment by Holt's method. He has never met with any untoward accident while using Mr. Holt's instrument. He thinks it is of great importance that the left hand should be held perfectly steady during the operation, lest, in forcibly driving the stillette onwards, the left hand be jogged onward likewise, and thus the bladder be wounded by the point of the instrument.

We next saw a case of *Fractured Patella*, in a young man, where excellent results had been obtained with very close approximation of the fragments. The means employed had been merely straps, bandages, and position. Mr. Hulke rarely uses Malgaigne's hooks, because he very rarely meets with a case suitable for their employment. In his opinion they are of not much use unless they are applied immediately after the occurrence of the accident, and before appreciable swelling or extravasation has occurred. When they are thus used, and are carefully applied, complete apposition of the fragments is obtained, extravasation is to a great degree checked, and they are of the utmost service. The application of the hooks is a painful process, and it is generally advisable to administer chloroform. Whenever the hooks are employed the case should be watched most carefully from day to day, and at the first sign of suppuration they should be removed. Mr. Hulke told us of a case in which the careless use of Malgaigne's hooks had nearly cost the life of a patient by causing necrosis of the patella and suppurative inflammation of the knee-joint. He had not seen any decided benefit from the use of Malgaigne's hooks in the later stages of fractured patella.

Extensive Burn.—A middle-aged man, in a fit of drunkenness, had fallen across the fire. He was admitted on Dec. 9th. The whole of the left half of the thorax, the left arm and shoulder, and the left half of the face were burnt in some places to a considerable depth. The extent amounted fully to a fifth part of the surface of the body. The dressing first used was carron oil; subsequently chalk ointment had been employed. Opium had been given internally to ease pain and encourage sleep. We saw him on the twelfth day after the accident. He had been placed in a private ward in order that the fetor of the burnt surface might not annoy the other patients. His bed was placed in the centre of the ward, quite away from the walls, in order that the air might circulate freely around him. The wounds had commenced to granulate. He was delirious, especially at night. Respiration quick; sputa distinctly rusty (he had had some bronchitis almost from the first); pulse 100, of good volume; skin moist; tongue slightly furred and moist; was taking nourishment fairly well. For the last two days had been troubled with slight diarrhœa; no blood had been passed with the motions. Notwithstanding the extent of the burnt surface, and the unmistakable evidence of pneumonia, the temperature, when we saw him, was only 99°F. The highest temperature which had been reached at any time was 101·2° F. The great depression caused by the extensive burn might (in the opinion of Mr. Hulke) possibly counteract the tendency towards elevation of temperature. In consequence of the delirium Mr. Hulke ordered that the opium which he had been taking should be replaced by the hydrate of chloral.

In the *Cancer* wards we were shown two or three patients who were taking decoction of condurango. The uselessness of this remedy has already been commented upon in this journal. The decoction is of a brown color, and, in appearance, taste, and smell, closely resembles decoction of senega. At first it seems to improve the appetite, and to act like other bitter tonics; but after a few days the patients are generally troubled with flatus and abdominal cramps. It is now never given unless combined with ginger to prevent the attendant griping.

Erysipelas of Scalp.—J. T——, a coachman, aged thirty, sustained a slight scalp wound at the back of the head, for which he was treated as an out-patient at the Middlesex Hospital. About a week after the injury he was seized with shivering and general malaise, and three days later was admitted as an in-patient of the hospital, complaining of intense pain in the parietal regions of the head. At this time there was neither puffiness nor bogginess of the scalp, but there was a red spot on the side of the right temple. Pulse 120; temperature 105° F. The grave constitutional symptoms, together with the intense headache, favored a diagnosis of suppuration of the veins of the diploe and pyæmia. Mercurial purgatives were administered, poultices containing sulphurous acid were applied to the head, and the patient was placed on a light moderately stimulating diet. Two days after admission the scalp became tumefied, and more or less red, especially about the ears. There was great tenderness, and the headache had increased, but there was a decline of temperature, which now was 102·3° F. There was no paralysis, and he was ordered iron and quinine, and a liberal amount of brandy. Poultices and lead lotion were applied to the head. He was very delirious, so much so that on one day the temperature could not be taken. Two days later (five from his admission) the erysipelas had increased, and included the whole scalp and great part of the face. There was no evidence of pus under the scalp. He was still delirious, his urine (which contained a fifth part of albumen) was passed involuntarily, and there was slight symptoms (noisy breathing) of œdema glottidis. The pulse was 128, but the temperature had declined to 100·8° F. From this time he rapidly improved, and on the next day his erysipelas almost completely subsided, the tenderness disappeared, and he became perfectly conscious. On the day following (six days after his admission) the temperature was normal. He remained in the hospital for a fortnight longer, the delay being partly caused by two small abscesses—the one beneath the right orbit, the other on the left upper eyelid,—which had resulted from the erysipelas.

GENERAL HOSPITAL, VIENNA.

CYSTIC TUMOUR OF UNUSUAL CHARACTER IN THE WALL OF THE UTERUS.

(Under the care of Professor Löbl.)

About the second week in September last, a woman forty years of age consulted Dr. F——, private lecturer on diseases of women in the Vienna University, for uterine hæmorrhage. She had for two years previously noticed that there was a tumour at the lower part of her abdomen apparently rising out of the pelvis, and she sometimes felt pain in the lower part of her right side.

On examination, Dr. F—— was able to feel through the abdominal wall, rather towards the left side, an uneven tumour, rather larger than a man's head, painless on palpation, and movable. Vaginal examination only indicated the presence of a large, globular, elastic swelling, projecting into the upper part of the vagina on the left side. It seemed to fluctuate, was smooth over the greater part of its surface, but had at its lowest part small folds and hollows, which might have been taken for the os uteri. The sound, however, passed backwards and to the right, so that its end was clearly felt through the abdominal wall. Its introduction gave no pain, but there was blood on the instrument when withdrawn. The finger was also bloody after examining. The sound showed that the uterus was much increased in size and lengthened, and that the tumour was closely united to and movable with the former.

Four or five days after the first examination the patient had great pain in the abdomen, vomiting and fever, and all the signs of peritonitis, from which, however, she entirely recovered. She was at this time admitted into Prof. Löbl's wards, and remained there under observation till she died.

She was sounded on October 18th by Dr. F——, and the sounding gave the same results exactly as at the first examination. While in the hospital the patient was seized with symptoms of pneumonia; had bloody sputa and great dyspnœa. On October 19th she got up to sit on the night-stool, and died suddenly before any assistance could be rendered.

The post-mortem took place in the Pathological Institute on October 21st. The corpse presented externally nothing notewothy. On opening the abdomen a tumour, not unlike an ovarian cyst, projected in the position of the distended bladder from the pelvis, nearly in the middle line. It was found externally to occupy the front and left walls of the uterus, and to push the right wall over towards the right side. There were signs of an old peritonitis, with adhesions and pigmentation behind the uterus, attaching it to the rectum, but no sign of any perforation, or of a recent inflammation. There was some fluctuation, or at any rate great softness, to be made out towards the right side of the tumour. On removing the genital organs *en masse*, and slitting up the vagina and uterus, the following points were observed. The introitus vaginæ was very narrow, but the vagina itself soon became very wide and filled with a tumour of smooth white surface, on some parts of which were scars of a shining appearance, as if of a tendinous nature. The tumour extended up through the cervix (which was enormously dilated, and whose lip could only be recognised as a narrow rim or ridge) into the uterus itself. The mucous membrane of both vagina and uterus was hyperæmic and injected, and the right wall of the later was thickened. The left side and front of the uterus was occupied internally by the tumour, whose size was that made out during life, and whose shape can be best represented by imagining two slightly oval balls, one half as large as the other, each cut flat at a fifth of its diameter from one end, and the flat surfaces cemented together, the larger ball being uppermost. A section through the mass showed that it was made up of a number of compartments not communicating with each other, whose thin walls were formed of connective tissue, carrying bloodvessels which passed across the tumour. The contents of the cysts was a clear yellow, apparently serous fluid. Finally, it must be added that the tumour was seated completely in the substance of the uterus, the fundus being its upper boundary, while remains of muscular fibres could be detected in its outer wall, close under the peritoneum. No reason for the patient's sudden death could be found. There was some œdema of the lungs, but not enough to cause death, and the kidneys and every other organ in the body were found healthy on most careful examination.

It remains only to consider the possible nature of this growth. Rokitansky has never before seen one like it, and it is believed that an account of a similar one does not exist in pathological literature.

There are two views which can be taken. First, that it is due to enormous hypertrophy of the ovula Nabothi, or obstructed and distended follicles of the portio vaginalis, which are known to attain considerable size in chronic uterine catarrh and hypertrophy of the vaginal portion of the cervix. The microscopic examination is however opposed to this view, as cylindrical epithelium could nowhere be found on the walls of the cysts. Secondly, that a myxoma or fibro-myxoma in the uterine wall had undergone a mucous degeneration, the true structure having disappeared, leaving only the septa formed of connective tissue. This is probably the correct view, as the fluid in the cysts contained much mucin, and gave a copious precipitate with acetic acid.

A detailed account of the whole case will probably be published before long.

Medical Annotations.

"Ne quid nimis."

PHOSPHORESCENCE OF MARINE ANIMALS.

Professor Panceri, we learn from *Nature*, has been studying for some time past the phosphorescence of marine animals. He has examined Noctiluca, Beroe, Pyrosoma, Pholas, Chætopterus, and has lately published a paper "On the Phosphorescence of Pennatula." He finds in all cases that the phosphorescence is due to matter cast off by the animal; it is a property of dead separated, not of living tissues. In all cases except Noctiluca he also finds that this matter is secreted by *glands*, possibly special for this purpose, but more probably the phosphorescence is a secondary property of the secretion. Further, the secretion contains epithelial cells in a state of fatty degeneration, and it is these fatty cells, and the fat to which they give rise, that are phosphorescent. Hence the phosphorescence of marine animals is brought under the same category as the phosphorescence of decaying fish and bones. It is due to the formation in decomposition of a phosphoric hydro-carbon, or possibly of phosphuretted hydrogen itself. In Pennatula Professor Panceri has made phosphorescence the means of studying a more important physiological question—namely, the rate of transmission of an irritation. For when one extremity of a Pennatula is irritated a stream of phosphorescent light runs along the whole length of the polype colony, indicating thus by its passage the rate of transmission of the irritation. This admits of accurate measurement, and furnishes data for extending Helmholtz's and Donders' inquiries to animals so widely separated from their *Versuchsthiere* as the Cœlenterata. It is also a proof of the thoroughness of Professor Panceri's investigation that he has made use of the spectroscope for studying the light of phosphorescence.

THE ACTION OF QUININE ON THE WHITE CORPUSCLES OF THE BLOOD.

Some time ago Binz stated that quinine possessed the property of arresting the movements of the white corpuscles of the blood. This was, however, considered by Stricker not to be justified by the facts, since the quinine was applied in the form of an acid solution, the acid of which, whether the quinine were present or not, would be certain to exert a powerful action on the white corpuscles. The subject has recently again been taken up by Kerner, who contributes a paper on this subject to the last part of Pflüger's *Archiv*, being incited by the observations of Mosler on the cure of certain cases of leucæmia by the administration of quinine. Kerner remarks that it is quite possible to obtain a neutral salt of quinine, and in his experiments he used the chloride and the carbonate. He drew small quantities of blood from cats and dogs, and applied a one-tenth solution of this salt in the proportion to the blood of 1 part to 4000, upon a microscopic stage maintained at blood heat. The result was striking. The white corpuscles became round and darkly granular, and the movements were very speedily completely arrested. It of course became interesting to compare these effects with those produced by other neutral salts, and in pursuing this investigation to some extent he found that salicin, caffein, atropine, and arseniate of potash were all either wholly indifferent or possessed only the slightest influence. Quinine, therefore, exerts a remarkable action on the white corpuscles of the blood, independent of its antiseptic properties.

ABSORPTION BY THE VESICO-URETHRAL MUCOUS MEMBRANE.

M. E. Alling has recently investigated anew this much contested subject, and has arrived at the following conclusions:—1. The healthy bladder does not absorb to an appreciable extent either remedial or toxic agents. 2. The healthy urethra, on the contrary, is perfectly capable of absorbing them. 3. The mucous membrane of an inflamed bladder is unquestionably capable of absorption. 4. Lastly, in a therapeutic point of view, this capability of absorbing possessed by the inflamed bladder may be advantageously employed as a means of cure.

DISTANCE OF THE MACULA LUTEA FROM THE OPTIC DISC.

In a recent number of the *Centralblatt*, Dr. Ed. Londolt, of Zurich, states that, from careful measurements he has made in upwards of one hundred eyes, he has ascertained that in healthy or emmetropic eyes the distance of the macula lutea from the centre of the optic papilla, in a horizontal direction, is 3·915 millimetres; the former being also 0·785 of a millimeter higher vertically. The distance is always greater than that represented by these figures in hypermetropic and less in myopic eyes.

Transfusion of Blood.—At a late meeting of the Obstetrical Society of Dublin, two cases of transfusion of blood were detailed by Dr. Ringland, one of which proved successful. In both cases the instrument used was that invented by Dr. McDonnell, of Dublin, in which an arrangement exists to obviate the danger of air entering the circulation during the operation.

Correspondence.

"Audi alteram partem."

SUBCUTANEOUS INJECTION OF MORPHIA IN CHOLERA.

To the Editor of The Lancet.

Sir,—I wish to bring, briefly at present, before the notice of the profession the short experience I have had of the effects of the subcutaneous injection of morphia in the treatment of Asiatic cholera.

Soon after the outbreak of the disease in this suburb on the Golden Horn, where all the English mechanics working in the Imperial Arsenal reside, Dr. Patterson, the surgeon to the British Seamen's Hospital in this city, and I, having repeatedly failed in giving permanent relief to the most urgent symptoms of the disease by almost all the agents usually employed for such a purpose, decided upon giving a trial to the subcutaneous injection of morphia, merely hoping, at that time, to quiet thereby the great excitement of the nervous system in the presence of a mortal disease, and to allay the irritability of the stomach to the extent of allowing other remedies to be retained. To our great delight, and to the unspeakable comfort of our patients, we found in the first two or three cases that there was no further occasion for drugs of any sort; for in fifteen or twenty minutes after the injection the patients fell into a calm sleep, from which they awoke in two or three hours bathed in a warm perspiration, and expressing themselves as feeling "quite well again." After such success we were, of course, encouraged to persist, and I have now before me the records of twenty-two cases in which I used it with the most beneficial results. I have not time to enter into the details of each case; but I may say generally that there is hardly a condition known in the active stages of the disease in which I did not find the method of great use—violent vomiting; severe and persistent cramps in all the limbs and in the chest, so strong as to jerk a man out of bed; dyspnœa almost threatening suffocation; reduction of temperature to such an extent that every part of the body, tongue included, felt icy cold to the touch; diarrhœa so urgent as to be evacuated in bed before help could be given by the attendants; paralysis of the sphincter ani, so that stools were passed unconsciously; the facies choleritica in all its ghastliness; with the circulation so weakened that the pulse was imperceptible at the wrist. Any of these conditions I have seen completely and permanently removed by one injection of morphia. In several cases I had, upon the recurrence of the symptoms in two or three hours, to repeat the injection, and in one or two instances I had to do it a third time—perfectly successful in all cases in mitigating the sufferings, and thus diminishing the consequent exhaustion. The dose of morphia injected varied from the one-twelfth to the one-half of a grain, according to the age and the circumstances of the patient.

I am, Sir, yours, &c.,
Augustus Werry, M.D. Edin.

Hasskeni, Constantinople, Nov. 1st, 1871.

INCONTINENCE OF URINE IN CHILDREN.

To the Editor of THE LANCET.

SIR,—May I ask that you will kindly insert the enclosed communication respecting the treatment of the involuntary flow of urine, especially during night, of young children. I have tried, as others, the usual remedies, and without effect; but I have seen by experience that the administration of creasote is attended by the happiest results. The mode in which I administer it is in the form of a pill—creasote, one minim; bread crumb, q. s.;—or creasote, one minim; assafœtida pill, compound rhubarb pill, of each two grains. This is to be taken three times a day. I have had lately about five cases, in which complete success has followed this treatment. Your obedient servant,

HOLMES COOTE.

Margaret-street, Cavendish-square, Jan. 25th, 1872.

MR. BIRKETT'S LECTURES ON NEW GROWTHS.

Delivered at the Royal College of Surgeons in May and June, 1871.

MR. BIRKETT's first lecture was devoted to the general characters of cysts and cystic growths. Effusions of fluid in the body are either diffused, percolating through the connective tissue, or circumscribed, constituting a cyst. The membranous sac which encloses the fluid consists essentially of three parts: an external fibrous coat derived from the connective tissue of the part in which it happens to be developed, a tunica propria, and an internal epithelial lining. In all cysts, at some period or other, these parts may be traced.

There are five chief methods in which cysts are developed. First, by irritative action. A blister, which contains all the elements of a cystic growth, presents a familiar example of this class. The cyst which becomes formed around a foreign body, as a bullet, or an effusion of blood, especially upon the scalp, is developed in the same way. Simple pressure may thus produce a cyst, as in the case of the small bursa which is sometimes met with beneath a corn. Secondly, cysts are formed out of pre-existing structures, such as sacculi and canals of various kinds. The cyst sometimes formed from the bursa in front of the patella, ranula, and the cystic growths which occur in the labia majora of the female, formed by the dilatation of the duct of a secreting gland by obliteration of its aperture, are examples of this class. Thirdly, cysts are sometimes suddenly developed in a hard tumour by a process of softening down. We are surprised to find a tumour which was once firm, solid, and resistant to the touch, now consists of little else than a small growth and a large collection of fluid. In carcinomatous growths this sometimes occurs by a mechanical process. A sort of ulceration may go on within the fibrous capsule on the side of the tumour farthest from the skin; the escape of the resulting secretion is prevented, and the distension of the fibrous envelope results in a cyst. The irritation of the pent-up fluid still further augments the process. The occurrence of this change may render the diagnosis of the nature of such a growth very difficult. A fourth method is that which was first advocated by Rokitansky, although the idea had been previously suggested by Dr. Hodgkin—that of the development of a cyst by the enormous enlargement of a single nucleated cell. Mr. Birkett has never been able to trace any process of this character, and the idea now finds little favor even in Germany. Fifthly, cysts may arise from the acini of gland-tissue. This follicular development is well known in the ovary and in the thyroid gland. The change in the follicles is not difficult to understand. It might arise from effusion of fluid either into the cæcal termination of the acinus or into the developing nuclear tissue which surrounds it.

The contents of cysts are of three classes: first, blood; secondly, serum; and thirdly, the various contents which depend on the organ or structure in which the cyst is developed. Two kinds of serum are met with; one coagulable, like the ordinary fluid of a hydrocele, and another which will not coagulate by any means which will coagulate the ordinary form. The latter occurs occasionally in cysts in the mammary gland, and are of great interest, though they have not received much attention. Sir Benjamin Brodie relates two cases. The wall of such a cyst, which is lined by epithelium, is so delicate that after the sac is emptied scarcely any trace of it can be found. The triple division of cysts according to their contents, which was proposed by Mr. Simon, and has been adopted by Virchow, answers very well for practical purposes; it is into extravasation cysts, exudation cysts, and retention cysts. A fourth class might be formed of those in which the serum will not coagulate—transudation cysts. The delicate wall may act as a sort of filter, and keep back the albumen. A class of cysts which, however, it is not easy to arrange in any one of these divisions consists of those which arise from an increase of volume of the spaces of the alveolar and connective tissue. The surrounding tissue is sometimes converted into a very firm envelope.

The chief changes which may take place in the walls of cysts are the following: thickening by new growth (often seen in bursæ); inflammation, sometimes leading to adhesions which render very difficult any subsequent attempt at excision; new growth around a cyst, as in a remarkable case in which scirrhus became developed about a mammary cyst which at first was quite simple; growths from the interior; and, lastly, calcification and ossification. According to German authors, true bony tissue may be developed.

In the second lecture Mr. Birkett continued the subject of cystic growths. He described, first, the extravasation cysts. Familiar examples are those which result from effusions of blood, either into pre-existing spaces, as the tunica vaginalis, or into tissues, as the brain or the connective tissue. A more interesting class consists of those milk cysts which occasionally arise in the mammary gland during the period of lactation in consequence of the rupture of a milk duct. They may occur in a healthy breast or in association with a new growth. They usually arise suddenly, and may persist for a considerable time, sometimes ultimately presenting much difficulty in diagnosis. The relation of a new growth in the mammary gland to its active or inactive condition is a point of great importance for consideration in determining its nature.

Several cases of this disease were related. In one, a woman thirty-five years of age, the tumour had existed for thirteen years, and measured four inches in diameter, but had varied in size at different periods. On incision ten ounces of cream escaped, the serous part of the milk having been absorbed. As in the other cases, the wound healed completely by granulation. In another instance a number of ducts opened into the cyst from what had appeared to be a large adenoid tumour, but which proved on examination to be a newly-formed mass of perfectly healthy gland-tissue. It is not, indeed, uncommon in ordinary mammary grandular tumours to find small cysts containing milk. When a milk cyst contains fluid, simple incision suffices for its cure; if solid cream, excision is necessary.

Retention cysts are those in which the duct of a gland has become occluded, and its wall stretched over the retained secretion. Sebaceous or artheromatous cysts, associated with hair-follicles, are familiar examples. Of these there are three kinds: one filled with epithelial scales, another containing sebaceous and greasy matter, and others hard and fibrous, with very little cyst wall. The first is probably due to obstruction at the neck of the hair-follicle, the second to obstruction in the follicle near the opening of the sebaceous gland, and the third to an obstruction lower down in the hair-sac, so that the cells which should constitute the hard fibrous part of the hair become transformed into a dense laminated mass. Atheromatous cysts are not always simple: one which was excised by Mr. Birkett from the thigh had irregular processes extending for a considerable distance, which had occasioned much difficulty in diagnosis. Virchow regards this irregularity as due to the involvement of several follicles, but in Mr. Birkett's opinion it is a mechanical result of the pressure upon the thinner portions of the sac. When these cysts become irritated and inflamed, granulations may sprout up from their base, and simulate closely fungoid disease. One or two cases were related in which great difficulty in diagnosis had arisen from this cause.

In his third lecture, Mr. Birkett continued his description of retention cysts, speaking first of those associated with mucous membranes. Obstruction at the orifice of the racemose muciparous glands may cause the dilatation of the secreting part into one single sacculus. Such cysts are especially common in connexion with the labial glands, also on the inside of the nose, the epiglottis, and the pharynx. They contain a very tenacious fluid. Sublingual cysts constitute an important class. Many of them are doubtless mucous cysts; but there are several ducts in this situation, and it is generally supposed that these cysts are connected with them. In one case in which a large cyst existed below the tongue, Mr. Birkett was able to pass a probe along the submaxillary duct, proving beyond doubt that there was no obstruction in it. What is commonly called ranula is, in most cases, only a mucous cyst. The submaxillary duct may become obstructed, but it is usually in consequence of a concretion there, and in circumstances quite different from those in which sublingual cysts are ordinarily developed. In one case of obstruction from concretion there was no swelling in the mouth. Sudden obstruction is attended with great pain; ranula is usually painless. Moreover, in the contents of these cysts Mr. Birkett has never been able to detect a trace of the chemical constituents of saliva; they always contain simple mucus. The word ranula, originally introduced from some supposed resemblance to the neck of a frog, is quite useless and had better be given up.

Another interesting class of mucous cysts are those which develop between the alveolar process of the superior maxillary bone and the lip. They are readily distinguished, and present a peculiar sensation under the finger, like parchment crackling, due to the thinning of the wall of the antrum. Muciparous cysts also sometimes form within the antrum, and these may produce by pressure so much absorption as to show themselves along the edge of the alveolar process, where it is not uncommon to find these swellings. It is probable that some forms of nasal polypus are of a cystic character.

In the neighborhood of the vagina cysts are very common. They are developed both from the glands of Cowper and from the mucous glands which extend up to the uterus. The labium is a frequent seat and may be enormously distended by one of these formations. Cysts here are often most painful, but their appearance and characters render the diagnosis easy. Higher up in the vagina the posterior wall is the most frequent seat.

Obstruction of the excretory ducts of the breast near the nipple often produces cysts at that part. Many present no difficulty in diagnosis. The contents may often be squeezed out through the duct. Sometimes an elongated distension of a duct is convoluted, and presents on section an appearance as if it consisted of many cysts. They are often accompanied by a new intracystic glandular growth.

The treatment of cysts of this class is tolerably simple. Where practicable, excision should be practised. In the floor of the mouth they often recur again and again and give rise to much trouble. A plan which has of late been adopted with success consists in dividing the mucous membrane of the mouth, making a hole in the sac and stiching that to the submucous tissue, so as to form a permanent fistulous opening and not any longer to allow the cyst to enclose the liquid secreted.

In the fourth lecture Mr. Birkett proceeded with his account of mammary cysts. Simple cysts in the breast, away from the nipple, are, he said, of rare occurrence. They present the remarkable character that the fluid they contain never coagulates by any means we can adopt. It is encircled by a delicate fibrous membrane. Such small cysts are sometimes met with scattered over the posterior surface of an atrophic organ. Mr. Birkett believes that they arise outside the true secreting membrane, in the space occupied by wavy fibrous tissue and oval nuclei, which lies between this and the limiting cæcal termination of the duct. The fluid contained in them is of a peculiar light-brown opalescent tint, and always very alkaline. When excessively alkaline the fluid sometimes becomes clouded by heat, but becomes clear again on the addition of a little acid. It never becomes turbid with acid only. These cysts occur usually about the middle period of life in all grades of society, in the robust and healthy, but almost invariably in association with some ovarian or uterine disturbance. When these cysts are very tense from fluid, and surrounded by gland-tissue, their diagnosis sometimes presents much difficulty. Fluctuation may be quite imperceptible, but there is always a marked elasticity about them, and not that dead hardness which a solid growth presents. The markedly globular form, and the presence of a distinct line of circumvallation around the cyst, are also distinctive characters.

Another class of retention cysts are those connected with the epididymis. The fluid in most cases contains spermatozoa. Most are doubtless formed from the tubes themselves by a local distension, but some have been thought to be due to the formation of a cyst in the connective tissue underneath the tunica vaginalis, in consequence of the rupture of one of the delicate tubules. Thus, instead of a retention cyst, we should have an extravasation cyst. Mr. Curling has shown that in some of these cases, by passing mercury along the vas deferens and injecting the tubules of the epididymis a certain quantity of mercury gets into the sac, there is reason to think apart from injury. Mr. Paget has suggested that the presence of spermatozoa may be accounted for by the fact of the same vessels traversing the sac which supply the epididymis. At the bedside these cysts are often thought to be ordinary cases of hydrocele. But the outline of the tumour, when of moderate size, is exceedingly characteristic. The distinction between the cystiform development and the testicle shows that the two are not in the same sac, and the axis of the swelling in many cases is not from above downwards, but transverse.

Cysts of the spermatic cord sometimes pass back through the external abdominal ring into the inguinal canal. They occur at a much earlier age than those of the epididymis, and are due to the canal of the tunica vaginalis being obliterated only in part. When they commence later in life they are probably due to local irritation, as from wearing a truss.

Another class of cysts are those which are due to the cutting off of a portion from an existing sac. Thus cysts are sometimes developed in connexion with an old hernial sac; and ganglia are probably of the same nature. Another important group of these cysts consists of those in association with the spinal cord and brain, meningocele, spina bifida, &c. The diagnosis of a connexion with the central organs is often of great difficulty, and an operation in most cases attended with considerable danger. Their congenital character will suggest their nature. Cystic growths on the face in early life are also to be looked upon with suspicion.

Cysts are sometimes connected with bloodvessels. A case of this kind was related. A small cyst, evidently containing fluid, existed on the abdomen of a French medical man, oval in shape, three and a half inches long, slightly discolored, looking very like fungoid disease just about to come through. A grooved needle was passed in, but only grumous blood escaped. The cyst was emptied thus, but in a few hours it had swelled up again to the original size.

In his fifth lecture Mr. Birkett described, first, the development of cysts from bursæ. If a bursa be exposed to frequent pressure, inflammation is set up in it, and its wall undergoes great thickening from the development of connective tissue. Trabecular bands are sometimes formed across its interior. Such cysts occasionally occur in positions where bursæ are not known to exist, but their identity in character with those developed from known bursæ renders it probable that their origin is similar. A cyst which was probably of this character was removed by Mr. Birkett from the front of the thigh. It was pyriform in shape, the prolonged extremity extending up behind the trochanter. In its interior were bands and minute outgrowths quite similar to those found in bursæ. In the museum of the College is an enormous cyst of a similar shape from the same position. A drawing and preparation were exhibited of another cyst removed from the forearm of a boy. It was situated beneath the flexor sublimis, and had a thick wall, with many trabecular bands crossing from side to side. The boy had been in the habit of carrying a basket on his forearm, and it was probably an instance of the development of a cyst as the result of local irritation and the thickening of the cyst-wall from the development of connective tissue.

The "dermoid" or "proliferous" cysts sometimes met with beneath the integuments and in the ovary constitute another interesting class. They are perfect in formation, consisting of the tissue of the dermis, with a cuticular lining, and on them hairs and true sebaceous follicles are developed. All are congenital, and there is little doubt that they owe their origin to some malposition of some of the minute elements from which the hair-follicles, sebaceous glands, &c., are developed.

A remarkable group of cysts are those sometimes called "serous" cysts or "fibro-cystic tumours," developed in immediate relation to the connective tissue of the body, and which seem to be associated with some vascular growths. They are most common in the neck and in the axilla, and in those loose parts where the connective tissue is in most abundance. Several examples of these growths were detailed at length. One of them occurred in a girl aged nineteen, and filled up the posterior triangle of the neck. It was quite superficial, and could be completely emptied by a single puncture. Its contents were pure serum. The bands crossing it consisted of simple connective tissue. Another, occurring in the axilla of a man aged twenty-eight, was so tense and lobulated as to resemble a fatty growth. Mr. Birkett believes that the multilocular character in these cases is simply produced by the mechanical distension of the cyst at its thinnest parts.

There is another remarkable class of cases, in which the cyst is first noticed soon after birth. An instance of this sort occurred in a boy, whose right axilla, when first seen at four years old, was filled with a large cyst, which extended forwards to the nipple, and backwards between the scapula and the chest. It was tapped several times, the serum being coagulable at first by heat and afterwards spontaneously. In 1862 a solid mass could be felt deeply seated, and the whole was excised. The character of the growth was remarkable, resembling in aspect some nævoid growths. Many lymphatic glands were unavoidably removed with the tumour, and Mr. Birkett mentioned it as remarkable that, although abundant lymphatic vessels were attached to the glands, not the slightest inconvenience in the arm was noticed in consequence of their removal.

In the sixth and last lecture Mr. Birkett concluded his account of cysts, considering especially those associated with intra-cystic growths. Some of these, he said, are glandular in character. The cyst in these cases may or may not communicate with the nipple. When it does, there is a constant escape of serous fluid. When the cyst is closed, its contents are usually tenacious and glairy, and always coagulate with heat or acids. German pathologists regard it as due to a peculiar "mucous" change occurring in the rapidly formed connective tissue, and not as a formative fluid, according to the older English view.

Another class of cysts contain within them growths composed not of glandular tissue but of elementary cells, epithelial-like structures, somewhat resembling those met with in carcinomatous growths, but arranged differently. Some of these cysts are connected with ducts opening on the nipple, some are not. As an instance of the former, the case was related of a lady, aged forty-five, in whose breast local hardness associated with a bloody flow from the nipple had existed for several years, dating from an abscess after a blow. Subsequently a soft tumour formed, the skin ulcerated, a small vascular flocculent growth projected from an opening in the integument, the nipple became everted, and the axillary glands tender. After excision there was a circumscribed cyst with firm fibrous walls, and from its interior three pedunculated masses of soft vascular new growth were hanging, composed of a fibrous tissue with reticular stroma, and an arrangement like papillæ or villi—a structure quite like that of granulations. Several similar cases were detailed by the lecturer: in nearly all, the disease dated from a blow or contusion upon the breast; in all, the fluid presented the same characters, differing entirely from the non-coagulable fluid from a dilated duct; in all, removal of the growth was not followed by recurrence. A strong proof of the innocent character of most cystic tumours of the breast was afforded by the comparative longevity of many suffering from them, and the absence, in most cases, of any traceable connexion between the tumour and the actual cause of death. When there is such a connexion, it is often, as it were, an accidental one, as in a case mentioned of a lady aged thirty-five, who had one of these cystic tumours, and after her confinement the whole mammary gland sloughed, and she died in consequence of the local sloughing. The lecturer described also the spontaneous cure by sloughing of a cyst in the breast probably due to a dilated duct. As an instance of the commonly satisfactory result of an operation in these cases, the case of a lady was cited, from whom a large tumour was removed in 1840, which would probably soon have destroyed her life by ulcerating. She died in 1860, aged seventy-three, of senile decay, never having suffered the slightest inconvenience after the operation.

The lecturer then briefly alluded to the rare occurrence of cysts in connexion with fibro-plastic tumours of the breast, tumours composed chiefly of those elongated fusiform cells which are associated with the development of connective tissue. Such tumours in the breast, he said, are not unfrequently mixed, sometimes with innocent growths of gland-tissue, occasionally with neighboring nodules of genuine cancer. In their recurrence also, they have a remarkable tendency to become cancerous in character.

Colloid tumours of the breast, in Mr. Birkett's experience, have been innocent in character. Cysts associated with carcinoma are by no means common, although about the middle of the last century the occurrence of bloody cysts in cancerous breasts attracted considerable attention. They are formed by the degeneration of the tissue of which the cancer growth is formed; during the softening a little blood or serum is easily poured out, and, collecting at various points behind or in front of the breast, it forms cysts. A cancerous growth in such a cyst is never pedunculated, like the innocent granulation growths already described; it is attached by a broad base, and seems to grow through the cyst-wall, or rather it grows from the tissue of the mammary gland, and then forms the cyst upon itself. In conclusion, an interesting and unique case was described, in which an apparently quite innocent cyst had become collapsed, and around it nodules of cancer had developed.

Foreign Gleanings.

SYPHILITIC AMBLYOPIA AND AMAUROSIS.

M. Galezowski has published in the *Arch. Gén. de Méd.* of Paris (first three months of 1871) an essay on this subject, the deductions of which are as follows:—1. Syphilitic retinitis and neuritis may exist without any alteration of the choroid, and are mostly constituted by an apoplectic retinitis with exudation. 2. Syphilitic retinitis presents no pathognomonic symptoms by which it may be distinguished from other kinds of retinitis. 3. But when retinitis or optic neuritis is accompanied by iritis or choroiditis, with or without flakes in the vitreous, the affection is, without doubt, syphilitic. He has always found that no other affection, save glaucoma, can give rise simultaneously to apoplexy of the retina and to iritis or choroiditis. 4. The derangement of the chromatic faculty is ever present in these two forms of ocular alterations, and especially in optic neuritis. 5. These complaints are best treated by large doses of iodide of potassium and corrosive sublimate (one drachm and a half of iodide of potassium, and from three-quarters of a grain to one grain of the sublimate per diem). 6. Syphilitic choroiditis is the most frequent form of syphilitic amblyopia and amaurosis. The signs of this choroiditis are very characteristic, and, as it were, pathognomonic of syphilis. They are as follows:—*a*, Disturbance or loss of vision, occurring in fits often at long intervals; *b*, a cloud in the shape of a cobweb constantly floating before the eyes; *c*, very frequent photoptics; *d*, photophobia; *e*, hemeralopia at an advanced stage of the complaint; *f*, preservation for a long time of central vision, with diminution of peripheric field; *g*, cloudy papilla; *h*, pigmentary retinitis at a more advanced period of the complaint; *i*, atrophy of the central vessels of the papilla with preservation of the rosy tint depending on the trophic or cerebral vessels of the optic nerve. 7. Pigmentary retinitis occurs very often as a sequel to syphilitic choroiditis. 8. The syphilitic pigmentary stains occur along the vessels of the retina, but they form, besides, circular masses in the shape of herpes circinatus. 9. Pigmentary syphilitic retinitis, the result of contamination, differs from congenital pigmentary retinitis (especially that form hitherto attributed to the consanguinity of the parents) only by the circular form of the pigmentary stains. 10. Congenital pigmentary retinitis is a hereditary syphilitic affection. 11. The latter complaint should, at a tender age, be treated by mercury or iodine. After a certain age the progress of the complaint cannot be stopped; it goes on increasing, and is followed at a more or less advanced period by loss of vision. 12. Children born of syphilitic parents should, at birth, be examined with the ophthalmoscope. If retinitis be found, the means just mentioned should be used.

CHYLOUS URINE.

The *Scalpel* publishes a case of this kind in a lady born in Brazils. She left her country at forty-four years of age, and travelled over the greater part of Europe. About six years ago, whilst at Florence, she noticed that her urine was lactescent. As she was getting weak, she went to the hills of Piedmont, where the urine became normal, after having been white for eight months. In Paris and London (in which latter capital she was seen by a well-known surgeon) the abnormal urine reappeared at intervals. On the fourth attack, in the space of fifteen months, she was seen by Niemeyer and Eggel. By analysis they found two kinds of albumen in the urine, and these physicians expressed the opinion that the fatty matter was not derived from the tissues of the body, but from a faulty digestion. This affection, so common in the tropics, is caused by weak assimilation, which itself is the consequence of the high temperature. The atrophy of the renal capillaries often coincides with the chylous dyscrasia, the latter being sometimes the consequence of the former. This lesion of the kidneys facilitates the transudation of albumen and fat of the blood into the urine. Chyluria and hæmaturia are two symptoms of the same complaint, which is well marked in infancy, diminishes in the adult, and disappears spontaneously in old age.—*Osservatore Gaz. delle Cliniche.*

FREQUENCY OF LARYNGEAL ULCERATIONS IN SYPHILIS.

Dr. Sommerbrodt, in a valuable article published in the *Wiener Med. Press*, gives the following figures:—Out of 100 post-mortem examinations of patients affected with secondary syphilis performed at Prague, Kühle saw ulcerations of the larynx 15 times; whilst Altenhofer met only 25 cases among 1200 living patients. Gerhardt and Roth, on the other hand, found it 18 times in 54 syphilitic patients—viz., 11 times with 44 secondary patients, and 7 times with 12 tertiary ones. Out of 1000 syphilitic patients Lewin found 44 presenting an affection of the larynx and more or less hoarseness. Engelsted counted 25 laryngeal cases among 521 patients suffering from syphilis—viz., 14 cases among 292 men, and 11 cases in 229 women. Dr. Sommerbrodt himself collected cases for nine months at the Allerheiligen Hospital; and out of 84 patients with constitutional syphilis, 15 presented ulcerations of the larynx in different periods, and 14 had catarrhal affections with hypertrophy of the mucous membrane. These lesions seem, therefore, rather frequent, as already stated by Turck in his manual. Among 288 persons suffering from various laryngeal affections, says the author, 45 belonged to syphilis. Laryngeal syphititic symptoms may occur in all the stages of the contamination. Turck, who has just been mentioned, observed it in a patient who had had primaries thirty years before; and Frankl has seen such ulcerations in a child two months old, with whom the first signs of syphilis had appeared one month after birth. The earliest time after primaries has, in Turck's time, been six months; in Sommerbrodt's, five months; and in Lewin's from two to three months. As to the seat of the lesions, it is indicated in the following table in reference to the 92 cases:—

Epiglottis			... 21 times.
True vocal cords	both	17	... 34 "
	right	4	
	left	13	
False vocal cords	both	2	...
	right	0	... 5 "
	left	3	
Interior of larynx	superiorly	9	19 "
	anteriorly	10	
Aryteno-epiglottic folds ...			... 6 "
Upper opening			... 2 "
Right pyriform sinus ...			... 1 "
Lower portion of the larynx			... 4 "

It will therefore be seen that the vocal cords are the parts most frequently attacked, especially the left. This circumstance should be recollected, as, in Rheiner's experience, tubercular ulcers are mostly situated in the *riyht.* This peculiarity has a diagnostic value. It is, in fact, towards the upper part of the larynx that the syphilitic ulcerations are found.

REVERDIN ON SKIN-GRAFTING.

M. Reverdin, to whom belongs the discovery of this method (introduced Dec. 8th, 1869), has just sent a paper on the subject to the Academy of Sciences of Paris. The author states, among other matters already well known, that the skin may be taken either from different individuals of the same species or from subjects of different species. It is impossible in the operation not to include some true skin with the epidermis. By means of the microscope it has been found that the adhesion of the transplanted skin takes place, first, by the epidermis, and secondarily by the dermis. Also that the epidermis acts by contact (the catabiotic action of Gubler), so as to induce the contiguous embryonic surfaces to take the shape of epidermis.

ALCOHOLISM IN FRANCE.

M. Bergeron presented, Dec. 5th, 1871, to the Academy of Medicine of Paris, his report on essays by M. Roussel (representative) and M. Jeannel respecting the repression of this vice. The conclusion of the author is that, as regards eventual improvement, attention is to be directed to the moral training and education of the bulk of the population. As a more immediate remedy, M. Bergeron advises to relieve wines from the greater part of the duties now paid, and to lay very heavy ones on spirits. Nor should the penalties for drunkenness advocated by MM. Roussel and Jeannel be neglected. Punishment should be inflicted with due regard to individual liberty and the rights of the insane. The report will shortly be discussed by the Academy.

News Items, Medical Facts, &c.

The Medical Profession in Russia.—From official papers it appears that during the past year 10,000 medical men carried on practice. Of these about 6113 were in the service of the Government, and 4686 independent of it. These figures give one medical man for 7182 inhabitants. In many districts a long journey must be performed before a medical man is found. In the United States of America, on the contrary, there are 49,798 doctors for the thirty-nine millions of inhabitants—viz., one medical man for every thousand inhabitants.

Bread Made with Sea-water.—M. Rabuteau, after considering the effects of sea-water in small or large doses on the economy, thinks that bread made with it might be taken with advantage in dyspepsia, phthisis, and scrofula. The bread is extremely pleasant to the taste. M. Rabuteau cites many authorities, especially seafaring men, to show how useful this peculiar bread has been.

At three inquests lately held at Liverpool on the same day, the jury in each case gave a verdict of "Died from excessive drinking."

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery,
Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 5. NEW YORK, MAY, 1872.

PRACTICAL PHYSIOLOGY.

Lectures
ON
EXPERIMENTAL PHYSIOLOGY.

By Wm. Rutherford, M.D., F.R.S.E.,

Professor of Physiology in King's College.

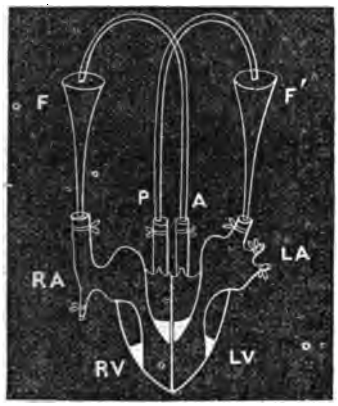
LECTURE XII.

CIRCULATION—(continued.)

The Pulse and Blood-Pressure.

Gentlemen,—In order to facilitate your study of the circulation, I have invented several models. In order to understand the action of the heart, study this model (Fig. 54). I have taken a sheep's heart, and have inserted the nozzle of a glass funnel into each auricle, a bent glass tube into the pulmonary artery, and a similar tube into the aorta. The patent venous openings of the auricles have been closed. The pulmonary artery tube has been arranged to pour its fluid into the left auricular, and the aortic tube to send its fluid into the right auricular funnel. I pour some colored water into both funnels, and now you have a simple piece of apparatus which any of you can easily make, and which is capable of showing you many valuable facts. When the ventricles are suddenly squeezed, you notice that a mass of fluid is projected through the arterial tubes into the auricular funnels, and that, as the ventricles are permitted to relax, the fluid descends in the funnels into the heart. [The action of the valves was explained. The competence of the sigmoid valves was shown, and it was demonstrated that if the funnel in the right auricle be filled with water, so as to cause distension of the right ventricle by the increased pressure, the tricuspid valve permits of partial regurgitation of fluid into the right auricular funnel ere the closure of the valve takes place. The importance of this "safety-valve" action of the tricuspid was indicated. It was shown that an increase of pressure sufficient to cause this regurgitation through the right auriculo-ventricular valve failed to do so through the corresponding valve on the left side of the heart. The probable explanation being that the mitral valve closes more perfectly than the tricuspid, and that, owing to the thickness of the wall of the left ventricle, it requires a much greater distending force to produce dilatation.] The mass of fluid which you see projected from the cardiac ventricles occasions the pulse.

Fig. 54.



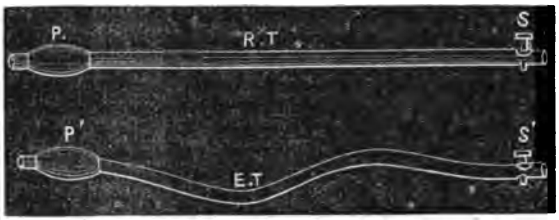
Rutherford's model of the circulation. A sheep's heart. R V, Right ventricle. L V, Left ventricle. R A, Right auricle. L A, Left auricle. P, Tube in pulmonary artery. A, Tube in aorta. F, A funnel in right auricle. F′, A funnel in left auricle.

I now take an ordinary elastic pump having a long india-rubber tube connected with it. When we pump water through the tube you feel that at each contraction of the pump the tube distends and then recoils. This distension is the pulse. It is simply a wave projected from the heart, and the wave is due to the mass of fluid thrown into the elastic tube. As the fluid flows out, the tube of course regains its former calibre. We coil the tube, and you see that with each pulsation the coils open up; there is elongation as well as lateral distension of the elastic tube. The elastic tube is, by the contraction of the pump, compelled to hold more fluid, and so it distends in every direction.

Commonly the pulse is confined to the arteries.

There is usually no pulse in the capillaries. Why is this? The reason will presently be evident. The elastic tube which we have been employing (Fig. 55, E T) has a pump at the one end and a stopcock at the other (P′ and S′). The pump represents the heart and the stopcock represents the muscular fibres of the small arteries. I have caught the idea of putting a stopcock to represent these fibres from the beautiful observations of my distinguished colleague Professor Johnson, regarding the altered condition of the small arteries in Bright's disease. Well, let us leave the stopcock fully open. Squeeze the heart as fast as you like, and you see there is a distinct pulse at the patent end of the tube. The flow of fluid is distinctly intermittent. Why so? Because the fluid can get out of the tube as fast as it is sent into it. Now this state of the stopcock represents a dilated condition of the small arteries; and it is a fact that when the small arteries are dilated the pulse is not confined to the arteries, but extends into the capillaries and veins. But now turn the stopcock and permit the fluid to flow out by a much smaller aperture. In doing so you offer resistance to the exit of the fluid; of course the more you close the stopcock the greater resistance do you offer. Now work the pump, and you need not do it very fast. See the result; the pulse has disappeared from the capillary stream at the nozzle, and there is instead of it a continuous flow. Why? Because the fluid cannot now flow out as fast as you send it into the tube. It therefore accumulates in the tube, and the time occupied by its escape fills up the intervals between the contractions of the heart. Here there is a demonstration of why it is that, in an ordinary case with semi-contracted small arteries, the pulse is confined to the arteries and does not extend into the capillaries. In accumulating within the tube, the water is not compressed as air would be; you know such fluids as water and blood are practically incompressible, but the tube, being elastic, distends, and the recoil of the tube keeps the water flowing during the intervals between the heart's contractions. But now let us perform similar experiments with a rigid tube (Fig. 55, R T). You must be struck by the fact that you cannot now get rid of the pulse at the nozzle, however small you make the aperture. Why this difference? Simply because the tube we are now using will not distend. The water being incompressible, and the tube non-elastic, the water cannot accumulate in the tube, and so no more can be forced in than what flows out. You see, then, that if the artery were not elastic the pulse would never disappear at the small arteries. Do not, however, jump to the idea that the elasticity of the arteries is simply the cause of the disappearance of the pulse. Were this the only factor in the case, then, with dilated small arteries, there ought to be no pulse in the capillaries. The calibre of the small arteries is the other factor which you must not forget.

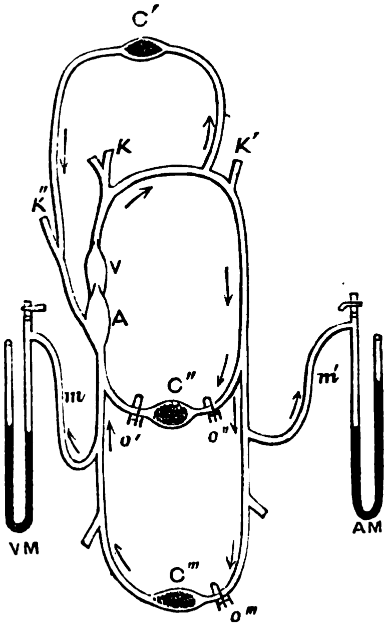
FIG. 55.



Rutherford's models to illustrate the pulse. P and P′, Two elastic pumps. S and S′, Two stopcocks. R T, A rigid tube. E T, An elastic tube. The tubes used are three or four feet long.

The cause of the disappearance of the pulse, and many other facts connected with the circulation, may be studied with much advantage on this third model or schema of the circulation. (Fig. 56.)

FIG. 56.



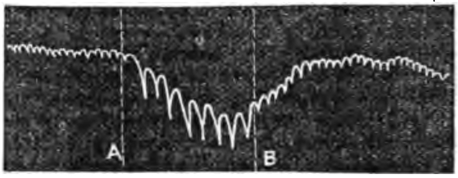
Rutherford's schema of the circulation.—V. Ventricle. A, Auricle. C′ C″ C‴, Capillaries of head, abdomen, and feet. O′ O″ O‴, Clamps for compressing the vessels. K, Innominate artery. K′, Left subclavian artery. K″, Right subclavian vein (these can be opened if necessary). V M, Venous manometer. A M, Arterial Manometer. m and m′, Tubes permitting the fluid in the schema to communicate with the manometers. The arrows indicate the direction of the current.

Weber and Marey have constructed schemata of the circulation; but I think I may, without need for hesitation, say that their schemata show little compared with the one I now exhibit. The schema simply consist of vulcanised rubber tubing filled with water. The ventricle of the heart (V) is an ordinary elastic pump. The capillaries (o′ o″ o‴) are represented by thin elastic bags containing sponge. The pores of the sponge represent the capillaries (read description of figure). The pressure of the fluid in the schema is indicated by two mercurial manometers—simple U-tubes with mercury. One manometer (A M) communicates with the femoral artery, the other (V M) with the femoral vein; so that the arterial and venous pressures may be compared. The clamp o‴ represents the muscular fibres of the small arteries. The clamps o′ and o″ are used to compress the abdominal vessels, and so to maintain them in a collapse state when necessary. The arteries are made of thick-walled rubber, because they have to bear much distending pressure. The veins, on the other hand, are made of thin-walled rubber, so that when the fluid is drawn out of them into the heart they may collapse. When I planned this model it was my intention to have this collapse taking place only in a large thin-walled auricle. This, no doubt, would have served all the purposes of the schema perfectly; but my friend Dr. Lauder Brunton suggested to me that the schema would more perfectly represent the natural condition of the vessels were the veins also made of walls thin enough to permit of their collapse. Acting upon this excellent suggestion, I have had the schema constructed as above.* (Fig. 56.) [In the schema which I at present use the valves are leaden balls. Such valves do not give sphygmographic tracings exactly like those got from the human arteries with the membranous sigmoid valves. When such tracing are wanted, one has only to adapt a lateral tube to one of the arterial tubes in the model of heart which I first showed you (Fig. 54). I am engaged, however, in the construction of membranous valves for the schema, and doubt not I will succeed in rendering it perfect even for sphygmographic purposes.]

You notice that in both manometers the column of mercury in the one limb is above that in the other. This indicates that the schema is overfilled. It is distended. The elastic wall is recoiling upon the fluid, and tending to drive the superfluous fluid out of the tubes. It would do so were it not that it is driving it against a heavy column of mercury, which it can only move for a short distance. The length of the mercurial column in the one limb above that in the other is balancing the pressure exerted upon the fluid by the elastic wall of the schema. The height of this column indicates the pressure that is being exerted by the elastic recoil of the schema upon the fluid. In other words, it indicates what is termed the pressure or tension of the fluid. Were the fluid blood, we should speak of it as the blood-pressure or blood-tension. To convince you that all this is so, I press on the wall of the schema, and you see that at once the mercury is still further depressed in the one and raised in the other limb of the manometer. A higher column of mercury is now supported, because the pressure on the fluid is greater.

I now imitate slowly the heart's action by squeezing the pump (V), and you see that an oscillation takes place in both manometers. The mercury rises for a moment and then falls; that indicates that the mass of fluid projected from the heart caused a wave of increased pressure to pass round the system. This is the pulse. This wave reaches the arterial sooner than the venous manometer. The pulse-wave travels rapidly, but, nevertheless, it takes quite an appreciable time to go round this not very long tube. The small arteries are dilated, so we have a pulse in both arteries and veins; but now I contract the small arteries, and you see the pulse is no longer seen in the venous, although it is as evident as ever in the arterial manometer. Well, I dilate the little arteries again. Pump as fast as you like, and you cannot keep the arterial much above the venous pressure. But in the body the arterial is higher than the venous pressure. Why so? Because the small arteries are to some extent contracted, and the fluid does not flow out as fast as it is sent in by the heart, until the pressure in the arteries reaches a height sufficient to give a velocity to blood flowing out of the system sufficiently great to cause the blood sent into the arteries at each cardiac contraction to escape before the next systole occurs. When the term "blood-pressure" is used without any qualification, the arterial blood pressure is indicated. The arterial pressure chiefly depends on two factors—(A) the rapidity with which blood is sent into the arteries by the heart, and (B) the rapidity with which it flows out through the small arteries. In one hand I hold the heart—the factor A. In my other hand I hold the capillaries and small artery (o″ and artery at o‴)—the factor B. Leaving B untouched, I vary the rapidity of the action of the heart, and you see how the pressure varies. It rises when I drive fluid into the arteries rapidly, and it falls when I send it in more slowly. But now let us keep the heart going at a uniform rate, and alter the rapidity with which the fluid escapes from the arteries. When I contract the capillaries, you see, the arterial pressure rises; and when I relax them it falls. Note well the influence of these two factors. I show you two tracings to illustrate this important point more fully. The first tracing (Fig. 57) was taking from a mercurial manometer in connexion with the carotid artery of a rabbit. The wavelets are the pulsations. The vagus was divided in the neck, and the inferior extremities of the nerve afterwards irritated. The period of irritation is that between two vertical dotted lines (A and B) which I have added to the tracing. You see the heart's action was rendered much slower by stimulating the cardio-inhibitory nerve. The rapidity with which the blood was sent into the arteries was diminished, and so the pressure fell, as indicated by a fall in the curve. Now look at this other tracing (Fig. 58). This was taken by me from the carotid of the same rabbit as the former tracing. This one shows the effect upon the blood-pressure of irritating the upper end of the superior cardiac branch of the vagus after it had been divided in the neck. The first trace (Fig. 57) showed the influence on the arterial pressure produced by stimulating the cardio-inhibitory nerve. This (Fig. 58) shows the effect of stimulating a vaso-inhibitory nerve. You remember that a great function of the superior cardiac branch of the vagus is to dilate bloodvessels. The line B indicates the pressure. The irritation of the nerve was begun at the vertical dotted line, and you see how the pressure fell during the irritation. The heart's action was slightly retarded, but this is quite insufficient to account for so marked a lowering of the pressure (compare Fig. 57). The major cause of the depression is due to the more rapid

FIG. 57.



Tracing from a mercurial manometer in connexion with the carotid of a rabbit. The wavelets are due to the pulsations of the heart. The fall of the curve indicates a fall in the blood-pressure. Between the lines A and B the lower end of the vagus in the neck was irritated, and the effect of this upon the blood-pressure is seen.

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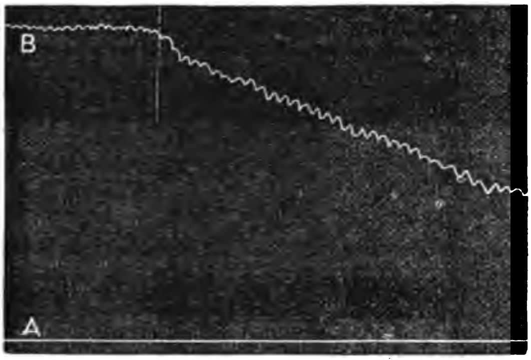
* It has been cheaply and satisfactorily made for me by the North British India-rubber Company, Canonmills, Edinburgh, from whom it can be obtained.

escape of blood from the arteries, owing to dilatation of the arterioles. Horizontal line A in the last tracing is an abscissa, to which I shall presently allude.

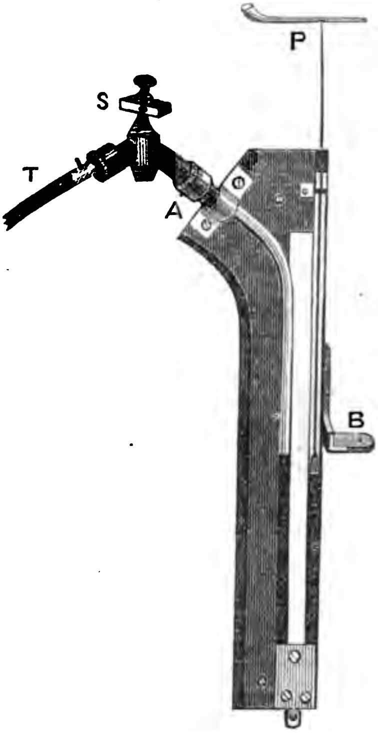
FIG. 58.



Tracing from same rabbit as Fig. 57, showing effect of stimulating the upper end of the superior cardiac branch of the vagus. B, The line of the blood-pressure. A, The abscissa line. The irritation was begun at the dotted line which intersects the line B.

The compression of bloodvessels by the contraction of neighboring muscles of course affects the blood-pressure. In consequence of this when we are measuring the blood-pressure in an animal we find that with every movement of the animal the blood-pressure rises. Such variations are very distracting, and are sometimes fatal to the success of the experiment. To get rid of them voluntary motion is paralysed by a watery (filtered) solution of curara. Respiratory action is paralysed by this, and therefore artificial respiration by means of bellows must be had recourse to. The dose of curara should just suffice to paralyse the voluntary motor-nerves. If the dose be excessive the vaso-motor nerves are also paralysed. The amount given should be six milligrammes for a rabbit in winter, and three milligrammes in summer; twelve milligrammes for a small dog, and eighteen milligrammes for a large one. The aëration of the blood must be thoroughly maintained in such experiments, else the accumulation of carbonic acid in the blood stimulates the vaso-motor centre, and causes a rise in the blood-pressure. The carotid is usually the artery in which the blood-pressure is measured. It is the one which is most convenient as regards position and size. The instrument employed for measuring the pressure is an ordinary manometer; here, however, it has received the name hæmodynamometer. There are several forms of this instrument. That most commonly employed consists of a U-tube containing mercury. I show you (Fig. 59) the hæmodynamometer employed by Ludwig (read description of figure). The motions of the mercury may be measured by placing a scale behind the mercurial column in the free limb of the apparatus. But such a mode is now superseded by the far more elegant and accurate method of registering the motions of the mercury upon a revolving cylinder. Excellent though Ludwig's kymograph be, a simpler and cheaper one is that I have adapted to my schema (Fig. 56). A simple U-glass tube of narrow bore (Fig. 60, M), half filled with mercury, and having at its one extremity a T-tube the stem of which is connected with a thick-walled elastic tube (E) bearing a clamp (S′), which can be screwed up so as to act as a stopcock. This tube is brought, by means of a glass nozzle, into communication with the bloodvessel. The third end of the T-tube is joined to a long elastic tube, communicating with a bottle (B) containing a saturated solution of sodium bicarbonate. This tube also bears a clamp (S). The tube (E) is filled with the alkaline solution, in order to prevent as much as possible the blood from entering the tube (E), and also to retard the coagulation. Before permitting the blood to leave the artery a pressure nearly equal to that of the blood in the animal operated on, should be produced in the tube (E). This is done by filling the tube by elevating the bottle (B) to the requisite height. When the desired pressure is obtained the clamps (S and S′) are tightened upon the tubes. In the course of the experiment it is sometimes necessary to wash out the apparatus so as to get rid of clots. This is easily done by elevating the bottle, and opening the clamp (S), the tube (E) being of course removed from its connexion with the artery. The pen (P), may be a sable hair brush, or glass pen (see Fig. 62, A and B) when the tracing is to be made with ink, or a light glass or aluminium stilette if the tracing is to be made upon a smoothed surface. The pen is borne by a thin

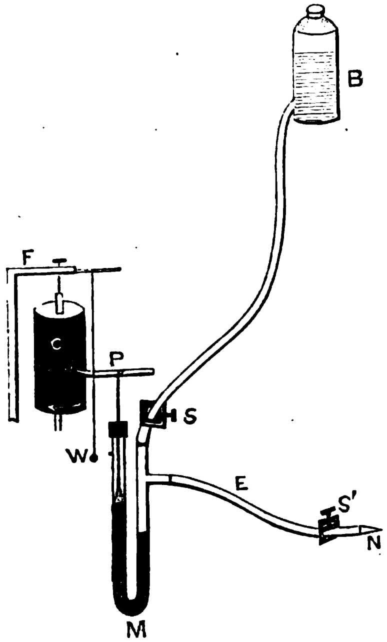
FIG. 59.



Ludwig's hæmodynamometer, or kymograph. Two glass tubes containing mercury communicate at the lower part of the apparatus, so as essentially to form a U-tube. A light steel rod having at its summit a glass pen, P, floats on the mercury. The pen registers the oscillations of the mercury upon a revolving cylinder. B, an iron bar by which the instrument may be fixed to a table bearing the revolving cylinder. T, A leaden tube which is brought into communication with the bloodvessel. S, Stopcock. A, Binding screw.

steel or glass rod, which is retained in a vertical position by a cap with a narrow hole through which it passes upon the upper end of the kymograph. The lower end of the rod is fixed in a vulcanite or glass float. The pen is easily kept against the cylinder by means of a thread with a weight at the end of it (see Fig. 60, W). A convenient cylinder to employ is Marey's, adapted for a continuous tracing when occasion requires (Fig. 29, THE LANCET, February number 1872.) The paper should always move towards the left hand, so that the tracing may be read from left to right. Why the Germans should make their instruments to give tracings which must be read from *right* to *left*, like Hebrew, I am always at a loss to imagine. It is just as easy to make them write from left to right as in the opposite direction.

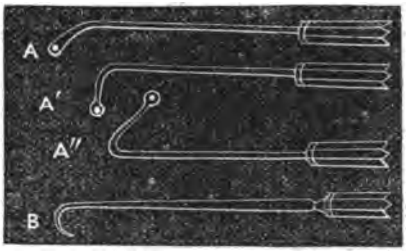
FIG. 60.



Mercurial kymograph or hæmodynamometer with cylinder, for registering the mercurial oscillation. —E, Elastic tube connected with bloodvessels by means of a nozzle. M, U-tube with mercury, B, Bottle with saturated solution of sodium bicarbonate. S and S′, Two clamps. P, Pen. F, Support for cylinder. W, A weight at the end of a thread. The thread presses against the pen (P), and keeps it against the surface of the cylinder.

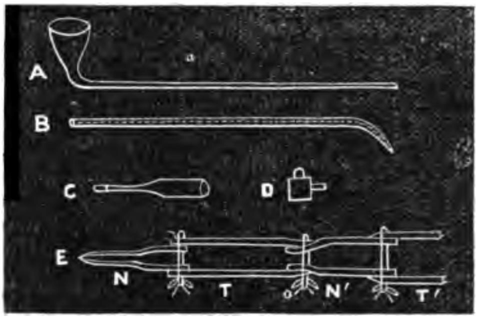
In connecting the myograph with an artery, first, the vessel is exposed and ligatured firmly; secondly, a pair of small artery forceps is placed on the artery on the cardiac side of the ligature, at the distance of about an inch from the latter; thirdly, a slit is made in the vessel, and a nozzle introduced and tied in. The introduction of the nozzle is often a difficult matter. It may, however, be rendered easy by the use of a little instrument (B, Fig. 61) invented by Ludwig. It is a hook-like piece of steel, with a groove on the convexity of the hook. The point of the hook is placed in the vessel, and the nozzle is pushed along the groove into the vessel. The nozzle may be glass or silver; some persons like silver ones of this form (D, Fig. 62). These are supplied with Ludwig's stromuhr instrument. They are of various sizes. I prefer glass ones; they are so easily made, and, as they cost little, their loss is never much to be deplored. One should be provided with nozzles of many sizes, and somewhat pointed at the extremity which goes into the vessel, and slightly fusiform near the point, so that when tied in the vessel they may be easily retained there by a ligature (C, Fig. 62). These nozzles should be previously tied to short pieces of elastic tubing (T, Fig. 62). There should be a tolerably large glass nozzle fixed in the elastic tube of the kymograph (E, Fig. 60), so that the connexion may be easily made by slipping the elastic tube (T, Fig. 62) over this glass nozzle (N′, Fig. 62).

FIG. 61.



A A′ A″, Three very convenient forms of ligature or "aneurism" needles. B, An instrument for introducing a nozzle into a vessel. (See text.)

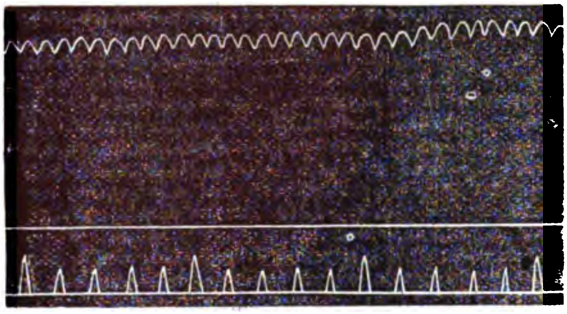
FIG. 62.



A, A hollow glass pen. The cup is filled with ink when required. B, Another form, consisting of a bent tube, containing a thread which is allowed to project for a very short distance beyond the pointed-or writing end of the tube. This acts as a fine brush, and, moreover, draws the ink to the point. C, Glass nozzle for artery or vein. D, Silver nozzle for the same, taken from Ludwig's stromuhr. The narrow end is put in the vessel, and the end next D is joined to a tube communicating with the kymograph. There is an eye on the nozzle for tying it to the neighboring tube. E shows connexion of the glass nozzle with the kymographic tube. N, Nozzle in artery. T, Elastic tube. T′, Elastic tube of kymograph (E, Fig. 60). N′, A glass nozzle permanently fixed in this. T can be readily joined to N′ when the apparatus is used in experiments.

If coagulation takes place, T (Fig. 62) is separated from N′, and N and T are cleared with a roughish whalebone probe, or a spiral rod made by rolling a narrow slip of blotting paper after the manner of a paper match. The blood is just permitted to spurt out to see that all is clear, and then the blood is washed out of the tube by injecting soda solution. The kymograph tubes are then washed out by a stream of the soda solution, the tubes are joined again, and the experiment proceeded with. Curara, or other poison, is injected into the external jugular vein. This is generally much to be preferred to subcutaneous injection. [An experiment was then performed, and a tracing similar to Fig. 63 taken. See description of figure.] To ascertain the pressure, I take a triangle of cedar wood, such as artists employ; a millimetre scale has been pasted on the short vertical side. I lay the wooden triangle on the abscissa, and place its edge perfectly parallel with that line. I then read off the number of millimetres between the abscissa and the pulse line, and double the number to get absolute pressure (see description of Fig. 63). I almost always ascertain the *mean* pressure. This is done at any point—of course taking the mean between the highest and the lowest. The mean pressure through a period of time, say twenty seconds, may be obtained by taking the highest and the lowest at a number of points, estimating the mean. The *number* of the pulse is accurately given by this instrument. To count the pulse accurately, I place the wooden triangle on the seconds line, and draw a vertical line to intersect the pulse and the seconds line. We then take a pair of compasses, open these to include say ten seconds on one side of our vertical line, and then pencil off the spaces on the pulse line immediately above the seconds whose breadth we took. We then count the wavelets in this interval. The *form* of the pulse is much better written by means of Fick's spring kymograph, or Marey's sphygmoscope, or the sphygmograph. The mercury, being heavy, has much inertia, and therefore acquires much momentum. On that account it is not suited for the transmission of delicate motions.

FIG. 63.



Tracing taken with kymograph. The upper line is the kymographic line, indicating the cardiac pulsations and the arterial pressure. The straight line in the middle is the abscissa line. This is produced by a pen fixed so as to indicate the position of the pulse-writing pen when there is no pressure being exerted upon the mercury at all. The blood-pressure is found by *doubling* the distance between this abscissa and the pulse line. It must be doubled because the mercury is in a U-tube, and the column is as much depressed in one limb as it is raised in the other. The time is registered on the lowest line. Each space between the peaks is a second. These seconds are produced by a pen connected with a metronome in a way afterwards to be described. For blackened paper the pitch-fork (Fig. 30, THE LANCET, February number, 1872) may be employed.

In the course of an experiment, say on the action of some poisonous agent upon the pulse and blood-pressure, I find it most convenient to draw a pencil line through the tracings whenever anything is to be noted. I write above the pencil line a letter *a*, *b*, *c*, and so on, and jot down in a book the notes under each letter. These are afterwards written in the proper places on the tracings. The results require to be tabulated. The following taken from my experiments on the vagus* may serve as an example:

Experiments 45.—Strong rabbit fed at 11 *a.m.; canula in carotid artery; trachea opened.*

Times.	Pulse in 10 sec.	Mean Pressure in inches of Mercury.	General Notes.
1 h. 28 m.	...	...	8 milligrammes of atropia sulphate injected into the jugular vein.
1 h. 35 m.	46	3	
1 h. 37 m.	44	2·75	
1 h. 44 m.	...	...	2 milligrammes of curara injected into the vein. Artificial respiration adopted.
1 h. 47 m.	44	2·8	
1 h. 48 m. 30 s.	41	2·7	
1 h. 49 m.	...	...	Both depressor nerves divided.
1 h. 51 m.	42	2·8	
1 h. 52 m. 10 s.	...	...	Right vagus divided in the neck, &c.

We may further simplify matters by transferring the results to charts, such as those used for weather tables, and which are now so commonly used in clinical medicine. On these we may make one trace represent the numbers of the pulse, another the pressure, and a third the respiration. The respiratory movements are usually represented in the pulse line in such tracings. They may, however, be accurately written upon the same paper by means of a special instrument, to be afterwards described. These trace lines are made in this way: a series of dots, representing the numbers of the pulse, are put down in their proper places, and then these are joined by lines.

This mercurial kymograph is undoubtedly a most valuable instrument. It is used in every research where the influence of any toxic agent upon the vascular system is tested by experiments on the lower animals.

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Clinical Lecture

ON THE

EARLY HISTORY OF CALCULOUS DISEASE,

AND THE TREATMENT BEST ADAPTED FOR ITS PREVENTION.

Delivered in University College Hospital in May, 1871.†

By Sir HENRY THOMPSON,

Surgeon Extraordinary to H.M. the King of the Belgians, Surgeon and Professor of Clinical Surgery to University College Hospital.

GENTLEMEN,—We have recently studied together and discussed very fully the various operations which are practised for the removal of stone from the bladder, and you have had the opportunity of seeing them performed many times, not less than eleven cases having passed through my wards in a few weeks, each one with a successful result.

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* Influence of the Vagus upon the Vascular System. Trans. Roy. Soc. Edin., 1870.

† Printed from the Reporter's shorthand notes.

But, satisfactory as all this is, it suggests to my mind very strongly that there remains an important question for us to consider; as important, indeed, as any of the preceding, and one which must naturally arise in all thinking minds. It is this: Is there not a period anterior to the stage of the malady already examined—a time at which we might prevent the formation of stone in the bladder, and so get rid of the necessity for removing it? Admirable as the results of operative means have been—perfect (one may almost say) as they have become, at all events so far as regards the crushing operation—great as is the triumph which surgery has achieved in removing stone from the bladder—I take it there are very few men indeed who would desire, if they could help it, to exhibit that triumph in their own persons; all would be infinitely better pleased if the formation of stone were prevented, rather than removed, however cleverly you may perform the operation. This, then, leads me to the consideration of an important question,—Can we do anything to prevent the formation of stone in the urinary passages? It is, in fact, the early stage of this malady that will be the subject of our lecture to-day.

I commence by saying that I think a great deal may be done. But at the outset of the inquiry we naturally ask, What is the kind of stone (for there are several kinds) the formation of which we may hope to do most in preventing? All calculi are of local or of constitutional origin. By "local" I mean formed by disease in the bladder, and not depending upon any constitutional conditions; by "constitutional" I mean formed by some vice in the system. Now, the large majority of stones are of constitutional, and not of local origin. When they are local you know that we cannot prevent their formation except by mechanical means. Calculous matter, the elements of which are produced in the bladder itself, may be washed out, or may be broken and then washed out. But when stones are of constitutional origin—and those are what we are going to refer to entirely to-day—the elements are separated from the blood, and no mechanical means of prevention can be thought of for an instant. Now, from observation we know that nineteen out of twenty of such stones have uric acid for their basis, the remaining one in twenty being oxalate of lime;* and, less commonly still, there are phosphatic stones which are of constitutional origin also. Therefore, practically, to all intents and purposes the problem before us is how to prevent the formation of uric-acid calculus.

Let us examine the early history of a case of persisting uric-acid deposit? First of all, let me say, going back to the root of the matter, that it is generally more or less hereditary. As a single illustration of this, we have just seen a case of uric-acid calculus in the ward, and learned that his father had "gravel or stone for the last twenty years of his life." And my almost invariable experience is, that either calculus or gout, most commonly the latter, has been observed in the family of the patient who comes to me with one of these formations in his bladder. I believe it, then, to be very strongly hereditary. We speak of tubercular disease and of cancer as being transmitted by blood relations, especially the former; but I do not think even it is more certainly hereditary than the disposition to uric-acid deposits in one form or another. I make a point of asking the question of all patients who come to me with this complaint; and although I cannot at present furnish you with an exact numerical statement, I do know that in a very large majority, gravel or gout (for I wish to show you the identity as to the origin of the two complaints) has existed in the preceding generation; indeed, it is rare to find it otherwise. This hereditary tendency varies in force or strength in different families. You will find some persons with persisting uric-acid deposits at thirty years of age, others at forty, others at sixty. Of course, the earlier the time at which it appears, the stronger you will find the hereditary disposition, and the more obstinate, probably, will be its tendency to persist.

What, then, are the first signs of this condition in the patient? Usually, the first sign is that the urine deposits pinkish matter, on cooling, at the bottom of the vessel, or the secretion may be merely cloudy when cold. It is passed quite clear, becoming cloudy only when it acquires the ordinary surrounding temperature. This phenomenon, therefore, may appear more frequently in winter than in summer, because the external temperature is lower. It is simply a deposit of salts from a hot solution as the liquid grows cooler, all being easily dissolved by raising the temperature of the liquid to that at which it was originally passed. This is a condition of urine which very often and very unnecessarily excites much anxiety on the part of patients, and only the persistence of which can be looked upon as a sign of what is called "the uric-acid diathesis." Mind, I mean strictly persistence, or at all events frequency of occurrence; for you or I, with no hereditary predisposition, may take a little more beer than usual, or an extra glass of champagne, or a glass or two of unaccustomed port, and find next morning a considerable quantity of this pinkish deposit, the urine looking almost like pea-soup, but not so thick; and when the vessel is tilted on one side a tidal mark, so to speak, is seen, showing the height at which the liquid stood; all this, as I said before, being redissolved by heat. But if, without any errors of diet, of which any but a very small allowance of alcoholic drink is only one, a patient habitually passes this kind of urine—if in time there is a frequent deposit of uric acid, manifested by the presence of little crystals looking like particles of cayenne pepper at the bottom of the vessel—when this occurs rather early in life, say before forty, there is not much doubt that there is a strong tendency to produce uric acid, either inherited or acquired. For this tendency may to a certain extent be acquired, or a pre-existing habit may be intensified; but, as I have before said, it is almost always inherited.

We have followed the complaint up to the formation of cayenne-pepper crystals. I now show you a specimen of urine quite cloudy with lithates, although you must be familiar with it in the wards, and how on heating the liquid it again becomes clear, and that in a short time, while we are talking, it again becomes cloudy on cooling. Let me once more remind you that this may happen with the most healthy individual; and it is only the persistence of the symptom, without errors of diet, which should lead you to suspect a condition that requires treatment. Of the cayenne-pepper deposit I have some very good specimens here, which have been collected from patients who passed it habitually. These consist mainly of the transparent rhomboidal uric-acid cystals, which you know to be very beautiful objects under the microscope. They may be passed almost daily and habitually, or only every month or six weeks, in large quantities, when they usually produce some irritation. At such periods the patient may experience pains in the back and great discomfort, and be then said to have an attack of sand or gravel. These attacks may occur at varying intervals, and usually become more frequent or severe, unless the patient does something to prevent the formation. Afterwards he passes tiny calculi, popularly called "gravel," which seem to be rounded aggregations of the same crystals; and these little bodies tend in time to become larger, sometimes as large as small peas, or even beans; and they are still specimens of the same product—that is, of uric acid, associated more or less with some base, such as soda or ammonia.

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* The deposits of oxalate of lime and of uric acid so often replace each other that the consideration of the latter becomes practically generally sufficient for our purpose.

Now let me recall for one moment what I said as to the relation of gout to this condition. I have frequently seen these two complaints alternating, comparing one generation with another: gout appearing in one, gravel in the second, and then gout in the third. But the same invidual may also have alternating attacks of gout and gravel. I have seen a patient suffering for years from gout, which ceased for several months, when he developed for the first time a uric-acid stone. Lastly, the so-called "chalk-stone," which you have often seen infesting the knuckles and disfiguring the hands of elderly people in advanced stages of gout, are composed of the same material—that is, of uric acid, usually as urate of soda. The identity of the two things, then, is unquestionable; they constitute two different series of phenomena, but both spring from the same root.

Now what is to be done for these cases? What mode of treatment will help to prevent the arrival of at least the advanced condition—namely, that of calculus too large to be voided by the patient? Generally speaking, I think we see such patients in a tolerably early stage, although this is by no means always the case. Some are much alarmed at a very early period, when the urine is only occasionally thick with urates. You will of course disabuse such patients of their false impressions, because numbers of persons mistake such thick urine for highly organised matter. I have known persons to become almost hypochondriac through not knowing that such deposits are of little consequence at first, and can be easily treated. But what are we to do for those who habitually pass the cayenne-pepper crystals of uric acid or small calculi? You will first seek the patient's antecedents, and learn all that he has to tell you of his habits, his diet, and his family history; and your mode of treatment will be determined accordingly. First of all, let me speak of the general principles upon which the treatment should be conducted. A very simple rule—indeed, too simple, I think—is often adopted. when the urine has persistently and habitually thrown down acid deposits, the patient has generally been prescribed alkalies; if, on the contrary, he has had alkalines deposits, he has been treated with acids That simple mode has too often formed the main portion of the treatment. In the former case he has soda or potash largely administered, or he will be told to drink so many glasses of Vichy water, which is mainly a strong solution of carbonate of soda, only a natural instead of an artificial one. Now it is quite true that with alkalies, provided enough to be taken, these deposits will disappear: the uric acid will no longer be deposited; the urine will become less irritating; the annoying symptoms will be diminished or got rid of. And of course the patient is very much pleased with this new condition of clear urine and disappearance of all deposit. And you will say, "What more can be desired?" This: you have merely made his enemy disappear, but he is by no means rid of its presence; you have not checked the acid formation. The uric acid is there as ever; but the uric acid and the urates are soluble in alkali, and you have only made them invisible. You really have the same condition as that of the fabled ostrich, which is said to put its head in the bush when pursued by hunters, and, no longer seeing them, believes itself secure. Just such is the security of the patient with uric acid who trusts solely to alkalies or Vichy water. His surplus deposits have become imperceptible to *his* vision; nothing more. I do not say the alkalies have been absolutely unserviceable as regarding his constitutional state, but they will not improve it to any great extent; and when he leaves them off the acid shows itself again. Diuretics must be regarded in the same light. In those cases which are treated with diuretics, the secretion of water is no doubt increased *quoad* the amount of solids, and the solids are thus dissolved. In both instances what you have chiefly done has been, to stimulate the kidneys, already overworked, to do more. You have by no means cured the patient.

Now let us ask what is the real pathology of these cases, and then I think I shall be able to show you a more efficient remedy. The problem has presented itself to me with great force and frequency, because people, naturally fearing they may arrive at the stage of calculous formations, come for advice in the earliest stages, and with the strongest desire to avoid the advanced one of stone in the bladder. So far from sending them to Vichy or giving them alkalies, I believe they can be more effectually dealt with by a different mode of treatment. Let me premise, in broad and simple terms —as our time here, and, I may perhaps add, the extent of our knowledge, will not permit me to be more minute or exact in detail,—that the origin of what we call gouty symptoms, as well as of a superabundant uric-acid deposit in the urine, is due to defective assimilation on the part of organs associated with or forming the primæ viæ. I am quite aware that it is common in practice to speak somewhat knowingly of the liver, its actions and its states, although we have still a good deal to learn about all this. Some years ago we talked and acted as if we were thoroughly acquainted with the liver and its functions; but during the last fifteen or twenty years new light has been thrown upon the subject by Bernard, Pavy, and other workers in the same field, and we have learned that the more we inquired the less did we certainly know of its natural functions, still less of its action in disease. Thus, if one thing was more settled than another, at least since the time of Abernethy, it was that mercury had a specific influence on that organ; but now we find there are grounds for believing that the action supposed has no existence at all. There were other things, indeed, which were vaunted to take the place of it, but no one ever thought of disputing the fact that you could augment the bile secretion by administering mercury. I am not here to say whether that is so or not, but it seems to have been proved that there are substantial reasons for doubting if our ancient faith in that dogma be tenable. In speaking, then, of the "defective action of the liver," or of "torpor of the liver," I merely use provisional terms, which most understand as indicating more or less distinctly a certain set of symptoms. Let them be briefly described as mainly consisting of a constant, or almost constant, deficient excreting function by the bowels, sometimes, but by no means always, associated with impaired appetite and slow or uneasy digestion; these latter being often absent if the diet is carefully selected, or if the patient lives in the open air and takes much exercise. On the other hand, considerable and multiform symptoms of disturbed digestion may be frequently present. I cannot positively state whether those phenomena are really due to inactivity of the organ in question; practically, for us to-day, this does not signify much, but the current terms are still convenient formularies until others can be substituted for describing the condition in question.

Now, at the bottom of this tendency to uric-acid production there often lies what is thus understood as inactivity of the liver; and the true *rationale* of the undue formation of the urinary salts appears to be that, the liver or some allied organ not doing its duty as an excreting organ, the kidneys have more work thrown upon them. Thus the solid matters of the urine, or rather some of its ordinary constituents, are augmented,—not all of them, for urea is not necessarily increased, but uric acid is largely produced, and is eliminated not only in solution but in crystalline forms. Uric acid is very insoluble in water; and although the quantity thrown out may be quite soluble at the natural temperature of the urine (100° Fahr.), when this diminishes to 60°, 50°, or 40°, the acid is deposited; and when the quantity is still larger, even the ordinary amount of fluid associated with it at a temperature of 100° will not suffice to dissolve the whole, and solid uric acid is deposited in some part of the urinary passages. This deposit may take place in the kidneys themselves, giving rise, if not thrown off, to the formation of calculus, at first renal, but sooner or later mostly becoming vesical. Now, if all this be so, the formation of uric-acid gravel is not by any means to be regarded as necessarily disease of the kidney; on the contrary, it is the result of an active and capable organ vicariously relieving some other organ which is torpid. The true remedy therefore is, not to stimulate the kidneys, already overworked—not, to use a familiar simile, to spur that horse of the team which is already doing too much work, but you are to seek the cause in that other one of the team which is doing deficient work, and that is almost invariably the liver, in the sense already explained.

The treatment, then, which I advise you to pursue is to employ such agents as will stimulate the excretory action by the primæ viæ without depressing vital power. No doubt that a powerful agent for the purpose is mercury; and it is quite unquestionable that the relief of the symptoms above alluded to is to be obtained in a remarkable manner by occasional small doses of that drug. For our purpose, however, it is neither so successful in action, neither can it be considered so harmless, as another class of agents, which in these cases I believe to be in every way superior—I mean certain kinds of natural mineral waters. These I also regard as superior, in these maladies, to taraxacum, nitric acid, alkalies, and the other substitutes, as they have been termed, for mercurial remedies, in promoting the function of the liver. The mineral waters which I refer to belong to a group of springs all containing sulphate of soda, and some of them sulphate of magnesia also, in solution. In studying these waters, I wish you to look with me at the composition of them, and at the same time to dismiss from your minds entirely those views of medicinal doses which you have acquired in the dispensary, and which necessarily belong to it, since small quantities of drugs, as they exist in mineral waters, will act more freely than will those quantities combined after the ordinary pharmaceutical method. You ask me for a demonstration, and I am quite ready to give it to you. At the same time, let me caution you against regarding the small doses of mineral waters as having any affinity, either in the matter of quantity or by manner of administration, with what is understood as homœopathic doses. Thus, for example, you know that you may give A an ounce of salts, or B half an ounce, and you purge them; but you may obtain the same effect with one-fifth of those quantities if you give it as prepared in nature's laboratory in the form of mineral water. It is a curious fact, which I give as an ultimate one, and without speculating here on the cause of the difference. As a proof of the superior force of the saline combinations found in natural springs, I may refer you to the following experiment. If you will reduce by careful evaporation, as I have done, such mineral waters to their pharmaceutical condition of crystallised salts, you will find them possessing little if any more power than similar salts as obtained by the ordinary processes, and met with in every chemist's shop. They no longer do their work on the same terms as when administered in the original water before they were separated by evaporation. You will therefore readily understand how essential to our end it is to employ the natural mineral waters; since what are called "artificial waters," however admirably prepared, are simply pharmaceutical products, and are destitute of the very quality which distinguishes the remedies they are designed to imitate.

Here is a table of the waters which I refer to, with a comparative synopsis of their distinguishing saline contents, representing the number of grains

—	Sulphate of Soda.	Sulphate of Magnesia.	Carbonate of Soda.	—
Saline:				
Pullna	154 grs.	116 grs.	—	
Friedrichshalle ...	58 „	49 „	—	
Marienbad (Kreuz)	48 „	—	9 grs.	Little iron.
Carlsbad (Sprudel)	25 „	—	13 „	
Franzensbad ...	30 „	—	6 „	Little iron.
Alkaline:				
Vichy (Celestins) about	30 „	—	47 „	Little iron.
Vals(Magdeleine) about	—	—	65 „	Little iron.

(without chloride of sodium and other less active agents) in an English pint. Below these I add two well-known alkaline waters.

The most powerful of this group is that of Püllna, which contains 154 grains, or nearly 2½ drachms, of sulphate of soda to the English pint, and nearly 2 drachms, of sulphate of magnesia. Those quantities would give a tolerably efficient purge to anybody. But you must not give a pint of Püllna; 5 ounces would be a full dose. I do not like Püllna generally for our purpose, because it purges too freely, often gripes, and is very nauseous. Half a drachm of sulphate of soda and half a drachm of sulphate of magnesia in this form is too much for many people. I therefore recommend Friedrichshalle, which contains not a drachm of sulphate of soda in a pint, and little more than three-quarters of a drachm of sulphate of magnesia. Nevertheless, you would not think of giving a pint; eight or nine ounces makes an efficient purge; for many persons six or seven suffice. I think I may say that seven ounces is an ordinary average dose. If you take seven ounces of Friedrichshalle water an hour before breakfast, and soon after a cup of hot tea or coffee, you will have a full, free action of the bowels; perhaps two. That, you see, would be about twenty-five grains of sulphate of soda and twenty grains of sulphate of magnesia, which, taken in any combination you like out of a druggist's drawer, would have no appreciable action; you might be a little uncomfortable perhaps, but there would be no action of the bowels. To repeat what I have said: if you evaporate a quantity of Friedrichshalle water in a warm water bath, and obtain as perfect a product as a chemist can produce, and administer four times the amount of salt in that form which exists in a dose of the natural water, you would still not have such efficient or such certain results as from the small quantity (in the natural water) named above. So that there is something, which I do not pretend to explain, and certainly shall not speculate about here, which distinguishes the action of mineral waters from the action of salts which are produced pharmaceutically. The next water on my list is Marienbad, which contains no sulphate of magnesia, forty-eight grains of sulphate of soda in the pint, with nine grains of carbonate of soda, and a small quantity of iron. About half a pint produces for most persons an easy motion. If this water is exposed to the air for a day or two, there will be an obvious brown deposit of the iron, and it may be regarded as slightly ferruginous, though that is a secondary character. The next is Carlsbad with its many springs, all of which contain about twenty grains of sulphate of soda and thirteen of carbonate of soda in the pint. Then we come to Franzensbad, which contains thirty grains of sulphate of soda, six of carbonate, and a little iron, which Carlsbad does not. That closes this group of springs.

But now I shall just point out the distinctive characters of the alkaline waters which are so popular in this country. First, Vichy, which contains only three grains of sulphate of soda, but nearly fifty grains of carbonate of soda, in the pint —a powerful solution. Then we come to Vals, which is also from the volcanic district of France, some of the springs of which contain upwards of sixty grains of carbonate of soda, and nothing else worth mentioning. These two waters are extremely famous, and are much resorted to against gout and gravel. Under their use the uric acid deposits disappear—that is, they are dissolved by the alkali. Inasmuch also as this appears to have some beneficial action on the liver, a certain degree of permanent benefit is perhaps also attained. Thus such patients are often better for a time after a visit to Vichy; but, as a rule, are not permanently benefited. I am satisfied, after observation on the spot, and on the effect of the waters here, that they only temporarily mitigate the complaint, and do not cure it. Now, the principle upon which the waters of Friedrichshalle and Carlsbad are beneficial is, that they produce activity in all the digestive functions, and thus waste matters which have been hitherto thrown out as uric acid by the kidney are eliminated in some other form. And thus it is that, if it be necessary to send these patients abroad, I prefer very much Carlsbad to Vichy, provided always that the subject of a Carlsbad course must not have become too weak, as for a time it makes a demand on the strength not well supported by a feeble person. Generally, however, this is not at all the condition of those who are passing uric-acid gravel. Happily, also, for most people our purpose can be attained as well at home. I believe that a short course of Friedrichshalle first, followed by Carlsbad, or by a combination of the two, produces the best results in these cases. This method has, at all events, been more successful with me than any other. Such a course should be continued, according to circumstances, for six or eight weeks.

A few words upon the way in which you should give them. If you have a patient coming to you whose digestion is not good, complaining of foul tongue, with deranged digestion and loss of appetite, it is sometimes, not always, desirable to give first a single dose, no more, say three or four grains, of blue pill at night, and the next morning eight or ten ounces of Friedrichshalle water, so as to ensure good action at first. Then commence with Friedrichshalle combined with hot water, one dose every morning an hour before breakfast, diminishing a little the quantity every day or every few days. One of the characteristics of this water is that the longer it is taken the smaller is the quantity necessary to effect the purpose. If, for example, seven or eight ounces taken in the morning, say with five ounces of hot water, produce one active movement of the bowels immediately after breakfast, the next morning six or seven ounces will do the same, and the morning after probably five or six ounces; and it is very likely that at the end of three weeks the patient will from four ounces experience the same effect produced originally by seven or eight. But after you have given it thus for one, two, or three weeks, according to the nature of the case and the results, you should combine it with Carlsbad, say three or four ounces of the former and five or six of the latter, with three or four of hot water every morning. When Friedrichshalle is given alone, and also when it is mixed with Carlsbad, 20 or 30 per cent. of hot water should be added, that it may resemble somewhat the original condition at the spring. Friedrichshalle is naturally hot, and is evaporated on the spot to a small extent, being regulated and rendered uniform by stopping the process when the water reaches a certain specific gravity. Carlsbad, which at the spring is too hot to drink until cooled, should, when taken alone and unmixed, be raised in temperature to 90° or 100°, by placing the tumbler containing it in a vessel of hot water for a few minutes. After giving this combination of the two waters for two or three weeks, six, seven, or eight ounces of Carlsbad may be taken alone for another fortnight perhaps. The quantities given are considerably less than those administered at the spa itself, where a patient's stay is necessarily limited as to time. I am quite satisfied that the smaller quantity here recommended, and employed for six to nine weeks, instead of the usual three weeks of a foreign course, is better for the majority of the patients we have to deal with. The same quantity of water given there in twenty-one days, producing often notable loss of weight and power, will, if given here in fifty or sixty days, attain the object as certainly and more safely. I by no means dispute that there are other cases which may be benefited by the more heroic plan adopted on the spot. I have largely and systematically employed these agents now for seven or eight years, modifying the quantity and the mode as experience has indicated, and the course thus briefly described is the result of it. The course may be repeated with advantage, if necessary, for most patients after an interval of three or four months. Meantime, as an occasional aperient and a corrector of digestion for these patients, I know knothing at all equal to Friedrichshalle. It leaves the patient as a rule less constipated after discontinuing it than he was before; and, as already said, may be taken habitually without lowering the system. I have known patients continuing its daily use for three or four years, but I do not advise this course unless in exceptional cases. Nevertheless I know a gentleman, nearly eighty years old, who has taken it for five years regularly, a wine-glass every morning, and the effect has been for him most admirable. Suffering much formerly from an obstinate constipation, he enjoys perfect regularity and excellent health. Let me say here that what is called "Carlsbad salt" is often used for the same purpose, and is taken also in the belief that it represents Carlsbad water. This it does not do, consisting almost entirely of sulphate of soda taken from the water, and has the same and no more virtue than that salt when obtained from any other source. No doubt that the sulphate of soda, known also as "Glauber's salt," is one of the most admirable medicines we possess, and deserves to be more popular than it is. I constantly order it, with or without a small addition of sulphate of magnesia, for the out-patients as the best substitute within my reach for the mineral waters in question.

In the few minutes which remain I will advert briefly to the subject of diet, certain restrictions in which are extremely important. It used to be said that when uric acid is largely deposited the nitrogenous elements of the food should be considerably diminished. I do not find in practice that a strict application of this rule is advantageous. On the contrary, the diminution of the deposit is more certainly attained by a course which is almost the opposite of that. There are three classes of aliments which must be permitted to the patient very sparingly, in order to attain the end in view—viz., alcohol, saccharine, and fatty matters. First, alcohol: any fermented liquor permitted as an article of diet should be selected in its more diluted and in its most pure form. Believing that form in which it is found existing in natural wines to be usually the best, I would advise a light sound Bordeaux or a Rhine wine of similar quality, the former perhaps agreeing better with most persons here. You will forbid champagne, as for the most part imperfectly constituted, and always bad if containing much liqueur. The stronger wines, as sherry and port, are mostly unsuitable, and strong beer is to be absolutely forbidden. Solutions of pure spirit and water are exceptionally desirable for some few persons with weak digestion. Secondly, sugar in all its forms: at every meal and wherever met with, forbid it altogether. Thirdly, let fatty matters, butter, cream, and the fat of meat, whether simply cooked or in combination to form pastry, be taken very sparingly. I cannot enlarge on the theory on which this advice is given. Suffice it to say that abstinence from the substances named probably lightens considerably the work of the liver, and so lessens the vicarious labor of the kidneys in accordance with the views already propounded. Let me just advert, moreover, to the dietetic system at Carlsbad. In recognition of some such principle there, no doubt, the use of sugar and of butter is absolutely forbidden during a Carlsbad course; and were you patients there, your purveyor would not supply you with the forbidden food, however much you might demand it. I can only say, as the result of observation, that this system, much more than the elimination of meat from the dietary, will reduce the uric-acid deposit. If you will cut off a portion of alcoholic stimulant when necessary, and it often is so, will forbid everything that contains sugar, and diminish considerably all fatty matter—giving nitrogenous food, in fact, and eliminating hydrocarbons—you will generally accomplish more than by the contrary method. In addition to all this, you will of course see that the patient takes daily a fair amount of exercise in the open air, and that he protects his skin and encourages the performance of its functions by habitual ablutions and sufficient clothing. These points I can only name; but they are essential concomitants to the rest.

Well, then, it is this system of diet and regimen, and the occasional systematic employment of the mineral waters named, which mainly constitute the treatment I strongly advise for the purpose of checking calculous disease in its early stages, and so to prevent the formation of stone in the bladder in that considerable majority of cases which are due to uric-acid formation and its consequences.

Clinical Lecture

ON

THORACIC ANEURISM

Delivered at the Liverpool Royal Infirmary.

By A. T. H. WATERS, M.D., F.R.C.P.,

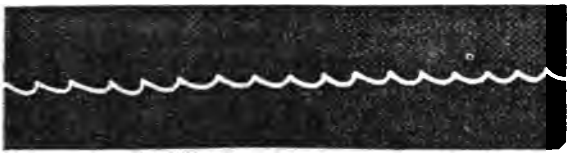
Physician to the Infirmary.

GENTLEMEN,—The subject of our lecture to-day is Thoracic Aneurism; and as there have been three cases of this disease under my care during the past few months, I wish to call your attention to them, and to make them the subject of some general remarks on the treatment of the affection.

Few diseases are more interesting to the practical physician. Thoracic aneurism is usually very insidious in its origin; its early symptoms are frequently very obscure; its progress is at one time rapid, at another time slow; its diagnosis, simple when the disease is fully developed, is extremely difficult in its early stages; and, unfortunately, its tendency is, sooner or later, to a fatal issue. But whilst, from the very nature of the disease, a cure or complete recovery cannot be anticipated, there can be no doubt that in most cases a goed deal may be done to check the progress of the affection, to mitigate its distressing symptoms, and to prolong the life of the sufferer from it. Indeed, I shall be able to refer to cases in which undoubtedly consolidation of the aneurismal sac has taken place to a very considerable extent. Let me call your attention to the three cases which you have seen.

The first is that of J. S—— aged thirty-five, who was admitted on the 10th of July last. He was a sailmaker on board ship, and had suffered from dyspnœa and cough for seven or eight months. He was a spare man, and had a very anxious expression—an expression at once indicative of some serious organic lesion. We found, on examination, a distinct pulsation under the left clavicle; in fact, the whole of the upper part of the chest on that side was pushed forward at each beat of the heart. We found dulness on percussion over the upper part of the sternum, and extending on either side. Moreover, a loud systolic bruit was heard over the seat of pulsation. The pulse in the left arm was very small, scarcely perceptible, and that in the right arm was by no means full. The patient complained of pain in the shoulder; he was somewhat hoarse, but there was no dysphagia. The diagnosis in this case was attended with no difficulty; the disease had so far developed itself that no doubt could be entertained of its nature. It was not a very promising case for treatment, although the patient proved to be exceedingly tractable, and carried out, I believe, the rules laid down for him.

Now as to the treatment. This consisted in a spare diet, rest in the horizontal position, the exhibition of the iodide of potassium, and the application of ice to the tumour from time to time. The iodide was given at first in ten-grain doses three times a day; but subsequently—viz., from the 28th of July—in twenty-grain doses. On Aug. 18th a tracing (which I show you) of the pulse in the right wrist was taken by the sphygmograph; but no tracing could be obtained on the left side, for the pulse was almost obliterated. At that date the pulsation in the tumour was somewhat diminished, and the man complained less of pain. His health was good, and his appetite fair. We noticed, however, that he had a loud, barking cough, to which I called your attention as being probably due to pressure on the trachea or one of the bronchi. He also had more dysphagia, a further indication of pressure. By the end of September it was evident that consolidation was taking place in the sac; the pulsation was less distinct but somewhat more diffused. At this time there was neither dyspnœa nor dysphagia. On Oct. 18th there was slight hæmoptysis—always an unpleasant symptom in thoracic aneurism, as it may indicate some ulceration of the tumour into the air-passages. The hæmorrhage, however, soon ceased, and the patient progressed fairly well, so much so, indeed, that on the 25th of November I allowed him to get up a little daily. The improvement, however, did not continue, and I enjoined rest again. About the middle of December he had a severe attack of dyspnœa, with a sensation of choking, whilst he was taking a meal, and on examination we found dulness and a systolic bruit between the right scapula and the spine.

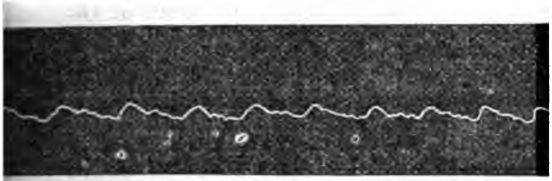


The dyspnœa and dysphagia increased from this date; in fact, it became evident that the aneurismal tumour was pressing on the air-passages and the œsophagus, and there appeared to be no prospect of affording the patient any relief. On Dec. 23rd I found him suffering greatly, and he expressed a wish to be removed to his home. I warned him and his friends of the danger of removal, but they took him away during the afternoon, and we heard that he died the same evening.

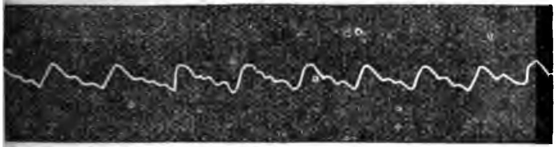
Now let me call your attention to the case of R. H——, in No. 10 ward. This man, a tinsmith by trade, thirty-four years of age, was admitted on the 5th of September last. He had, he said, been quite well until about four months before admission, when he had pain under the right clavicle, which subsequently extended down the arm. He first noticed a beating on the right side some three or four weeks before he came here.

We found, on examination, a pulsating tumour on the right side of the chest, with fulness under the clavicle. The pulsation was most distinct in the second intercostal space. There was a soft systolic bruit over the tumour. There was no dyspnœa, unless the patient lay on the right side; no cough, no dysphagia, and no hoarseness. The respiration was not so good on the right side as on the left, indicating some pressure on the right lung; the pulse was equal in both arms.

Here, again, there was no difficulty as to the diagnosis. The man was ordered a spare diet, twenty grains of iodide of potassium thrice daily, and rest in the recumbent posture. On the 4th of October he complained much of the beating in the tumour, and I stopped the iodide and applied ice to the chest. From this he obtained relief, and on the 13th the iodide was resumed. He still, however, complained of a pain in the side and shoulder, and we used subcutaneous injections of morphia, and on one occasion five ounces of blood were taken by venesection. On Dec. 9th I allowed the patient to get up a little daily. At that time the tumour was harder, there was less pulsation, and no increase in the extent of dulness; thus we had evidence of consolidation. He went on well up to Dec. 26th, when he was much frightened by one of the patients who seriously disturbed the whole ward. The fright brought on excessive action of the heart, and a return of pain in the breast, which, in spite of rest and anodynes, internally and externally, has persisted more or less to the present time. Moreover, the pulsation is more distinct than it was. There can, I think, be little doubt that the increased cardiac action removed or disturbed a portion of the deposit which had taken place, and thus has materially retarded the progress towards recovery. Unfortunate as such an occurrence is, it nevertheless affords a most valuable lesson. The man is now improving again. He has derived benefit to his breast pain, first, from the exhibition of solution of arsenic in seven-minim doses—a treatment which I found useful in another case of a similar kind which I shall allude to presently; and, secondly, from subcutaneous injections of morphia. I show you the tracings of his pulse.

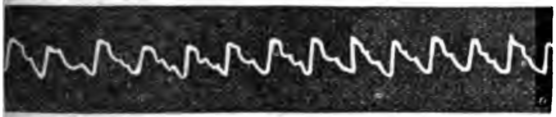


Right pulse.

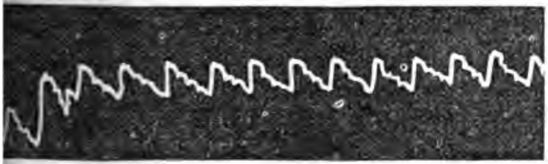


Left pulse.

The third case, which some of you have had an opportunity of seeing, is that of G. S——, admitted on the 1st of August; but as the man left us at the end of three weeks, and I cannot therefore say much of the results of our treatment, I will only remark that there was distinct evidence of a large pulsating tumour, and of pressure on the left bronchus and left innominate vein. The patient derived some benefit to his more urgent symptoms, and he left at his own request. I show you the tracings of his pulse taken on August 18th.



Right pulse.



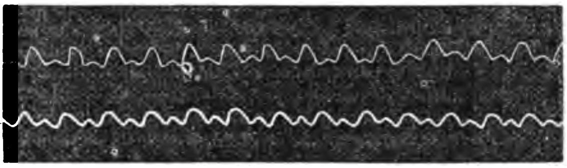
Left pulse.

The next case I shall refer to is one where decided improvement followed the use of the remedial measures which were adopted.

P. T——, thirty-seven years of age, a warehouse porter, accustomed to lift heavy weights, was admitted into the Northern Hospital under my care on the 9th July, 1870. He had suffered from pain in the left shoulder for twelve weeks, followed by a sensation of pins and needles in the left breast. He left off work about eight weeks before his admission, and a week later had an attack of dyspnœa; and then he noticed for the first time a pulsating tumour on the left side.

When admitted, he complained of pain in the left shoulder, shooting up the neck and sometimes down the arm, and of dyspnœa on exertion; otherwise he felt well. There was a round pulsating tumour to the left of the sternum in the second intercostal space. It was soft at its most prominent part, and felt as if near the surface. There was no bruit over it. The pulse was equal in both arms.

On the 11th July the patient was ordered fifteen grains of iodide of potassium three times a day, and on the 14th the dose was increased to twenty grains. Ice was ordered to be applied from time to time throughout each day. He was put on a spare diet; and rest in the horizontal posture was strictly enjoined. He continued this treatment without any change up to the 1st October, when the iodide was omitted. My note of that day is as follows:—"No pain; less beating; no prominence of tumour. There is a soft systolic bruit." The iodide was resumed on November 1st, and on the 17th he was allowed to get up a little daily. At this date consolidation had evidently taken place in the sac, and I judged it advisable, for the sake of the general health, to allow the patient to leave his bed; but on the 17th December I ordered perfect rest again, and on the 20th I stopped the iodide, which he did not take again during his stay in the hospital. His general health was good; he had not lost flesh; and there were no symptoms of any injurious effect having been produced by the prolonged course of the medicine. I show you tracings of the pulse taken on November 9th. About the middle of January the man complained of severe lancinating pain in the chest, for which I ordered seven minims of solution of arsenic three times a day. He experienced marked relief from this, and I stopped it at the end of a week. No further treatment was adopted, except that he was kept on a spare diet, and enjoined to keep moderately quiet. He was discharged from the hospital on the 23rd March, after more than eight months' stay, and was sent for three weeks to the Convalescent Institution at Southport. The following is the note made at this date:—"He is in good condition, and feels no pain. There is some pulsation, but it is distant and weak. There is no increase of dulness, no dyspnœa, no dysphagia. He says he feels the beating very little."



The only other case to which I shall specially call your attention is one which has been already published, and you can refer to it in my work on Diseases of the Chest. In brief it is as follows:—

J. L——, forty-five years of age, was admitted into the Northern Hospital on Dec. 14th, 1864, with an aneurismal tumour on the left side of the chest. There was no doubt as to the nature of the case. I submitted the patient to different modes of treatment. At first he was put on a moderate diet, with iodide of potassium in doses gradually increased to twenty grains three times a day. No perceptible effect was produced on the aneurism, although the treatment was continued for some time. After an interval of some weeks, in which nothing was done, the application of ice was tried.

The man was kept in bed, and a bag of ice was applied over the tumour during the day. This was continued for many days, but no noticeable change in the aneurism followed. Acetate of lead was then administered, but with no good result. On the 18th April, after having been in the hospital more than four months, absolute rest and a very restricted diet was ordered. For eleven weeks the man scarcely moved from the recumbent posture, and the result was that at the end of that time there was a remarkable change in the aneurismal tumour: we had in fact, distinct evidence of consolidation. The patient was allowed to get up in the twelfth week of treatment, but he was kept in the hospital nearly two months longer, that we might watch him. He then left us, and resumed his work as a ship-watcher. Three months later I saw him. He said he was quite well, and felt very little of the aneurism. It was smaller, harder, and less prominent than when he was discharged.

Thus far the case had progressed satisfactorily, and I think it highly probable that if the man had been in a position of life which would have enabled him to take sufficient care of himself he might have lived for years. He died, however, suddenly, from rupture of the aneurism into the right pleura, on the 5th March following his discharge, having, I believe, continued his work up to the day of his death.

I obtained a post-mortem examination. There was an aneurism connected with the arch of the aorta. The anterior part was filled with hard, firm laminæ, of fibrine, more than an inch in thickness. The posterior wall was thin, and had ruptured. A large quantity of blood was found in the right pleura.

The case is highly instructive, for it proves the possibility of producing deposit of a firm and stable character in these thoracic aneurisms. I might refer you to other cases which have been under my care, but I think it is unnecessary, and I will conclude with a few general remarks on treatment and diagnosis.

The great object to be kept in view in treatment is the consolidation of the aneurismal sac, and to effect this absolute rest is, in my opinion, the most important means we can employ. You know the effect of the recumbent posture in diminishing the frequency of the pulse and retarding the circulation, and thus giving increased facilities for the deposit of fibrine. I think, therefore, that rest affords the best chance of producing a cure—as far as cure is possible,—but it must be perfect and prolonged. The patient should not be allowed to rise from the recumbent posture for many weeks, and the bed on which he lies must be prepared with this view. No doubt this is very irksome, and it is difficult to get patients to submit to such treatment. A restricted diet—including a small quantity of meat daily—should be also insisted on, and very little fluid should be allowed. Thirst, if present, may be relieved by occasionally sucking lumps of ice.

With regard to the use of medicinal substances, given with the view of producing fibrinous deposits. Iodide of potassium has, of late years, been somewhat in favor with the profession. I have now used it in several cases; and although I cannot speak very confidently of its value, it has yet appeared to me to be of service. It should be given in large doses—twenty grains three times a day; and I have never seen it produce any injurious effects, although I have continued it for months. I think it is a remedy which you ought to exhibit, considering the favorable results which have, in many cases, followed its use. It may exercise a beneficial influence on the diseased bloodvessels. Acetate of lead has been recommended. I have tried it; but I cannot speak in its favor. I have seen good results from the application of ice to aneurismal tumours.

There are certain laryngeal and asthmatic symptoms which are apt to come on in connexion with aneurisms within the chest, about which I must say a few words. From pressure of a tumour on the pneumogastric or its recurrent branch, or on the air-passages, a reflex spasm is often produced. This may be either laryngeal or bronchial, and is sometimes so severe as to threaten suffocation. I have seen more than one case in which laryngeal spasm of this kind has been supposed to indicate disease of the larynx, attention being thus called away from the actual malady. I have seen a most violent attack resembling in every respect an asthmatic seizure produce by an aneurism pressing on the trachea. If therefore you are called to cases of this nature, examine the chest carefully; and should you fail to discover any signs of thoracic tumour, aneurismal or otherwise, do not pronounce an opinion before you have made an examination with the laryngoscope. If disease of the larynx is present, you will detect it; if not, you will be strengthened in the view that there is some internal tumour. These acute attacks of dyspnœa usually pass off; but they may often be cut short by the inhalation of chloroform. The laryngeal spasm is apt to assume in some cases the form of chronic laryngitis, and against a mistake in diagnosis you must be on your guard.

In connexion with these attacks of dyspnœa the question of performing tracheotomy may arise. It is well that you should have clear notions as to what tracheotomy can effect in such cases. If the dyspnœa were due to spasm of the glottis dependent on pressure of the pneumogastric or its branches, undoubtedly tracheotomy would give relief; but such spasm generally yields to other measures, or passes off without treatment. When, however, the dyspnœa results from pressure of the tumour on the trachea itself or a bronchus, it is obvious that the operation can do no good, except in so far as there may be laryngeal spasm. Opening the trachea might under these circumstances give some relief and prolong life for a short time; but the operation should only be performed after careful consideration, and after the nature of the hoped-for relief has been fully explained to the patient or his friends. In the case of the man J. S——, the dyspnœa was so evidently of the latter kind which I have referred to, that no thought of the operation was even entertained.

There are many interesting features connected with this disease which from want of time I must pass over. One I will refer to. You will occasionally meet with a difference in the size of the pupils—one will be more contracted than the other. This contraction indicates pressure or irritation of some kind of the sympathetic nerve, and may occur in any case of thoracic tumour. I have observed it as a permanent symptom, more especially in aneurisms at the base of the neck. In some cases I have seen the pupil on the aneurismal side only occasionally contracted. In the cases which I have brought under you notice to-day the symptom was absent.

ABSTRACT

OF

Clinical Lectures

ON

SURGERY.

Delivered at Guy's Hospital during the Session 1871–72.

By J. Cooper Forster, M.B. Lond., F.R.C.S.,

Surgeon and Lecturer on Surgery.

LECTURE II.

ON UMBILICAL HERNIA.

Gentlemen,—I take the opportunity of bringing under your notice to-day a case of umbilical hernia which has been for some months past under my care, and which during the last week has been completed by the death of the patient and the subsequent post-mortem examination. I do not propose to enter minutely into any abstruse details, but shall confine myself to a few of the practical points which the case before us would seem to enforce. Here is the report:—

Anne F——, aged fifty-four, a widow, living at Kensington, was admitted July 31st, 1871, into my female ward. She has had two children, the second now many years ago; and the catamenia ceased twelve years back. Six years ago she had an apoplectic fit, and lost the use of her limbs for twelve months. She is also subject to what she calls "inflammation of the stomach."

The history of the hernia commences ten years since, when she noticed a small swelling the size of a pea at the umbilicus, which she could take up and move about. She thinks it was produced by leaning out of window. She was not pregnant at the time. The tumour gradually increased, though she could always push it back again "into its place," and she was enabled to keep herself generally comfortable by a self-made truss worn tightly round the abdomen.

Five days ago the tumour came forward during the night as large as an egg, and she was now unable to return it. She was seen by a medical man, who told her she was ruptured, and ordered her to apply poultices and hot fomentations to the abdomen. This treatment failed to relieve her, and stercoraceous vomiting, it is said, commenced; the bowels were, however, opened several times by the aid of medicine. She has had very much abdominal pain since, and, getting worse rather than better, applied for admission to the hospital.

She was put to bed, a subcutaneous injection of morphia given, and reduction of the tumour attempted unsuccessfully by the dresser. The following is a note of her state at that time:—"She looks ill. There is a large and hard swelling about the navel, which is pushing itself forward through the abdominal wall; it is of the size of a fœtal skull, rounded and somewhat pendulous, with a broad neck, passing downwards and forwards, and lying in a wide semilunar fossa formed by the abdominal wall below the inferior margin of the umbilical ring. It is hard and brawny to the touch, and the skin over it is discolored, large and blue veins running just beneath the surface. It is freely movable with the abdominal wall. An impulse is felt in the tumour when the patient coughs, but it is not clear that this is anything more than part of a general impulse communicated to the whole abdominal wall. Pain is felt at the umbilicus of a sharp and burning character."

The symptoms were not urgent at her admission, and when I saw her some hours afterwards she had had no vomiting, had passed a good night, and the tumour had decreased in size, ice having been constantly applied to it. She was ordered half a grain of opium three times a day, and the ice was continued.

On August 5th, five days after admission, it is noted that she has not been sick since she came to the hospital, and the tumour has much diminished in size since the application of the ice. The bowels have acted to-day for the first time for five days. During the next week the bowels acted daily, the excreta being of a natural character. On the 17th, she got up for a short time, after which she vomited. She says that the tumour is painless so long as ice is kept to it. On the 29th she retched a good deal, but was not sick, and she had more pain in the tumour. Under these circumstances, Mr. Howse, who was taking charge of the wards during my absence, decided to attempt reduction of the hernia under chloroform, but no effect was produced upon the size of the swelling. She was sick several times after the chloroform, but the bowels acted naturally. During the next three weeks her condition fluctuated; at times she was better, so that she could take a little solid food, at others worse again, with complete loss of appetite. All this while the bowels continued very irregular, constipation lasting for twelve successive days at one period. On September 25th, ice being still applied to the hernia, the skin was noticed to be red and inflamed at its lower part on the right side. The skin gave way on the 30th, establishing a fistulous communication with the bowel, through which flatus and fæces passed. The fæcal matter was dark brown and fluid for the most part, though solid masses did occasionally come away.

From this date she went gradually down hill. Nausea and vomiting returned at times, and nearly all the intestinal excretions passed by way of the fistula. A fortnight or three weeks after the formation of the first opening, a second came on the left side, again at the most dependent part of the tumour, at what might be called its free border—i.e., where the anterior free surface joined the posterior, which rested in contact with the abdominal wall behind it. The last-formed aperture was the larger one, and most of the motions now came by it, but by *both* some fæcal matter passed; they also discharged a copious amount of pus.

During the last month of life no passage occurred by way of the rectum, till two days before death, which took place on November 18th. She had taken but very little nourishment for days previously, and had been latterly subject to strange illusions.

The post-mortem examination was made on the day of her death. It is unnecessary to read to you the whole report, so I shall confine myself to the account of the parts concerned. The abdominal cavity was opened by cutting out a large piece of the skin, with the sublying muscular wall surrounding the umbilicus. The protruded bowel was next divided a little on either side of its entrance into the sac, and the hernia was then taken out *en masse*. The abdominal walls were immensely laden with fat, as were also all the abdominal viscera which have fatty connexions. The incarcerated portion of bowel was found to be the transverse colon near to its left or splenic flexure, this part being dragged over to the median line. From the after-dissection it seemed that the omentum attached to this part had first entered the orifice of the sac, which, when cleared of its contents, measured about three-quarters of an inch in diameter; the omentum, becoming adherent to the farther wall of the cavity, had dragged in the *lower margin* of the bowel after it, thus producing a diverticulum from this border without detracting much from the normal calibre of the large intestine. The next step had probably been one of adhesion between the intestine and the sac; but inasmuch as a large mass of omentum was already adherent to the most dependent part, the bowel was prevented from forming a uniform pouch lining the hernial cavity, and it had, consequently, only united with the sides of the sac, that part which would have lined its bottom being pushed up towards the neck by the contained omentum. By this means two sacculi, or horns of bowel, were formed, separated from each other at the deeper part of the hernia, though having a channel continuous with each other and with the original colic canal at the neck of the sack. The two diverticula had subsequently ulcerated at their blind extremities, and openings had formed externally, as narrated in the report. Some recent inflammatory mischief had apparently taken place at the neck of the left diverticulum, so that at the time of the post-mortem examination this existed only as a blind pouch with one external opening. Nothing could be made to pass from it into the colon. The channel on the right side was much smaller than the pouch on the left, since the former would only admit an 8 gum-elastic catheter, while the latter would take the index-finger. The canal on the right side was also very tortuous and irregularly constricted; indeed, the passage was so obstructed that water could not be injected along it. The colon at the neck of the sac would admit the thumb easily. In addition to the bowel which was in the sac, and which really occupied but a very small part of the space, was a large mass of fat, apparently forming part of the mesentery. This had entered with the bowel, and had then become adherent. It had nearly obliterated the neck of the hernial bag, and had considerably narrowed the opening from the colon proper into its protruded portion. Till the fat had been dislodged, the orifice would only admit the tip of the little finger. The whole length of the intestine, both small and large, was much contracted; the part beyond the hernia, however, was less so than the earlier part of the intestine. The part above the hernia was quite empty; that below it contained some scybalous masses. The general peritoneal surface was healthy.

And now let us review one or two features of the case. This patient was *said* to have an umbilical hernia when admitted, the diagnosis had already been made, and it had now only to be confirmed. But let me ask, in the first place, suppose that any of you had been called to see the case in private practice, with no one at hand to indicate the direction your inquiries should take, with no experienced mind to tell you what was the nature of the disease, by what symptoms or appearances would you arrive at a conclusion as to its nature? It is all very well for you, as students in a hospital, to be able to learn at once that a good case of this or that disease has been brought in; but the convenience has its drawbacks, and one of these is that many of you go to the bedside with your minds prepared to see a case—say of umbilical hernia,—and you see it, and go no further. Now I would have you always to remember that, no matter what the disease, and whether or not you have ascertained beforehand its nature, you must invariably put to yourselves the question how your seniors have diagnosed it as such, and what are the points in any given case that will lead you to recognise it at once when it is met with again.

In the case under consideration the history alone is sufficient for the purpose. Notice that the patient had a moveable tumour, which she could reduce at pleasure, and that by wearing a truss she kept herself quite comfortable. These three points would lead one to say confidently that this woman was suffering from a hernia; the seat of the protrusion you would localise by looking at the body.

In the second place, observe that our patient is of the female sex. This is generally the case with umbilical hernia. Woman are much more liable to it than men, and women that have borne children more so than single or sterile women. Think of these facts, and you will see in them an illustration of a law to which I called attention at our last meeting, when I told you how that the weaker parts of our frame feel most the effects of additional strain—that the spoiled parts bear the brunt of disease. Now the weak part of any woman's abdomen is the umbilicus: in females who have been pregnant the abdominal wall has been much stretched and the recti separated; therefore, other things being equal, the median line of the abdomen will be weaker in them than in those where the parts have not been so stretched—i.e., than in childless women, or *a fortiori* than in men, in whom there is the pressure of strong and well-developed muscular walls to keep back the abdominal viscera. This is a case in point: our patient was exceedingly stout and had been pregnant; her abdominal walls were consequently weak and flabby, and the least supported spot of this confessedly insecure wall gave way, and allowed the descent of a hernial protrusion.

Five days before admission an important change is noticed in the monotonous ten-year history of the patient. The tumour suddenly increased in size during the night, and she could no longer put it back again. Why it should have thus come down at this particular time is doubtful; possibly it was owing to some fit of coughing, or other slight strain, while the patient was without her truss. It is, however, my own opinion on this point that the descent of a hernia is by no means so subject to accidental strains for its cause as to a constitutional state, in which diminished nerve force and weak muscular tension are the prominent characteristics. In corroboration of this the report says, very significantly, as I think, that "she has wasted lately."

Well, the patient was admitted with a large tumour, which is described as being the size of a fœtal skull, and rounded in form. Now the shape of umbilical herniæ is very irregular; they may often be seen much like what is represented in the drawing passed round, and in which the tumour is seen to consist of two parts, a large mass above and a smaller one below; they are sometimes, again, almost lobulated—indeed, they may be likened very much to fatty tumours from their appearance alone, and this likeness is carried still further by the manipulative symptoms, seeing that to all intents and purposes most of these cases are really fatty tumours, the greater part of them being filled with omentum. Thus the sensation given to the hand is that of a mass of fat between the fingers. The contents of these herniæ, I may say, are very various, as far as intestine is concerned: some contain much of the bowel; others only a very small piece. In one case, under my colleague Mr. Hilton, the cæcum was found in the sac; and I have seen the stomach, a great part of the large and half the small intestine, in the protrusion—indeed, so enormous do they at times become that nearly all the intestinal canal may be said to be outside the abdomen, in the hernial pouch.

The next point that you should notice is one that has an important practical bearing when an operation is necessary. It is well marked in this preparation which I send round, and in which the coats of the hernia are displayed. Observe that the skin is remarkably thin. This is the normal condition, and is not an attenuated one, the result of disease. So delicate is it at times that one is tempted to think the intestine can be seen beneath. The skin having been reflected, you come down upon the layer of fat, generally very thin and compressed; but this may vary a little. Beneath this is the transversalis fascia—the internal spermatic fascia of the inguinal region; and under this again is the peritoneum or sac. All these are very thin then, and there are no well-marked layers as in other parts. No this coat and that to be cut through; no nice dissection to be made. The whole thickness, I say, of umbilical herniæ depending upon the fat between the transversalis fascia and the skin, which may vary a little; the other coats never do. In operating, therefore, you are down upon the sac and its contents immediately; and much care is required, more especially if you wish to divide any constriction without opening the peritoneal cavity.

Continuing our review of the history, we find that vomiting set in soon after the descent of the tumour, five days before admission; and the vomit is said to have been stercoraceous. However, no severe symptom of strangulation occurred after she was admitted; indeed, for some time the report distinctly states she was not sick. I commend this to your notice, because the same rule holds good in umbilical as in all other herniæ—that vomiting, persistent vomiting, is the imperative sign of urgent strangulation. Here, then, was a patient with no vomiting (this had ceased for some hours when I saw her); a tumour with some impulse on coughing; while manipulation only gave omental signs. What, then, was the probable state of things inside the sac? Why, surely that, though the bowel might have been strangulated at first, it was so no longer; that it was only incarcerated, or, in other words, *irreducible*. Under these circumstances, I should not have been justified in submitting this woman to an operation. I say this now for your rule of guidance; and at the same time I tell you that, with the completion of the case before us as we have it to-day, it might perhaps have been better for our patient if we had operated. This fact, however, does not alter the rule that *without vomiting the surgeon must not operate*. In this case ice was applied to the tumour, and an injection of morphia administered subcutaneously to relieve pain. We attempted to reduce the tumour, but this failed. As she was not sick, I did not thing it necessary to give chloroform and try reduction by that means, because, if there is a long history of rupture, one may take for granted that the principal part of the swelling is due to adherent fat, which cannot by any means be returned; while the sudden increase may be owing to the nipping of a small piece of fresh bowel, which in most cases is very easily liberated. The probability is that some small coil was temporarily strangulated here, and that my dresser, in his attempt to reduce the mass, pushed this back into the abdominal cavity. The neck of the sac, moreover, is usually very large, and only very exceptionally causes obstinate strangulation.

A word here as to attempting reduction, not only of umbilical but of all herniæ. A golden rule for practice I believe to be this: never attempt to reduce a hernia except with the patient under the influence of chloroform. I believe more harm is done by violent taxis when the patient is straining than by letting the bowel alone. Strong language this, but I repeat that so much damage is done to the intestine by those who know nothing of the injury they inflict as to counterbalance much of the good which is unquestionably done when cases are operated upon early. You had much better give an anæsthetic, and so do away with all chance of muscular effort, than push and squeeze, and push again, for one, three, ay, four hours, or several times during the evening—a history of magnificent effort which has not unfrequently been narrated to me. Make, then, one decided attempt under chloroform; and, if it fail, operate. You cannot hear this too often, because forcible taxis and delay in operating are the great causes of death of all cases in our hospital. First comes the private medical man—he tries perhaps several times; then the dresser; then the house-surgeon; and then the advanced student who may happen to be looking on; and, lastly, the surgeon is sent for, and he very wisely at once operates. I cannot put the case more forcibly than this—that, if I myself had a rupture, no one should try to reduce it except the operator, with the proviso that if he failed after a very moderate attempt he should at once proceed with the knife. I look upon the operation itself as one of the easiest, the safest, and simplest of the severe procedures of surgery. Cases of umbilical hernia in which operations have been performed are most fatal for this very reason, that so much taxis and delay takes place; and aptly illustrating the converse of this, two cases came under my care, not so very long ago, in which the medical gentleman in attendance showed an amount of intelligence which was quite delightful. I saw both these cases within four hours—one within two—of the descent of the hernia, and in both I operated within an hour of seeing them, and they both got well—a very unusual thing in cases of umbilical rupture. To return to our patient once more. The bowels acted soon after her entry, and continued so to do, though irregularly, thus proving that the manipulation had in her case returned the bowel.

The last point that I would call your attention to is the fact that, on the 30th of September, two months after her admission, a fæcal fistula formed—that is to say, an opening formed between the intestine and the skin. It probably occurs somewhat in this way: the bowel in the sac gets some fæcal accumulation in it, which starts inflammatory mischief; adhesion then takes place between sac and bowel, new material is formed, and ulceration then occurs. A slight extravasation of fæcal matter takes place into the new adhesion-tissue, and an abscess forms, a slough of the skin usually forming over the collection of matter, and the fistula is completed. You will notice that this sketch of the process in our patient rather differs from that which goes on in the more usual form of fæcal fistula, the sequel to hernia. The rule is that ulceration takes place round the neck of the sac from the pressure of constriction, just as it would do if a piece of string were tied round one of your fingers. A fistula is the most favorable result that can happen in a case of strangulated hernia if left to nature; it is, in fact, the natural mode of cure, though I think we must say that the cure is only better than the alternative. I have seen more than one instance of such a result in umbilical hernia in obstinate old women. One old lady told me once that she would sooner die than be operated upon; well, she did not die, but she had one of these horrid apertures discharging her motions for the rest of her life. In our case most of the excreta came by the fistula. It was brown and very fetid, pointing to the seat of constriction as low down in the large intestine, and if you will examine the specimen on the table, you will see that the hernia was at the left end of the transverse colon. I may mention that during the last month of life no passage of fæcal matter took place by the rectum; this is unusual, the calibre of the bowel is generally wide enough—it was so in this case—to admit of the passage of much of the intestinal contents into the sigmoid flexure and rectum.

Clinical Lectures

ON

INFANTILE PARALYSIS AND ITS RESULTING DEFORMITIES.

By **Richard Barwell**, F.R.C.S.,

Surgeon to Charing-Cross Hospital.

LECTURE I.

Gentlemen,—Before showing to you one or two cases and reading to you certain short extracts from my note-book, it seems desirable that I should define the class of malady which I intend to bring before you. Infantile, like adult life, is liable to certain diseases of the brain and spinal cord which result in loss of power in one or more limbs. But, besides these, the infant is subject to a form of paralysis unattended by any symptom of illness—certainly of severe illness; and this, the class of case I propose for your study, being peculiar to the infant, alone deserves* the name of "infantile paralysis." Other paralytic maladies, whether occurring in senility, in the prime, or in infancy, should be classed in the categories of meningitic, myelitic, lead-poisoning, traumatic, or other form of disease. But to enable us to separate one thing from a heap of others, we must have means of recognising that single object whenever we may see it. Hence there devolves on me the ungrateful task of making a definition out of mere negatives; for the typical, and, in my experience, the common, form of the disease is, that the child should be put to bed perfectly well at night, and next morning be taken up with loss of power over a limb or part of a limb, yet otherwise quite well or only very slightly ailing.

For example: This child, Eva H——, aged thirteen (March, 1871), looks extremely healthy, except that she is lame from paralysis of the left leg. Her mother, an intelligent, clear-headed woman, says: "When she was about eighteen months old I took her out of bed one morning and put her on the floor to walk. She tumbled down. I thought she was stupid, and put her on her feet again. She fell at once, and I then found her leg was paralysed. She was not at all ill or fretful, either the night before or that morning; but I think that side of her body was perhaps a little tender."

Agnes P——, whom you have often seen, was, when she first came, nine months old, a plump and as healthy a baby as ever I saw, except loss of power in the deltoid of the right arm. The mother says: "Two months ago I took the baby out of bed, and it did not move the arm. I showed it to the doctor, and he said the child must have been hurt. I do believe the girl I had then lifted it up by the arm; but the baby did not cry either the night before or that morning more than usual. She always cried when she was being undressed and washed. I observed no difference in that, and she seemed quite well."

One more history from my note-book:—

Louisa R——, brought to me in April, 1870, when seven years and a half old, with foot deformity resulting from paralysis. Her mother, an active-minded and very acute woman, says that one very hot day, when the child was about eight months old, she was put asleep under a tree on the lawn, and on a damp cushion. The next day she was taken up with the right leg paralysed. The child had not then any sign of illness whatever, and always has been, with that exception, a strong, healthy girl.

Of such cases and histories I could give you an almost unlimited number. Some attacks may have been preceded for an hour or two, perhaps even for a day or two, by some slight disturbance, fretfulness, a hot skin or mouth, even a slight convulsion. On the morning of the discovery there may be some evidence of pain in the affected limb, some feverish disturbance, and the disease itself may affect a larger or smaller portion of the body. But the more the history and concomitants depart from the type which I have given above, the more any disturbance approaches absolute illness—especially if convulsions have been frequent and severe,—the more the stricken regions resemble the relationship of hemiplegic or paraplegic parts, the more likely is the case to be an example of myelitis or meningitis, and the less likely to belong to the pure infantile paralysis, which, as you have seen, does not exhibit a single symptom of central nervous disease.

I feel bound to insist strongly on this distinction, because medical opinion has of late years more and more agglomerated round the idea, fallacious in origin and in result disastrous, that all these cases are dependent on cerebral or spinal disease. Heine, Fliess, Laborde, Radcliffe, and many others, have upheld this notion; and I must shortly point out what essential differences separate this disease from spinal affections. But first as to pathological appearances. Only seven cases of anatomical investigations have been published. In four of these nothing was detected; in one—that of Dr. Fliess (*Journal für Kinderkrankheiten*)—was found a congestion which, emanating from the gums of the teething child, was continued on to the membranes of the cord. It need hardly be pointed out that the preconceived ideas which aided this description are more easily comprehensible than the anatomy of the congestion. To the two other post-mortem examinations I must unfortunately also take exception, for the two cases which form the groundwork of Dr. Laborde's treatise, "La Paralysie (dite Essentielle) de l'Enfance," are cases respectively of myelitis and meningo-myelitis, with their characteristic histories and symptoms, and do not belong to the disease under consideration. The absence of illness and of danger to life renders it improbable that morbid anatomy will ever furnish a clue to the causality of this disease; we must, at all events for the present, be content with studies from life.

The children we have now to do with are not ill, have no severe convulsion, insensibility, nor pain; therefore no disease marked by severe symptoms, such as myelitis or meningitis, can be present. But a congestion of the cord does not give rise to any attack of such suddenness and severity; it may also be supposed to vanish at death after having produced paralysis in life. There is, too, a certain similitude between congestive and infantile paralysis—such as immunity of the respiratory muscles, bladder, and rectum. Nevertheless, when the condition of a patient with congestion of the spinal cord is compared with a case of infantile paralysis, the observer will not fail to mark a vast difference between the plump limber limbs of the one, and the cold, shrivelled, flaccid muscles of the other. A further investigation into the condition, more especially by various modes of electricity, will render prominent other and more radical differences. A rapid view may best be given by arranging these into opposite columns.

SPINAL CONGESTION.*	INFANTILE PARALYSIS.
Paralysis incomplete—always at first, and generally throughout the case; it tends, however, to deepen.	Nothing can be more utter and entire than the loss of power at the very commencement; it tends, however, to lighten.
Generally paraplegic in form at the end, if not at the beginning. Usually one leg is attacked first, then the other. The malady becomes more extensive as it goes on.	Paraplegic form is not usual at the commencement, still less afterwards. When the disease has attacked one part, it never subsequently affects another. It is most extensive at its commencement.
Tingling in fingers and toes.	I have never been able to detect the slightest sign of pain or discomfort. In a few cases there is a little and a doubtful increase of sensibility to touch.
Shortness of breath not infrequent.	No alteration ever observed in breathing.
Fluctuations towards better and worse occur in all cases; sometimes these are rather frequent.	No change, backward or forward, ever occurs; if the patient gain a step, he does not lose it again. (Effects produced by treatment are not referred to.)
There is a decided tendency to relapse, even after the patient appears to have got well.	No tendency to relapse. I have never seen or heard of a limb being paralysed a second time, nor of another limb being the subject of a second attack.
Reflex irritability is, according to some authorities, increased—according to others, decreased.	Reflex irritability is so utterly annihilated as to leave no room for variety of opinion.
No wasting of paralysed muscles.	Extremely rapid wasting of paralysed muscles, setting in very quickly after the attack.
No marked impairment in affected muscle of electro-contractility.	Remarkable impairment of electro-contractility—indeed, total loss of response to the induced current.

These differences are very marked, and so important that the first six alone appear to separate nosologically the one disease from the other; but it is to the last three that I would especially direct attention. They are physiologically connected in infantile paralysis. The induced current ceases entirely to call forth any contractions of the paralysed muscles in from twelve to seventy-two hours. At the same time there is developed an abnormal sensibility to the voltaic current. Now this peculiar behavior never occurs in any case of central paralysis, but is exactly what happens when a nerve supplying a muscle has been divided; moreover, both muscles—i.e., the one whose nerve has been cut and the one paralysed by this malady—lose all excito-motor contractility. But when a nerve of a warm-blooded animal is cut, the muscle, at first flaccid, becomes very hard and strongly contracted as long as the wound remains unhealed. When this is well the muscle relaxes. The nearer the spine be the wound the harder and more contracted will be the muscle, and the less rapid is the loss of sensibility to the induced current. If we divide the nerve close to its entrance into the belly of the muscle we find hardly any rigidity, quick loss of galvano-contractility, and rapid wasting. Therefore if we could go a little further and destroy the nerve absolutely at its distribution among the muscular fibres we should, in all probability, carry the above three characteristics of infantile paralysis—namely, flaccidity, irresponse to faradaism, and wasting—as far as they are developed by the disease itself.

It appears impossible to escape the conclusion that infantile paralysis is a malady purely peripheral, affecting the ultimate nervous fibrillæ of distribution in the muscular elements. I am anxious, gentlemen, that this conclusion be not pushed too far. I am not supposing or imagining any degenerative or structural change in the nerve-fibres as the cause of this malady. The attack is too rapid and too quickly succeeds exposures to warrant such an idea. The essence of the disease probably lies in some subtle derangement of relationship between ultimate nerve and ultimate muscle-fibre; perhaps from some inflammatory, but more probably from a chemical or nutrient change. In after years any amount of tissue degeneration may occur.

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* The symptoms of spinal paralysis are taken from Reynolds's System of Medicine, vol. ii., p. 663 *et seq.*

Allow me now to refer to a former remark, and to explain its intention. A few paragraphs ago I said that the prevalent notion concerning the central origin of this malady was in its results disastrous, and in saying this I refer especially to the effects produced by loss of time. In the first few hours of the attack the affected muscles are still sensitive to the faradaic or interrupted current. This sensibility rapidly declines, and I am not aware that any use of this force would prevent its extinction. After a time, which varies in different cases, there is established a sensibility to the voltaic or battery current, and it is upon this that we must rely as our chief aid in the treatment of the disease; and a judicious practice, by keeping alive this contractility, preserves the muscle from extreme wasting and degeneration. To wait, with temporising treatment or with none at all, until this sensibility is in its turn extinct, is the disastrous result of the belief I have combated. Delay in applying the treatment pays a heavy percentage in subsequent difficulty and duration. Very few cases get well of themselves, and tissue change, as evidenced by the wasting, begins almost immediately. If, then, a child within twenty-four hours of its first attack do not already show marked signs of recovery, not merely in one or two, but in every muscle, the state of the case should be carefully and rigorously tested by faradisation, and again in another twenty-four or forty-eight hours. Any muscle which, at the end of thirty-six or seventy hours, has ceased to respond, or responds more feebly than at the last examination, will not of itself get well, and to delay treatment is in all probability to condemn the patient to a crippled existence. No disease is so obstinate when old; but while still new, few are more amenable to treatment.

Clinical Lectures

ON THE

SIGNIFICANCE OF UTERINE AND VAGINAL DISCHARGES.

By Robert Barnes, M.D. Lond.,

Obstetric Physician to St Thomas's Hospital, and Examiner in Midwifery and the Diseases of Women to the University of London and the Royal College of Surgeons.

Discharges of Air.—Air may get into the vagina, if not into the uterus, in the non-pregnant state. In the normal condition the walls of the vagina are maintained in perfect contact, and no air, or probably very little, is admitted. But where the parts are greatly relaxed, the vulva open as when the perineum is torn, the lower part of the vagina is no doubt exposed to the contact of air. But the very condition of patency prevents the retention of the air to such a degree as to lead to its escape in perceptible volume. Air also penetrates where too large a pessary is worn, which keeps the vaginal walls apart. But under peculiar circumstances air enters in large quantity, to be expelled with noise. Dr. George Harley details[*] a curious case in which he carried out decisive experiments to prove the correctness of the diagnosis. A pluripara frequently expelled air from the vagina with a loud noise. It was ascertained that no connexion existed between the rectum and vagina. Dr. Harley took a full-sized male catheter, to which was attached a long india-rubber tube with a stopcock at the other end. The catheter was introduced into the uterus, the end of the tube with the stopcock being placed in a tumblerful of water. No air escaped when the instrument was in this position; but, on placing the open end of the catheter in the vagina, an instantaneous discharge of gas took place. The water was found to be sucked up through the tube into the vagina. It was found that the vagina sucked in and expelled the air by spasmodic action. It was further observed that the abdominal muscles assisted in the suction process. The uterus was completely retroverted. This displacement being remedied, and the patient's health improved by tonics, a cure ensued.

If you observed the vagina when the duck-bill speculum is applied, you will see movements of rise and fall under the influence of the rise and fall of the diaphragm. Dr. Adolph Rasch has investigated[†] the phenomena with great care. He says, if a multipara whose genitals are normal be placed on her back, with the thighs flexed and abducted, and the vaginal orifice closed, movements caused by respiration are seen, but no air enters. In the lateral position the same thing is observed even if the vagina is lax, and even when the perineum is ruptured. When the patient is placed in the prone position, or on all-fours, if the vulva be open, air will enter, because the intestines falling downwards by gravity causes a vacuum. Under this condition violent exertion may expel air, giving rise to vaginal flatus. If the abdomen be supported by the hands or by a bandage, no air enters.

There are several interesting applications of this knowledge. It teaches that the best position after labour, if not during labour also is the dorsal; that the same position is also best in the case of pelvic abscess or hæmatocele discharging into the vagina; and that we must carefully consider this respiratory rise and fall of the vagina when selecting pessaries. It is by turning to account this action that we derive the greatest advantage from the spoon or Sim's speculum. The blade drawing the perineum well back, whilst the semi-prone position of the patients favors the falling forwards of the abdominal viscera, air fills the vagina, counteracts the effect of inspiration, and thus enables us to get a good view of the os uteri. The same position also greatly aids our efforts at reducing inversion of the uterus, and in replacing a prolapsed umbilical cord. In most operations, however, upon the uterus and vagina, where it is of importonce to bring the uterus as low down near the vulva as possible, the dorsal position, by bringing the force of gravity to counteract the respiratory rise of the uterus, which can further be greatly aided by direct pressure by an assistant's hand above the symphysis pubis, is the best.

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[*] Obstetrical Transactions, 1863.

[†] Ibid., 1870.

If we were to follow out in detail all the indications supplied by examination of the discharges, we should be led through almost the entire range of uterine and vaginal pathology. This, in fact, is simply the converse proposition to one already stated—namely, that almost every uterine or vaginal disease is attended by discharge. But to adopt the discharges as our point of departure, from which to proceed to study and classify the diseases of which they are consequences, would be a most intricate course, and not the most orderly or profitable. I will therefore not pursue this method further than to give one instance of how we may apply it to clinical analysis. We will at our next meeting take hæmorrhage, and sketch what its purport is.

The Watery Discharges.—When these occur, you must first of all determine the presence or absence of pregnancy. It is no uncommon thing that discharges of water, more or less profuse, take place in pregnant women. This is the hydrorrhœa gravidarum. Gushes of water, quite clear, may occur at almost any time during pregnancy; but they are more frequent in the latter months, and especially in the last month. Happening at this time, they are commonly taken as an indication of commencing labour, and many are the false alarms which patients and doctors have to suffer from this cause. "The waters have broke," says the nurse. You go, as in duty bound, and find probably the os uteri closed, nothing resembling active labour-pains. What are you to do? If you wait for labour, you may wait for a week, or two or three weeks. If, on examination by ballottement, you find the child still floats in the uterus, the os uteri not open, and no active pains, you may go home and wait in peace for another summons.

What is the source and nature of this hydrorrhœa gravidarum? Several theories have been expounded. The character of the fluid differs in some respects from that of liquor amnii. It is odorless, and resembles blood-serum or the serous fluid effused in the peritoneal sac. Ruysch and Rœderer thought it came from ruptures of lymphatic vessels, or of hydatids of the uterus; Böhmer thought it escaped from a second abortive ovum; Delamotte and Cruveilhier that it came from a cyst near the ovum; Deleurye, Puzos, Naegele, and Dubois that it came from the inner surface of the uterus, being secreted externally to the ovum. Dubois says it is the result of loosening of the membranes from the uterus when the vessels pour out serum. Hegar says the source is the uterine glands of the decidua. Thus, he describes* the glands of the mucous membrane as being found in the decidua at the sixth month of gestation, and argues that their sudden disappearance in the subsequent months is improbable. In a case of hydrorrhœa he found in the decidua vera, at the beginning of the eighth month, an anormously developed glandular body. At the bottom of this morbid growth was a general hypertrophic condition of the decidua and its glands. These gave out the excessive secretions. In a case related by Dr. Graef,* repeated discharges took place, and the fœtus was expelled at the end of six months. The membranes were very delicate, and openings were found in them. In this case, it is probable that the fluid was true liquor amnii. In another case the patient suffered, during the last three months, from repeated watery discharges; the uterus rising and falling with the gathering and escape of the fluid. The membranes were found without rent. Graef regarded this as a case of catarrhal hydrorrhœa.

I believe there are various sources. In some cases the fluid is liquor amnii. This may come either from rupture of the membranes; from rapid transudation under pressure from rapid formation and accumulation of liquor amnii in the amnion; or from the bursting of a cyst formed between the amnion and chorion, or between two layers of chorion, the proper amniotic sac remaining intact. In the majority of cases, however, the fluid is not amniotic; for when once the amniotic sac has fairly ruptured, labour is not far off. It is, then, the result of a rapid secretion from the uterine glands or from the cervical cavity. In the early months, whilst there is still a free space between the decidua vera and the decidua reflex, there is a large area of developed glandular surface.

I have observed a *puerperal form of hydrorrhœa.* Thus watery discharges may continue for a month or longer beyond the proper lochial flow. Generally in these cases the water is dirty, discolored, occasionally stained with blood, and offensive. The most common cause I have found to be the retention of a portion of placenta or of clots in the uterus; but a polypus may produce like results. The watery discharges alternate, but not always, with discharges of blood. The fluid may, under certain conditions, collect in considerable quantity in the uterus, so that the organ becomes greatly distended before the collection is expelled in a gush.

Sometimes watery fluid is mingled with air, constituting *physo-hydrometra.* This is also a puerperal or post-puerperal condition, and is commonly, the result of retention of some portion of placenta or membranes, and the admission of air into the uterine cavity. If an examination is made when the uterus is relaxed after labour, especially if the hand be introduced into the uterus, the vaginal walls are separated from their usual contact, and a channel is formed along which air easily enters. Merely turning on the side, or a little more prone, will often, by favoring the fall of the uterus forwards, produce a vacuum into which air will rush. This is one reason amongst others why I am unable to approve of the abolition of the old-fashioned binder, which some people would condemn, for no better reason that I can see than because it is old-fashioned. After labour, especially in pluriparæ, the abdominal walls are so relaxed that they can give no support to the uterus. The binder does temporary duty for the inert abdominal walls. The history of physo-hydrometra is, I believe, this: a portion of placenta, membranes, or clots, remain in the cavity of the uterus after labour; some air gets in as I have described; decomposition ensues, and the gases of putrefaction are added the air from without, whilst the os uteri is occluded by the placenta or blood-mass falling over it. When this occurs, there is invariably hectic or irritative fever; peritonitis and septicæmia commonly attend; great abdominal pain; the enlarged, distended uterus can be mapped out rising as high as, or higher than the umbilicus; and resonance is made out on percussion.

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* Monatsschrift fur Geburtskunde, 1863.

* Jenaische Zeitschrift, 1865.

One condition, the result of impregnation, often leads to copious and repeated discharges of watery fluid: the *hydatidiform degeneration of the chorion*. In this case the ordinary signs of pregnancy may not be present, and even the patient herself may not think she is pregnant. There is, however, always evidence of enlargement of the uterus, and generally great pelvic distress. The water escapes in gushes at uncertain times; it is often tinged with blood, resembling red-currant water; it has not the offensive odor belonging to the watery discharges of cancer; sometimes, but not often until late in the progress of the case, cysts will be found swimming in the water; it is generally expelled with painful uterine contractions. In a case we recently had in Adelaide ward, the nature of the disease was not at first suspected. There was some abdominal enlargement, retention of urine requiring the catheter, and most distressing pelvic pain with irritative fever. The os uteri was found high up above the symphysis pubis, whilst behind it the pelvic cavity was filled with a large, rounded, firm mass taken to be either the retroverted gravid womb or a fibroid tumour. One day a large quantity of water, blood, and a mass of chorion-cysts were expelled. We had, in fact, the condition of retroverted gravid womb complicated with hydatidiform or cystic degeneration of the chorion.

Apart from pregnancy, watery discharges are often of grave significance. During and after the climacteric period, the most frequent cause in some form of malignant disease, especially the so-called cauliflower excrescence of the uterus. In this case other symptoms will probably point to the seat and nature of the disease. The fluid discharged is seldom clear; it is generally turbid, dirty, often tinged with blood, resembling water in which flesh has macerated; it contains shreds or flocculi of solid matter, the proceeds of superficial erosion or necrosis of the surface of the diseased growth, and is almost always of a peculiar offensive odor. It often alternates with hæmorrhage. Local exploration will place the nature of the case beyond doubt. Another form of malignant disease giving rise to watery discharges is the "oozing excrescence of the labia."

But we must remember that similar discharges may take place from polypus or inversion of the uterus. Hence we have another example of the wisdom of not pronouncing a diagnosis until we have made an internal examination. Water may escape in large quantity from the rupture or perforation of an ovarian cyst into the vagina. In such a case, the rapid concurrent diminution of the abdominal tumour will lead to the right conclusion.

Original Papers.

CONTRIBUTIONS
ON THE
PHYSIOLOGY & THERAPEUTICS OF FOOD.

By F. W. Pavy, M.D., F.R.S.,

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No. VI.

NITROGENOUS MATTER IN RELATION TO FORCE-PRODUCTION (CONTINUED).

Professor Frankland[*] has shown that the results arrived at by Fick and Wislicenus afford in reality stronger evidence than they have contended for. Fick and Wislicenus were obliged to calculate the force-value of the nitrogenous matter, shown by the nitrogen in the urine to have been destroyed in the system, from the amount of force generated by the oxidation of its elements, because the actual determination for the compound itself had not been made. Professor Frankland, however, has since experimentally ascertained with the calorimeter the amount of energy or force that is evolved under the form of heat during the oxidation of a given quantity of nitrogenous matter as the oxidation occurs within the living system, where, it must be remembered, on account of the nitrogen carrying off the elements that are associated with it in urea, a portion of carbon and hydrogen escapes being consumed. Frankland's results give, as the actual amount of energy producible from the nitrogenous matter consumed in the bodies of the experimentalists, about half the quantity that they had reckoned in their calculation. Thus the results tell so much the more in favor of Fick and Wislicenus; and, taking all points into consideration, Frankland is of opinion that scarcely a fifth of the actual energy required for the accomplishment of the work performed in the mountain ascent could have been obtained from the amount of muscle (nitrogenous matter) that was consumed. Presuming the foregoing conclusions to be entitled to credence, the doctrine which ascribes muscular action to the oxidation of muscular tissue becomes utterly untenable.

Dr. Parkes has conducted, in a most careful manner, a series of investigations on the influence of rest and exercise under different diets upon the effete products of the system, and more particularly to test the accuracy of the results arrived at by Fick and Wislicenus. These investigations Dr. Parkes considered it desirable to make, because exception had been taken to the experiments of Professors Fick and Wislicenus on the ground "that no sufficient basis of comparison between periods of exercise and rest was given; that the periods were altogether too short; and that no attention was paid to the possible exit of nitrogen by the intestines."

The experiments were conduced upon perfectly healthy soldiers, whose regularity in diet and occupation, and whose habits of obedience, afford a special guarantee for the precision with which they will carry out the instructions imposed. The total nitrogen contained in the urine was determined as well as the urea, such information supplying evidence of a more reliable nature than that afforded by the determination of urea, as had only been done in the experiments of Fick and Wislicenus and others. In these latter experiments it might reasonably be suggested that nitrogen escaped, as it in reality to the same extent does, in other forms than that of urea.

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[*] On the Origin of Muscular Power: London Philosophical Magazine, vol. xxxii., 1866.

The experiments consisted of two series, extended in each case over several successive days. In the first series * a comparison is instituted of the products of excretion during rest and exercise under a non-nitrogenous diet. In the second, † the same comparison is made under a uniform diet containing an ordinary admixture of nitrogenous and non-nitrogenous food.

In drawing conclusions regarding the destruction of muscle by nitrogen eliminated, it is necessarily of the first importance that we see before us the whole of the voided nitrogen. Dr. Parkes is convinced, from his experiments, that no nitrogen escapes by the breath or perspiration, but that it is all to be found in the excreta from the kidneys and bowels. The evacuation of nitrogen by the bowel forms a comparatively small and varying amount, and, being derived from the undigested food and the unabsorbed digestive secretions, has no bearing in reference to the point before us. There remains only the urinary nitrogen to consider as a measure of the tissue-metamorphosis occurring in the system. With these introductory observations let us now see what light is thrown by the experiments of Dr. Parkes upon the matter under consideration. For the sake of simplicity, attention will be confined to the total urinary nitrogen voided, this giving, as already stated, more completely than the urea the information that is wanted.

The men forming the subjects of the first series of experiments are distinguished as S. and T. T., it is to be observed, was a much smaller man than S., weighing only 112 lb., against 150 lb., the weight of S. For six days the men were kept upon an ordinary mixed diet, and pursued their customary occupation. The urine was collected and examined during four out of the six days, and the following is the mean amount of the total nitrogen passed per diem:—

	Mean Urinary Nitrogen per diem.
Mixed diet with customary occupation:	S. 17·973 grammes. T. 13·409 ,,

During the following two days the diet was restricted to non-nitrogenous food, consisting of arrowroot, sugar, and butter. The sole nitrogen consumed—and this may be regarded as of no significance—was in the tea the men were allowed to drink, it being thought desirable not to deprive them of this beverage. Throughout the two days they remained as quiescent as possible, being allowed to get up, but not to leave the room.

	Mean Urinary Nitrogen per diem.
Non-nitrogenous diet, with rest:	S. 8·176 grammes. T. 7· ,,

The men were now put back for four days upon a mixed diet with customary occupation, just as at the beginning of the experiment.

	Mean Urinary Nitrogen per diem.
Mixed diet with customary occupation:	S. 12·988 grammes. T. 11·095 ,,

Next, they were restricted again for two days to the same non-nitrogenous food as before; but this time it was accompanied with active walking exercise. During the first day the distance walked was 23¼, and during the second 32¼ miles. The diet, it is stated, satisfied hunger, and there was no sinking or craving for other kinds of food. The results furnished were as follows:—

	Mean Urinary Nitrogen per diem.
Non-nitrogenous diet with active exercise:	S. 8·971 grammes. T. 8·034 ,,

To complete the experiment, four more days were passed under the mixed diet, accompanied by ordinary exercise. Rather more nitrogenous food was consumed during these four days than during the four days succeeding the two days' rest; the men feeling more hungry after the "work"-period than after the period of "rest." The mean for T. is for three days instead of four—one analysis having failed:—

	Mean Urinary Nitrogen per diem.
Mixed diet with customary occupation:	S. 13·361 grammes. T. 11·658 ,,

From this series of results we find there was no material variation in the amount of urinary nitrogen discharged during the two days when a distance of fifty-six miles and a half was walked, as compared with the two days spent in complete rest, on both occasions restriction to non-nitrogenous food being enjoined. Comparing both these periods, however, with those in which nitrogenous food was taken, we see a striking exemplification of the well-known fact that diet, on the other hand, exerts a marked influence over the amount of nitrogen eliminated with the urine. During each of the non-nitrogenous diet periods the quantity of nitrogen eliminated was very much less than during the others.

In Dr. Parkes's second series of experiments the amount of nitrogen eliminated was ascertained under the conditions of rest and exercise combined with a *mixed* diet. One of the two soldiers, S., was the subject of the former experiment. The other, B., was a fresh man, weighing 140 lb., and therefore nearer in size to S., who weighed 150 lb., than T. was, who weighed 112 lb. During the sixteen days over which the observations extended, each man took *precisely* the same allowance of food in the twenty-four hours—the food consisting of weighed quantities of meat, bread, potatoes, and other articles belonging to an ordinary mixed diet. For the first four days the men pursued their customary employment. The next two days were passed in rest. Then followed four days of ordinary employment; after this two days of active exercise, and finally four days again of ordinary employment. The amount of nitrogen eliminated by the kidneys during the several periods is shown in the subjoined table:—

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* Proceedings of the Royal Society, Jan. 1867, Vol. xv., No. 89.
† Ibid., June 1867, Vol. xvi., No. 94.

		Urinary Nitrogen per diem.
Ordinary employment (mean of four days)	S.	17·857 grammes
	B.	18·502 ,,
Rest (mean of two days)	S.	19·137 ,,
	B.	19·471 ,,
Ordinary employment (mean of four days)	S.	17·612 ,,
	B.	18·485 ,,
Active exercise: walking on level ground 24 miles the first day, and 35 the second (mean of two days)	S.	19·646 ,,
	B.	19·959 ,,
Ordinary employment (mean of four days)	S.	21·054 ,,
	B.	20·092 ,,

In these results it will be seen there is nothing to sanction the doctrine that the source of muscular power resides in the destruction of muscular tissue. In two persons subsisting on an unvarying daily diet, and subjected to varying conditions of muscular exertion, we find nearly the same quantity of nitrogen eliminated during two days' hard walking as during two days of complete rest. It is curious, and also, it must be owned, does not appear explicable, that during the periods of both rest and active exercise the daily amount of nitrogen eliminated was in excess of that eliminated during the first two periods of ordinary employment; the figures at the same time for the associated periods respectively agreeing very closely with each other. In the third period of ordinary employment, however—that is, after the two days of walking exercise,—the nitrogen voided was greater in quantity than at any other time; still, the excess, it is noticeable, was not of a particularly marked description.

Comparing in detail the nitrogen eliminated during the corresponding portions of the two-day periods—those of rest and active exercise,—Dr. Parkes observes, with respect to the results presented: "On the first day of exercise the nitrogen in each man fell below the corresponding day of rest by 1·626 and 1·181 grammes. In the next twelve hours, which were almost entirely occupied in exercise [this period extending from 8 A.M. to 8 P.M.], the diminution was still greater, being 2·498 in 1·225 grammes, which would be equivalent to 5 and 2½ grammes for twenty-four hours. In the last twelve hours [8 P.M. to 8 A.M.] of rest after work, the elimination increased greatly, so that 5·142 and 3·331 grms. more were excreted than in the corresponding rest-period." Seeking to reconcile his results in relation to muscular action, Dr. Parkes says: "It appears to me that we can only express the facts by saying that a muscle during action appropriates more nitrogen than it gives off, and during rest gives off more than it appropriates."

But must we, I would suggest, look only to the muscles as the source of the variation in the amount of nitrogen discharged in these experiments? The results, as a first point, conclusively showed that the nitrogen eliminated forms no measure of muscular work performed; and hence it may be inferred as a corollary that muscular work is not a result of muscle destruction. But, taking the variation in the voided nitrogen that was observable independently of that occasioned by diet, why, I ask, should we look exclusively to the muscles as the source?

Looking at the several daily amounts of nitrogen discharged, I remark the presence of instances in which considerable variation occurs within the respective periods themselves. Thus, during the first day of the first period, when the men were engaged in ordinary employment, B. discharged 20·417 grammes of nitrogen, and during the third day only 17·090—a difference approaching to three grammes and a half. Again, during the last period, which was also spent in ordinary employment—and it will be remembered the daily diet was the same throughout the experiment,—the urinary nitrogen voided by both men stood as follows:—

	S.	B.
1st day	21·25 grammes	20·25 grammes.
2nd ,,	19·942 ,,	19·273 ,,
3rd ,,	23·488 ,,	19·248 ,,
4th ,,	19·536 ,,	21·597 ,,

On the third day, it thus appears, S. discharged nearly four grammes of nitrogen in excess of that on the fourth day, and about three grammes and a half in excess of that on the second. No corresponding fluctuation was observable in the case of B. Here, then, are marked variations in the elimination of nitrogen without variation of muscular action.

Now we know that the nitrogen of the urine is derivable from the metamorphosis of the nitrogenous ingesta within the system. It is true the food taken was every day the same throughout the experiment under consideration, but it does not follow that the rate of metamorphosis was every day similarly identical. Doubtless, like other processes of life, it is influenced by various internal conditions. We know also, as the result of observation in the case of starvation, that notwithstanding an absence of ingoing nitrogen an elimination of this element still continues, and that the nitrogen eliminated is drawn from the nitrogenous principles of the body, belonging alike to the solids and fluids. There is a *general* waste or loss occurring, and the only difence noticeable is that the loss goes on with different degrees of rapidity in the different parts of the system. In the muscles it certainly occurs somewhat more rapidly than elsewhere, but this is all. With these considerations before us, it appears to me that we are taking an unjustifiably narrow view in looking only to the muscles to account for the variation in question in the voided nitrogen. Exercise cannot fail to influence the processes going on in the system generally, as well as in the muscles, and in accounting for the results observed, instead of limiting ourselves, with Dr. Parkes, to the assertion that "we can only express the facts by saying that a muscle during action appropriates more nitrogen than it gives off, and during rest gives off more than it appropriates," I think what we ought rather to say is, that during exercise the *system* appropriates more nitrogen than it gives off, and during rest gives off more than it appropriates.

Voit, however, disputes the fact of exercise producing *any* influence over the elimination of nitrogen, and has taken exception to some of Dr. Parkes's experiments, on the ground, more particularly, that the daily ingress of nitrogen could not be kept sufficiently stable. This has elicited from Dr. Parkes a further series, the results of which are recorded in the Proceedings of the Royal Society for March, 1871. In these it appears that there was no change induced, either at the time or afterwards, by a moderate amount of additional exercise, under a mixed regulated diet; but, under a non-nitrogenous diet the increase in the nitrogen on the following day to the performance of a hard day's march was exceedingly striking. The non-nitrogenous diet was continued through five successive days. During the first three it was associated with the ordinary work of a soldier; on the fourth, with a march of thirty-two miles carrying a weight of forty-three pounds and a half; and on the fifth with rest. As an ordinary result of abstinence from nitrogenous food, the eliminated urinary nitrogen underwent a steady decrease during the first four days; on the fifth, however, it showed a marked ascent, the amount being then in considerable excess of that discharged on the first.

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INTESTINAL WORMS.

By William Date, M.R C.S.

Although it is probable that worms in the human intestine never cause the death of their host, yet they undoubtedly often give rise to symptoms of an alarming character. Moreover, they are so common and so disagreeable in their effects that it may not be out of place to consider the different species met with in this country, the symptoms they cause, and the treatment they require. Many of these cases, either from the real nature of the symptoms being overlooked or from being considered beneath notice by the regular practitioner, fall into the hands of quacks. These persons, by drastic remedies, succeed in expelling the worm, often without curing the patient, and reap all the credit of success. I have resided for some years in places where intestinal worms were prevalent, and have thus been led to devote some attention to their nature and to a trial of various remedies recommended for their relief; and I propose to lay the results at which I have arrived before the readers of The Lancet.

There are only four species of intestinal worms commonly met with in this country, and which now need to be considered. These are: the Tæniæ or tapeworms—viz., Tænia solium and Tænia mediocanellata; the Ascaris lumbricoides, or long round worm; and the Oxyuris vermicularis, or short thread worm. The long thread worm (Trichocephalus dispar) is also said to occur frequently. If so, the symptoms caused by its presence are indefinite, and, owing to its small size and brownish color, it escapes observation. The broad tapeworm (Bothriocephalus latus) is common in Switzerland and northern Europe, but is never seen in this country except in imported cases.

The two indigenous species of tapeworm have a considerable general resemblance, and are often confounded one with the other. Tænia mediocanellata may, however, be easily distinguished from Tænia solium by the flat head and absence of hooklets and by the larger size of the joints. In consequence of this distinction not having been observed, T. solium was formerly supposed to be much the more common of the two in this country. It is now conceded that the reverse of this is the truth. Practically there is not much gained by distinguishing the species, as there is not much difference in the symptoms, and the treatment is the same for both. T. mediocanellata is rather the more robust and difficult of expulsion.

The old notion of the spontaneous generation of worms in the intestinal mucus is long since exploded. Thanks mainly to the patient investigations of the German helminthologists, the general conditions under which tæniæ originate from ova, grow and arrive at maturity, have been accurately noted and recorded in all the stages of their development. The ova are never matured in the human intestince, but are discharged either in the parent proglottis or in a free state. After a sojourn on the face of the world more or less brief, during which embryonic formation takes place, they are swallowed by swine or by cattle. In the intestine of their new host the embryo becomes released by the solution or rupture of its envelope, and, by means of a boring apparatus, penetrates its way into the muscles of these animals, where it becomes encysted. In this state it forms the "cysticercus" or "measle," which, in pork, is the larval condition of T. solium, in beef and veal, of T. mediocanellata. These cysticerci, when swallowed by man, become developed into the mature animal of the respective species. Besides pork, beef, and veal, it is possible that certain parts of the sheep may be the medium for the introduction of tapeworm. The embryo of T. serrata, swallowed by sheep, becomes encysted in the muscles, or bores its way into the brain, and there becomes developed into the Cœnurus cerebralis, the cause of that peculiar disease called the "staggers." This cœnurus administered to dogs develops into T. serrata. Now Von Siebold says that T. serrata is only another form of T. solium. If so, then the cœnurus might be the cause of T. solium in man.

A great outcry has recently been raised against sewage-grown-produce as a medium for the dissemination of entozoa. Whether this charge is just is far from certain, but, inasmuch as it is supported by a practical helminthologist like Dr. Cobbold, it certainly demands investigation. It is said that the ova floating in the sewage adhere to the crops, and are thus swallowed by the animals fed upon this produce. I cannot, however, see that ova are more likely to be transported on to the land in liquid sewage than they are in solid manure. The contents of our cesspools or the refuse of our dry-earth closets must be equally liable to contain them, and it appears to be at least questionable whether they would not adhere as readily to the roots or stems of plants when conveyed in the latter medium as in the former. Nor am I aware of any positive evidence brought forward to support the charge. On the other hand, Dr. Cobbold himself has recently dissected and examined with minute care a cow reared and fattened on sewage-grown produce alone, and he has, with great candor, admitted that the animal was entirely free from internal parasites of any kind whatever. Although Dr. Cobbold afterwards endeavors to explain away the force of this absence of entozoa and to lessen its influence on the controversy, there appears no reason to doubt that it is a fact of great weight, and forms a piece of evidence of much importance to the defence.

Patients suffering from tapeworm sometimes present a characteristic appearance which is easier to recognise than to describe. There are dark circles round the eyes, a white and pasty complexion, enlarged pupils, an absent, purposeless expression, with a hesitating manner and unsteady gait, giving a general impression of languor, both mental and bodily. There is often a peculiar "sinking" sensation about the epigastrium, with nausea and faintness. Pain is often complained of, and is described as being of a gnawing character. The appetite is capricious—sometimes voracious, sometimes the reverse. The tongue is generally pale and flabby. Females frequently suffer from derangement of the uterine functions and palpitation of the heart. Dr. Graves mentions a case in which a cough which long resisted treatment was speedily cured by the expulsion of a tapeworm. When the symptoms point to tapeworm as their origin, the motions should be examined for a few days. As a separation and discharge of the caudal joints is a frequent occurrence, their appearance will often decide all doubts.

Many are the remedies which have, at one time or other, been recommended for tapeworm. It is only necessary here to refer to two or three of them. *Turpentine* acts with a considerable degree of certainty, but, if given in an effectual dose, is apt to produce disagreeable intoxication and strangury. *Kousso* was at one time a fashionable remedy. It is not easily obtained genuine, and (perhaps for that reason) is uncertain in its action and not to be relied upon. *Kameela*, a powder which consists of minute glands from the capsules of Rottlera tinctoria, has been highly lauded. It is said to be certain and speedy in its effects, and not more unpleasant in action than an ordinary purge.* I have tried it in a few cases, and found, when given in effective doses, that patients complained of the severe griping pain which it induced. Dr. Garrod says he has found its active purgative properties rather objectionable.† For the last few years I have been accustomed to rely entirely on the etherial tincture of the root or rhizome of the male shieldfern (*Aspidium filix mas*); and, when properly administered—i. e., after due preparation of the patient,—I have found it thoroughly efficient. Sometimes, however, it requires a considerable amount of persuasion to induce the patient to submit to the necessary restrictions, which are certainly by no means pleasant nor always convenient. No food must be taken after breakfast except mutton-broth or tea. In the evening the patient takes a brisk dose of castor oil or compound senna mixture. The object of this is to clear away accumulated mucus, and expose the worm to the full action of the vermifuge. The following morning, early, one drachm and a half of liquid extract of male fern, rubbed up with half an ounce of mucilage, should be taken in about two ounces of milk. It is better to lie quiet for two or three hours after taking this dose, otherwise it is apt to cause nausea and faintness. At the expiration of that time another aperient dose should, if necessary, be administered, since the male fern itself does not always purge. Milk is the favorite food of the worm, and is therefore the best vehicle for the administration of the medicine. It is commonly recommended to be taken as food the day before. I regard this as a mistake, as the animal becomes satiated with it, and is thus more likely to avoid the poison. Nothing but broth or tea should be taken until the bowels have been evacuated, unless it be a little brandy-and-water in case of faintness. After undergoing this ordeal the patient may be allowed a week's rest. The whole process should then be repeated. This time perhaps no fragments of the worm will be expelled. I have given the male fern in several cases with perfect success after other remedies had failed. In no instance have I found it necessary to give more than three doses, although I always give one dose after any joints are expelled. Generally the whole of the worm has been killed by the first dose, and thus only two doses have been required. It is seldom the head can be found, owing to its small size, and, perhaps, its maceration in the intestinal juices. The discovery of some of the smaller joints may be regarded as tolerably satisfactory evidence of the destruction of the worm.

Being satisfied as to the entire expulsion of the worm, the next thing is to prevent the development of others. For this purpose, two indications are to be observed:—1. To avoid the introduction of living embryos. 2. To restore the tone of the intestinal mucous membrane, so that it may not offer a favorable nidus for their development. ***No meat should be eaten which has not been thoroughly subjected to the action of heat.*** It is clear that tapeworms cannot originate *de novo* in the intestine. It may not be possible always to avoid the "measle," but we may always kill it by fire and render it innocuous. To fulfil the second indication nothing is more useful than a mixture of nitro-hydrochloric acid with a bitter infusion. If there be much anæmia, as there often is, a few minims of tincture of perchloride of iron may be added to each dose, as in the following mixture:—Dilute nitro-hydrochloric acid, three drachms; tincture of perchloride of iron, a drachm and a half; syrup of oranges, half an ounce; infusion of quassia to eight ounces: take one ounce three times daily. At the same time general hygiene should be well attended to. The diet should be plain and nourishing. A daily sponge bath also will be of service.

The natural history of Ascaris lumbricoides, although pretty well understood, has not been placed so entirely beyond all doubt as that of tapeworm, since it has not been actually traced through all the stages of its development. Some of its changes of condition have been observed; but one link in the chain is still wanting. The sexes are separate (unlike tæniæ), the female being rather larger than the male. The ova are never matured in the human intestine. They leave their first habitat in large numbers, adhering to the fæces, and are wanderers upon the face of the earth for an indefinite period, until they find favorable conditions for the first change which they undergo, which consists of segmentation of the yelk and formation of the embryo. This may not take place for months after their expulsion. The embryo is enclosed in a thick tough envelope, which resists wear and tear, and preserves its occupant during the vicissitudes to which it is exposed. So far all is clear enough; but the next stage is still doubtful. It is by no means certain whether they pass now into the human intestine and there develop into the mature animal, or whether they undergo a previous metamorphosis in the tissues of some other host. Judging from the analogy of tæniæ, we might expect the latter to be correct. It is possible, however, that they find an entrance floating in drinking-water or adhering to imperfectly cooked vegetables.* Any way, there s no difficulty in understanding the prevalence of this worm. Dr. Eschricht has calculated that the matute A. lumbricoides contains the almost incredible number of 64,000,000 of ova. Looking at the immense number of these which must be expelled, to find their way on to our fields and into our rivers—remembering the thick envelope in which they are enclosed and by which they are so efficiently protected—remembering the long time during which they may remain dormant, but ready to spring into active life the moment a favorable opportunity arrives—remembering their small size which shields them from observation,—taking all these things into consideration, we are driven to the disagreeable conclusion that the ingestion of these ova must be an exceedingly common occurrence, and that it is only the general absence of favorable conditions for their development which prevents them from being much more frequent guests of humanity than they actually are. Let them only find a collection of mucus or some other favorable nidus, and they will soon take up their abode and quietly spend there the remainder of their existence, or, at least, remain until they receive summary "notice to quit."

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* Aitken: Science and Practice of Medicine, vol. i., p. 903.
† Garrod: Materia Medica, p. 313.

* There is some evidence in favor of this view. See "Science and Practice of Medicine" by Dr. Aitken.

Tapeworms are more frequent in adults—most frequent in females. Round worms are more common in children than in grown-up people; and, as might be expected from the more delicate nervous organisation of the former, the attending symptoms are often of a more serious character. It has been the fashion of recent writers—for there is a fashion even in matters medical—to assert that worms, of themselves, never give rise to symptoms of any kind. This view, no doubt, is a reaction from the old notion which attributed to intestinal worms all sorts of extraordinary phenomena. The truth, as is so often the case, seems to lie between the two extremes. I have repeatedly seen cases in which the expulsion of the worm has been followed by a relief of urgent symptoms so immediate and so marked as to convince me that the worm itself was the cause of the mischief, and this without any evacuation of mucus or fæces sufficient to account for the change. On the other hand, it cannot be denied that worms may and often do exist in the intestinal canal, even of delicate children, without giving rise to any very special symptoms. It may be that reflex irritation is induced only in children of very excitable temperament, or in certain peculiar states either of the general nervous system or of the intestinal mucous membrane. Certainly the gravity of the symptoms does not appear to depend on the number of the worms, except in those rare cases where they have multiplied to such a degree as to cause obstruction of the bowel. In children we generally find some or all of the following symptoms: The appetite capricious. Pain of a gnawing or griping character; even where this is not complained of, deep pressure often reveals an unnatural degree of tenderness. There is often a tumid condition of the belly. The state of the bowels varies; commonly there is constipation alternating with attacks of diarrhœa, sometimes severe, a considerable quantity of slimy mucus being discharged, followed by great relief. In rare cases blood is passed. These worms are often expelled during the progress of enteric fever, and according to my experience, the expulsion is often followed by a relief of the more urgent bowel symptoms.* The tongue is pale, flabby, indented by the teeth, and often has a peculiar shiny appearance. The pupils are generally dilated. Squinting sometimes occurs, and nervous twitchings, sometimes almost amounting to chorea. Even convulsions may supervene. The sleep is restless; the child grates its teeth, and often awakes suddenly with a startled expression and cries. Fever often runs high and assumes the remittent type, with great heat of skin. In short, the symptoms often resemble those of hydrocephalus. The cases under consideration may be distinguished from the latter by the more distinct remissions; by the previous history, showing the primary symptoms to be referable to derangement of the alimentary canal; by the less obstinate character of the constipation; and, finally, by the expulsion of the worm. Other cases may simulate tubercular disease; but here, again, a careful consideration of the symptoms will clear up the diagnosis. Thermometric observations are particularly valuable. In tubercular disease, the temperature, although it may vary, is always above the natural standard; in these cases the temperature is either normal or the elevation is only temporary. The above symptoms are not to be observed in all cases, and are often very obscure. When many or most of them exist, without any other apparent cause, we shall be justified in administering an anthelmintic. The appearance of a worm will set all doubts at rest. Dr. W. H. Ransome* recommends a microscopic examination of the fæces for the ova. To those possessed of a microscope, with the necessary skill and time for its use, and a stomach strong enough to face the required manipulations, this, no doubt, is the most satisfactory method of diagnosis.

The best remedy for these cases is *santonine*. It stands *facile princeps*, and, if properly administered, may be said to be a specific. It may be given either in a single full dose, or in repeated smaller doses. I prefer the latter method. A large dose is apt to produce disagreeable symptoms; such as irregular pulse, singing in the ears, vertigo, and nausea. There is sometimes partial suppression of urine, but this is always temporary. Sometimes all objects appear of a green color. The urine is invariably of a cloudy orange appearance, changed by liquor potassæ to a bright-red. Dr. Sieveking has recorded a case in which severe urticaria twice followed the administration of a very small dose to a child four years old; and he refers to a case reported in the "Annales de Thérapeutique" for 1852, in which a child of the same age, "after taking three grains of santonine, experienced severe abdominal pains, vomiting, purging, great prostration, cold sweats, spasm of the extremities, and dilated pupils; but ultimately recovered." These cases are doubtless quite exceptional. Nevertheless it is better to avoid the risk by giving repeated smaller doses; which, indeed, are quite as effectual. Thus, for a child five years old, I should order three or four grains every night at bed-time. A slight tea of mutton-broth with very little bread may be allowed, but no food should be taken after this. The powder must be taken in dry sugar or honey, and no liquid swallowed immediately after. I have found it more efficacious when this caution is observed. It may be that the drug is not so readily absorbed by the stomach as when dissolved in liquid, but passes on into the bowel to exert all its effects upon the parasite. Before giving the santonine the bowels should be well cleared out by a brisk purge. Nothing is better for this purpose than the old mixture of salts-and-senna, as in the mistura sennæ co., which acts so efficiently on the small intestine. The powder should be taken every night for a fortnight; and, as it is not itself aperient, it may be necessary occasionally to repeat the dose of compound senna mixture. It is not sufficient, however, merely to dislodge the worms and pronounce the case cured. The morbid condition of the mucous membrane, which favors their development, still remains, and must be dealt with. It is probably a neglect of this precaution which occasions the numerous cases of relapse which one sees and hears of. To restore the tone of the intestine and prevent the reaccumulation of mucus, the treatment is the same as after tapeworm. At the same time we should endeavor to prevent the introduction of living embryos. If there is any suspicion about the drinking-water, it should be boiled and filtered. All vegetables, as well as meat, should be thoroughly cooked. Salads, therefore, are to be avoided. General hygienic measures are specially to be looked to.

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* I have seen the long thread-worm expelled in like manner.

* A System of Medicine, by Dr. Reynolds, vol. iii., art. Intestinal Worms.

The short thread-worm (Oxyuris vermicularis) inhabits the lower bowel. Its life history is not clearly determined. Except that embryonic formation takes place before the ova leave the intestine, the different metamorphoses probably differ but little from those of A. lumbricoides. Short thread-worms may exist in considerable numbers without their presence being marked by any very striking symptoms; but they generally excite a good deal of local irritation, and, by the intolerable itching which they occasion, they render the patient's life miserable. They are often most annoying in bed, or after particular articles of diet, such as onions, carrots, and some kinds of fruit. These might possibly act as prophylactics. In females they sometimes escape into the vagina, and cause troublesome leucorrhœa. They may always be detected in the stools. They often co-exist with the large round worm; hence the presence of the one should always lead to inquiries for the other. Beyond the local irritation which they occasion, and which is often annoying enough, thread-worms do not cause much harm. In children, however, they are often attended by a lax state of the rectum; and this, with the straining which they induce, sometimes results in prolapsus ani.

As oxyures infest the lower bowel, they are best reached by enemata. Of these, one of the simplest, and by no means the less efficacious, is a solution of common salt in infusion of quassia, in the proportion of two ounces to a pint, given every morning. Where this treatment fails in entirely dislodging them, they are probably resident high up in the colon. It is then advisable to continue the injections, and at the same time to administer by the mouth small doses of aloes, in combination with iron and quinine. The following forms a useful pill for adults:—Sulphate of iron, sulphate of quina, aloes pill with myrrh, compound galbanum pill, of each one grain: to be taken three times daily. For children, wine of aloes, with syrup of ginger, may be ordered. If this treatment be continued for a fortnight or three weeks, the patient will rarely be further troubled with the annoyance. As the life history of these creatures is probably analogous to that of A. lumbricoides, the same preventive measures may be adopted. In addition to these scrupulous cleanliness must be enjoined, in order to avoid the risk of self-infection by the ova lurking in the folds of skin near the anus.

I have thus gone through most of the *practical* bearings of this subject. I might, had space permitted, have selected from my note-book some interesting illustrative cases. It may be objected by some that a knowledge of the life history of entozoa does not assist in their expulsion; but the higher province of the physician is the prevention, rather than the cure, of disease; and, as a means to this end, a knowledge of the origin and growth of these creatures becomes of the highest practical importance: "ista quoque naturæ rerum contemplatio, quamvis non faciat medicum, aptiorem tamen medicinæ reddit."

Krewkerne, January, 1872.

CASE OF EMPYEMA TAPPED TWICE;

GREAT IMPROVEMENT FROM DRAINAGE-TUBE.

By C. Theodore Williams, M.D., F.R.C.P.,

Physician to the Hospital for Consumption, Brompton.

William P——, aged twenty-eight, a letter-carrier, was admitted under my care July 4th, 1871. He had always enjoyed good health till four months and a half ago, when he caught a severe cold, followed by rigors, sharp pain in the right side, dyspnœa, and cough. He has lately been losing flesh, and for two days has had diarrhœa. The pain, dyspnœa, and cough have continued up to the present time; the cough is exceedingly troublesome, and the expectoration copious, muco-purulent, and rather offensive; there is orthopnœa; aspect pale; skin perspiring; bowels relaxed; appetite fair.

Physical signs.—Dullness, deficiency of motion and respiration, absence of vocal fremitus over the whole right side, the intercostal spaces being bulged, and the right shoulder considerably raised. Some tubular breathing and bronchophony is audible in the first intercostal space and above the scapula. On the left side the dulness extends slightly beyond the sternum into the second and third intercostal spaces, showing the right pleura and its contents to be encroaching on the left side of the chest. The heart is pushed to the left and slightly upwards, its apex being felt beating between the fifth and sixth ribs, 2 in. to the left of nipple; harsh breathing over the whole side. The diagnosis was empyema of the right side. Patient was put on the full hospital diet, and in addition ordered bacon for breakfast, beef-tea for supper, and four ounces of sherry.

July 8th.—Appears worse, having hardly slept, and suffering great pain in the right mammary region, where there is œdema and redness; the dyspnœa has increased, and diarrhœa continues; tongue furred; pulse 130; respiration 36; temperature 103·2°. The right side measures through the mammary line 1½ in. more than the left.

10th.—The diarrhœa has ceased; the cough is worse and the patient expectorated a pint and a half of muco-purulent matter in twenty-four hours; pulse 140; respiration 40; temperature 103·2°.

The symptoms of oppression of breathing being great, Sir Wm. Fergusson was called in, and at my request passed a trocar and canula between the fifth and sixth ribs, about 4 in. to the right of the nipple, and drew off four pints of purulent fluid, which issued freely at first, but when the stream began to abate Sir Wm. Fergusson used a glass exhausting syringe, and in this way drew off all the remaining fluid, and any air which had happened to enter. After the operation the patient seemed much relieved, his breathing being less labored, and in the evening his temperature fell from 103° to 101°. He passed a quite night.

11th.—The expectoration has diminished. Pulse 108; temperature 98°; respiration 24. Marked resonance and breath-sound with crackle are detected as low as the nipple anteriorly, and above the spine of the scapula posteriorly. Below these points there is dulness and absence of vocal fremitus. There are no signs of air in the pleura. The tubular sounds and bronchophony in the first intercostal space have entirely disappeared. Each side measures 16 in. through the mammary line, being a diminution of 1¼ in. on the right side. The patient's appetite being bad, an additional pint of milk was ordered and two ounces of brandy; and a few days later fish was substituted for meat.

On the 14th the temperature rose, the morning being 99·8°, the evening 103; pulse 116; respiration 32. On the 17th the swelling in the right mammary region had increased in size and become very painful. Pus began to ooze from the wound. He was ordered two grains of quinine twice a day in dilute sulphuric acid. Port wine was substituted for the sherry, and a chop for the fish, half a pint of ale being added.

22nd.—Some improvement was noticed on the establishment of the discharge, the evening temperature having fallen to 100°, and the pulse 108; but the patient is suffering great pain and shortness of breath. The swelling has much increased, and is very red and œdematous. At my request Mr. Bartlett, the assistant medical officer, opened the swelling with a bistoury, between the fifth and sixth ribs, an inch to the right of the nipple. Much pus streamed out, the abscess communicating with the pleural cavity. The next day the discharge ceased from the primary opening, but flowed freely from the abscess. He began to complain of sore-throat, this symptom afterwards developing into tonsillitis, which had subsided on the 26th. On the 25th it was found that the dulness commenced an inch below the right nipple, and above this point good breathing was audible on the patient sitting up.

28th.—Patient is free from pain; sleeps well; the wound discharges copiously. Morning temperature, 99·8° F.; evening, 101·8° F. Pulse 120; respiration 24. On probing the wound, a channel was found, passing probably between the two sets of intercostal muscles, and communicating with the primary opening, and thus with the pleura. This was a point of some importance to ascertain, as it explained, firstly, why the discharge from the primary opening ceased; and, secondly, why the pus had not become offensive, the muscles acting as a valve, and preventing the entry of air. After this the discharge increased.

On the 5th of August it became offensive, the morning temperature rising to 100°, the evening to 102°; pulse 104; respiration 24.

Aug. 7th.—The patient was seen by Dr. Tatham in my absence. He sleeps well, and is not disturbed by cough or dyspnœa. On the right side creaking friction is heard at the anterior and posterior apex; breathing is audible posteriorly, but there is dulness from the spine of scapula to base of lung; percussion hyper-resonant from clavicle to nipple; harsh breath on either side. His appetite being poor and spirits much depressed, Dr. Tatham ordered him a pint of champagne, and had him placed on a spring bed.

17th.—Appears much worse, and has lost flesh considerably; aspect has become more anxious; the discharge is not so free, and strongly offensive; skin of the right side is very red, partly from oakum dressing; dulness has increased in right front, but percussion and breathing are good at apex. Right side measures 15¼ in., left 15 in. Temperature, morning 100·2°, evening 101·2°; pulse 112; respiration 24.

21st.—Complains very much of scalding pain caused by the pus which oozes from the wound, and is offensive. Temperature: morning 101°, evening 100·7°. As the discharge was by no means free and had become offensive, and as the patient was losing ground, at my request Mr. Henry Smith (in Sir W. Fergusson's absence) saw the patient, and at 3 P.M. on the 22nd of August made a counter opening between the seventh and eighth ribs, two inches to the right of the spine; he first having passed a long silver probe into the anterior opening, carrying it across the chest until its point could be felt, and then cutting down upon it. A seton was passed from the opening in front to that behind, and at first no discharge came from the new aperture; but at 10.15 P.M. fetid pus flowed copiously through both openings, mostly through the front. Temperature 100·6°; pulse 120, small; respiration 32. Patient to continue champagne, but to take four ounces of brandy, and, if possible, to eat a double portion of chops daily.

24th.—The discharge is so copious that it soaks through everything, and amounts to some pints in the course of the day; it is so intolerably offensive that the patient has to be kept in a separate ward, and the smell appears to overpower him and anybody who happens to be present. He cannot sleep, perspires profusely, and seems much exhausted. Morning temperature 99·3°; pulse 120; respiration 32. At 3 P.M., at my request, Mr. Smith passed a drainage-tube from the front opening to the back, and injected into the side a solution of carbolic acid (1 part in 40). In the evening the patient complained of much pain in both openings, and a great deal of puriform fluid came through the front tube. The temperature fell to 99·1°—the first time it had been below 100° for six weeks; pulse 118; respiration 32. The quantity of brandy was increased from four to six ounces a day.

25th.—The change in the patient from yesterday is quite wonderful. He has slept well, and is free from pain and discomfort. The discharge is reduced to a small amount, and is slightly tinged with blood flowing chiefly through the posterior opening; the fetor is entirely gone. Temperature: morning, 98·8°; evening, 99°. Pulse 104; respiration 24.

26th.—Sleeps well; aspect greatly improved; discharge less; hyper-resonance over the front of the right chest as low as the fifth interspace, where dulness commences; good breathing over the whole anterior surface, with some crepitation in the lower fourth. A dessert-spoonful of cod-liver oil was ordered with the quinine twice a day.

Sept. 4th.—Has been injected with carbolic-acid solution every morning since Aug. 24th, and has steadily improved. Appetite is very good, and the patient is gaining flesh. Discharge slight, of a thin consistency, and not in the least fetid. The morning and evening temperatures have been normal during the last week; pulse 90; respiration 24.

On the 7th the physical signs were: Good breathing and fair resonance as low as one inch below the right mamma, dulness and deficient breathing commencing beyond this point; metallic tinkling audible in the right axilla; less dulness behind than in front. The right side measures 15¼ in., the left 16½ in. The champagne was stopped, and a pint of porter substituted; the brandy was gradually discontinued, and he was allowed a glass and a half of sherry.

29th.—Better; getting fat. Breathing free; but harsh above the mammæ; friction-sounds an inch below. Weight, 8 st. 5½ lb. A few days later Mr. Henry Smith examined him, and could hardly believe him to be the person on whom he had twice operated. He said he had never witnessed "so complete a resurrection."

Oct. 14th.—Dr. C. J. B. Williams, who had seen the patient shortly after admission, again examined him, and was much astonished at his progress. He found the physical signs similar to the above, and suggested that the carbolic-acid injection should be used less frequently. It was accordingly done on alternate mornings.

22nd.—The discharge has increased; cough is more troublesome. Injection to be repeated every morning.

25th.—Cough and discharge have subsided into the previous state.

Nov. 16th.—Has been increasing in fat, especially over the thoracic wall, where it is now difficult to detect the ribs. The discharge continues, but is very slight. The tube is still retained. The quantity of carbolic-acid solution which can be injected is about two ounces, which shows that the pleural cavity must be to a great extent obliterated, and that only a small portion of secreting membrane remains. There is much flattening of the right side, especially below the nipple. The right side measures 16 in.; the left 17¼ in.: the increase being chiefly owing to accumulation of fat. He has increased 9 lb. since last weighing, being now 9 st. Dulness commences an inch below the nipple in front, and at an angle of the scapula behind, and increases in intensity downwards, being most perceptible along the lower margin of the ribs. Anteriorly, respiration is good as low as the nipple; for two inches beyond some friction-sound is audible; below this respiration is absent. Posteriorly, it is indistinct below the scapula.

The following week the patient was sent to Eastbourne with the tube still in his chest, having been instructed to continue the carbolic-acid injection himself.

Few cases on record exemplify more strongly than this one the advantages of the use of Chassaignac's drainage-tube in empyema. A curious point in the case was the course which the pus burrowed for itself between the external and internal intercostal muscles before it pointed in the form of an abscess externally, the muscles seeming to act as a valve opposing the entry of air, and thus preventing the discharge from becoming foul. The increased pressure of the fluid rendered the valve inefficient, air was admitted, and the discharge became offensive.

Park-street, Grosvenor-square, Jan. 1872.

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ON DYSPEPSIA OF LIQUIDS.

By John C. Thorowgood, M.D. Lond.,

Assistant-Physician to Victoria-Park Hospital, and Lecturer on Materia Medica at Middlesex Hospital.

The cases here briefly recorded have seemed to me good illustrations of that indigestion of liquids which has been carefully described by M. Chomel, in his work on Dyspepsia published in 1857.

The affection, in its fully developed form, does not seem very common in this country, but during the last few years I have met with a few cases exactly corresponding to the description of the malady given by Chomel.

Case 1.—James McC——, aged thirty, a pale, dark, intelligent man, came under my care September 14th, 1871, complaining generally of dyspeptic symptoms, and specially of the great uneasiness caused by the presence of any amount of liquid in his stomach. Liquids in the slightest degree acid were most distressing to him, and at intervals he had attacks of sour pyrosis. He complained much of dryness of mouth, with dry skin and costive bowels; urine loaded with lithates, but free from traces alike of sugar or albumen. No loss of flesh; pulse slow and soft; nothing irregular to be found in heart or lungs; no sort of tumour or thickening about pylorus of stomach, but on gently vibrating this organ, fluid was heard splashing about within it, and this sound could be always produced irrespective of any liquid recently drunk. The stomach was much distended. The early treatment of this case consisted in the use of alkalies, with bismuth and various bitters, but no improvement resulted; the only noteworthy feature was the effect of a pill of extract of opium at night, which regulated the action of the bowels so completely that the patient asked for the pills as *aperient* pills.

Often at his visits this man drew my attention to the quantity of liquid always present in his stomach, and at last we agreed that he should drink as little as possible, and take no other medicine than a powder of rhubarb and magnesia every morning. From this time he steadily improved, and after about two months' treatment he was let go cured, and six months afterwards was still in good health.

Case 2.—A man aged fifty-three years, dark, sallow, and very hypochondriacal, had been dyspeptic for eighteen months, and had taken quantities of physic. He often has pyrosis, has large sodden tongue, costive bowels, and dry skin; urine dark in color, specific gravity 1030, acid, and nothing abnormal chemically or microscopically. The indigestion of liquids was not so marked in this case as in the one preceding, but no medicine gave relief till the dry plan of diet was enforced, then he amended considerably, and I lost sight of him.

Case 3.—A young healthy-looking man, aged twenty years, by trade a bricklayer, came to me complaining of faintness and palpitation of the heart, and said he had got water in his chest, for he could feel it. Examination of the chest showed the lungs and heart to be in every respect quite healthy; the stomach is large and dilated, and when he shakes the splashing noise of liquid therein is plain enough. He says he can produce this noise at any time. This patient has a dry skin; large sodden tongue; pulse 80; urine clear. No waterbrash.

So far as treatment has gone during one month with alkalies, bismuth, and various tonics, little good has been done. The dry-diet plan is the only means of effecting a regular cure; but as this involves a pretty complete abstinence from beer, the patient can hardly be made to give it a fair trial. That the less he drinks the better he is however, is a fact already well proved.*

At first sight this sort of cases may appear of a pretty ordinary kind; but what I claim as their characteristic features are these:—They are cases of obstinate dyspepsia, marked specially by the discomfort of feeling the stomach always distended as with fluid, while the mouth is usually dry, thirst troublesome, and yet drinking always aggravates the discomfort. There is no acute pain, and the patient rarely gets thin. Movement of the patient produces that splashing noise or "*clapotement*," as Chomel calls it, significant of the presence of wind and water in the stomach; and this can be produced at any time by the patient. Lastly, there is the almost certain amelioration of the symptoms under the influence of the dry plan of diet; and till that is followed no medicine does any good whatever.

It is necessary to seek carefully for any organic disease, for if there be stricture or thickening about the pylorus it will not add much to the doctor's credit to promise a certain cure if the dry plan of diet is followed; relief there may be, but not cure. Chomel says of one of his cases: "J'ai vu un autre individu, atteint de dyspepsie des liquides, qui, en appuyant avec quelque force la main sur l'epigastre, faisait remonter dans sa bouche plusieurs onces d'un liquide aqueux; mais ce fait a été unique pour moi." I have sought to elicit this symptom in the cases recorded, and in others, but have not been able to produce it.

At times fits of fainting, with irregular action of the heart, are prominent symptoms in these cases of dyspepsia of liquids, the cause being due to the distended state of the stomach. Marked relief is obtained to these symptoms on the dry-diet plan, and this confirms the soundness of the practice of not allowing patients with heart disease to live much on liquids and slops, but rather to feed them on easily digested solids, such as mutton, chicken, boiled fish, and game. These are all forms of solid food, suitable in cases of dyspepsia of liquids. Milk does not seem otherwise than beneficial, but much of farinaceous or vegetable food should be avoided.

The patient must bear a certain amount of thirst as well as he can, and when he does drink must take but very small quantities at a time, and not drink for an hour or more after he has taken his meal of solid food. Weak whisky-and-water, sherry wine, and toast-and-water, are among the least objectionable drinks; and sometimes a small cup of pure beef-tea, free from any farinaceous admixture, will suit well. One of my patients found a wineglass of good stout to agree well, and relieve his thirst.

Compared with diet, medicine is but a secondary matter; but a dose of Gregory's powder in the morning, and a tonic mixture of rhubarb and tincture of nux vomica twice a day, I have found serviceable. Mild saline purgation I have tried, but its effects are very uncertain, relieving at times slightly, at other times adding decidedly to the discomfort, and it is not a method of treatment to be recommended. A pill of opium at night, when there is much nervousness and hypochondriasis, is certainly beneficial, and a pill of bitter extract or of galbanum and assafœtida twice a day may be used with advantage.

It is rare for the patients to emaciate, provided the case be uncomplicated. In one of Chomel's cases, however, he noticed a degree of emaciation that caused great anxiety, being an unusual symptom, and likely to indicate organic visceral disease. The dry regimen was enforced, and the lady got quite well, though any over-indulgence, even in such a fluid as plain water, would be followed by return, in some degree, of the unpleasant symptoms.

Welbeck-street, Feb. 1872.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

KING'S COLLEGE HOSPITAL.

REMOVAL OF A LARGE FIBROUS POLYPUS FROM THE PHARYNX.

AMONG the operations on the 27th January, was one by Mr. Henry Smith of more than usual interest. The patient was a lad of about twenty, who had a large fibrous polypus growing from the upper part of the pharynx behind the posterior nares, probably from the pterygoid process of the sphenoid. The mass hung down behind the nostrils, completely blocking them up, and pushing forwards the soft palate to a very considerable extent. Its presence seriously impeded respiration, and the patient's health had deteriorated in consequence of the faulty aëration of the blood. His respiration, especially during sleep, was extremely noisy, and he had proved from this cause a source of great annoyance to the other patients in the ward. Mr. Smith has had one other case almost exactly like the present, which, strangely enough, had been sent to him from the same village.

The growth, which was as big as a moderate-sized pear, was removed without much difficulty. The patient being anæsthetised, and a gag having been placed in the mouth, the mass was seized with a pair of strong polypus forceps passed into the mouth with the left hand, and dragged forcibly forwards. An examination was then made with the forefinger of the right hand passed up behind the palate; and the fact was ascertained that the tumour grew by a fine pedicle from the situation above mentioned. A second pair of polypus forceps was then passed through the right nostril, by which the pedicle was seized quite at its root, and by a little twisting in this situation, whilst forcible traction was kept up in the mouth, the whole mass broke away, and was removed through the mouth. Considerable hæmorrhage ensued, and it was necessary to discontinue the anæsthetisation. When the hæmorrhage had ceased the nostrils were examined, and a second independent polypoid mass was found in the left nostril, and removed in the ordinary way.

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* Since this paper was written this man has left my care cured; experience having taught him the value of the dry-diet plan.—J. C. T.

TENOTOMY.

Sir William Fergusson divided the extensor tendons of the four larger toes of the right foot of a girl about twelve years old. She was the subject of slight talipes equinus. In this condition there is, said Sir William Fergusson, a tendency to draw the toes upwards, so that the patient walks almost entirely upon the distal ends of the metatarsal bones. In consequence of this, a row of troublesome corns is apt to be engendered along the line of pressure; and, the knuckles of the toes being forcibly drawn up against the upper leathers of the boot, corns appear in this situation also. This condition of things is very painful, and the foot is rendered utterly useless. It was to remedy this that the present operation was performed. Both feet were affected, but Sir William had thought it best to operate upon one at a time, and had chosen the worst for the first operation.

HYDROPS ARTICULI; TREATMENT BY TAPPING.

The subject of this affection was a man aged fifty-nine, a patient of Mr. Henry Smith's. He had suffered from chronic effusion into his knee-joint, which had resisted every form of treatment. As a *dernier ressort*, Mr. Smith tapped the joint with a small ordinary trocar. Eight ounces of fluid were drawn off, the limb was placed upon a splint, and the man was sent home. This was effected about eight months ago in the out-patients' room. He made a perfect recovery, and regained a considerable amount of movement in the limb. Six months later, however, an abscess formed on the outside of the joint, for which he was admitted to King's College Hospital. The abscess being opened, it was found to extend round the upper part of the joint, but to have no connexion whatever with its interior, which, to all appearance, is healthy.

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LONDON HOSPITAL.

TREATMENT OF HYPERPYREXIA BY BATHS.

(Under the care of Dr. SUTTON.)

THE governors of the London Hospital, on the suggestion of their resident medical officer, Mr. Mackenzie, have made arrangements, on a scale of great liberality and completeness, for carrying out the treatment of hyperpyrexia by baths. In a small ward, containing two beds, there has been placed a bath, with its head only against the wall, so that a patient can be lifted in and out of it with the greatest facility. This bath is fitted with a large supply-pipe for both hot and cold water, and a rapid waste. The cold water pipe comes from a tank, into which, if need be, lumps of ice can be placed, so as to reduce the temperature with the greatest rapidity. By these arrangements the temperature of the bath has actually upon one occasion been reduced fifty degrees in seven minutes. Not the least of the advantages of these arrangements is the fact that the treatment is robbed of all its terrors, and that the patient is not frightened by an array of buckets and lumps of ice, &c.

We append two cases taken from among Dr. Sutton's patients, which illustrate the mode of treatment. These cannot be spoken of as hyperpyrexial, neither of them having reached that temperature (106° F.) at which, in the opinion of Dr. Wilson Fox, the cold bathing should be commenced. It is interesting to remark that, in the case of rheumatic fever, a fall of temperature followed immediately upon the bathing; while, in the case of typhoid fever, large doses of quinine apparently accomplished (?) what cold bathing failed to effect. It will be remembered that in Dr. Wilson Fox's remarkable cases, which were both of them rheumatic fever, large doses of quinine utterly failed to reduce the temperature in any appreciable degree. In the first of these cases recorded below slight diarrhœa and albuminous urine seemed upon one occasion to follow as a consequence of the bath.

Case of Rheumatic Fever with moderately high temperature successfully treated by baths.—The patient, a girl sixteen years old, was admitted to the London Hospital on September 16th. on the twelfth day of her first attack of rheumatic fever. On admission her condition is thus described by Dr. Sutton: "Patient lies on her back, her mouth open, her eyes staring in a vacant manner; seems to pay no or very little attention to what passes around her. If spoken to she says, in a very excited, irritable manner, 'I want my mother.' Her nostrils expand very much during inspiration. Her breathing is labored, and her cheeks are very flushed." There was a well-marked mitral systolic murmur. At 3.15 P.M (her temperature being 104°, pulse 140, respiration 26) she was placed in a bath at the temperature of 96°. She was kept in the bath twenty-five minutes, the temperature of the bath being reduced to 83°. When the patient was removed her temperature was 102·9°, pulse 126, respiration 22. When first put into the bath she seemed very comfortable, but when the temperature of the bath was reduced she complained of feeling cold, and cried out a good deal. When removed to bed she said she felt very comfortable; she had lost her wild expression, answered questions rationally, and, closing her eyes, passed off into a sleep. At 9 P.M. the same evening her temperature again rose to 104·4°, pulse 126, respiration 30. She was bathed again as before, and in twenty minutes her temperature was reduced to 102·2°. At midnight on September 17th (temperature 104·1°, delirious and fretful) she was placed in the bath (temperature 98°, reduced to 82°), and when removed her temperature was 102°. When removed from the bath she was observed to shiver. After the bath during the night she passed three liquid dark-yellow stools. On the morning of the 19th her temperature was 101·6°; her general condition had improved; her urine was loaded with lithates, and contained a fourth part of albumen. From that date her condition gradually improved, and on October 17th she was sent into the country.

Case of Typhoid Fever with high temperature; the bath unsuccessful; large doses of quinine quickly followed by a fall of temperature.—The patient, a woman aged twenty-two, appeared to have reached the eighth day of a mild but well-marked attack of enteric fever. She progressed favorably in all respects until the end of the third week, when the evening temperature, instead of falling, showed a persistent tendency to rise. On the evening of the twentieth day the temperature in axilla was 104·4°, pulse 110, respiration 28. Between the twentieth and twenty-eighth days she had in all nine baths (the baths were similar in temperature and duration to those used in the last case, but the reduction was usually made to 70° instead of 80°). These baths had hardly any effect in reducing the temperature, which, at the most, within a few hours of the bath was as high as it was before. The first bath seemed more effectual than the last. Three-quarters of an hour after her last bath the temperature was 104·8°. On the twenty-ninth day it was 105·9°, and on the evening of the thirtieth 105·5°. The administration of quinine was now commenced in doses varying from ten to twenty grains. A fall of temperature immediately followed the exhibition of the quinine, but the fall was very gradual and slow, and it was not till the forty-fifth day that the normal temperature was reached. With the exception of a slight return of pyrexia on the fifty-third day, which quickly yielded to quinine, the patient made a good recovery, and is now recruiting her health at the sea-side.

PERIPHERAL HEMIPLEGIA.

(Under the care of Dr. Andrew Clark.)

The following case for much information concerning which we are indebted to Mr. M'Kenzie, the resident medical officer, seems to agree in most particulars with the disease described by Todd under the name of "peripheral hemiplegia." In a footnote to Todd's account of this disorder, a case (recorded by Dr. Cheyne) is recited which very closely resembles the one under consideration.

The patient is a man thirty years of age, a musician. His general health is, and always has been, very good. There is no evidence of a syphilitic taint. Two years ago he began to be troubled with an inability to lift his right toes, which were constantly troubling him by catching in the carpet or other obstacles. This was followed by a gradual loss of power over the right leg. Fifteen months later he began to find some difficulty in manipulating his violin bow; and since that time loss of power in the right arm has gradually supervened. At the present time there is some loss of power over the left limbs as well, but chiefly of the leg. There is no facial paralysis; but there is a general want of expression in the face. His mental state is not markedly effected. He talks rationally and sensibly about his ailment. The tongue is protruded forcibly in the middle line, and the ophthalmoscope shows a healthy condition of the fundus oculi. There is no loss of power over the sphincters; but he is rather troubled with constipation. At present his mode of progression is very striking. He uses a stick with his left hand, and advances his left leg first, slowly and cautiously. The right leg is then brought forward with a regular hemiplegic swing, the left shoulder is protruded at the same time, and the head turns to the left. He has no appreciable loss of sensibility of either the arms or the legs. At no time has he been troubled with any numbness, tingling, cramps, or spasm of the limbs. He is fairly nourished, and there is no marked wasting of any of the muscles.

OVARITIS; TETANY.—SIMILARITY BETWEEN OVARITIS AND TYPHOID.

(Under the care of Dr. Ramskill.)

In the wards set apart for female venereal patients we saw a case which seemed to answer in all respects to the disease descibed by Trousseau as "tetany". The patient was a female twenty-four years of age. She had been admitted for gonorrhœa, and subsequently had suffered from ovaritis. In addition to the inflammation of the ovary, she had suffered from diarrhœa, and during the attacks of diarrhœa she had been subject to tonic contractions of the extremities—the hand assuming the shape (as Trousseau described it) of the accoucheur's hand, the fingers being stiffened out and the opposite edges of the palm drawn together. Trousseau described the disease as being most common in, in fact almost limited to, the female sex, and as particularly liable to occur during periods of sexual excitement. He further states that diarrhœa is a common exciting cause of the spasmodic contractions. In this case we have all the favoring conditions brought together in a marked degree.

There were in the ward several cases of ovaritis, and it was pointed out to us that occasionally there was considerable difficulty in diagnosing between this condition and typhoid fever. We saw one case, in particular, in which for twenty days there had been a temperature ranging between 99·5° and 105°. There had been considerable dulness of intellect during this period, together, with marked tenderness in the right iliac region and considerable diarrhœa, the stools being very like those characteristic of typhoid.

CASE OF KELOID TREATED BY IODIDE OF POTASSIUM.

(Under the care of Dr. Davies.

Our attention was also called to a man suffering from a considerable development of keloid in some old syphilitic scars with which his body was pretty freely sprinkled. The keloid was rapidly disappearing under treatment by ten-grain doses of iodide of potassium. He had, as well, a tender node upon the forehead.

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GREAT NORTHERN HOSPITAL.

AXILLO-SUBCLAVIAN ANEURISM TREATED BY PRESSURE ON THE CARDIAC SIDE OF THE SAC; SLOUGHING OF THE INTEGUMENT AND DEEP TISSUES AROUND THE SAC; RECOVERY OF THE PATIENT; PARTIAL CURE OF THE ANEURISM.

(Under the care of Mr. Gay.)

The following case, to which there is only one parallel (as regards treatment) in Mr. Poland's tables of subclavian aneurism, is of great interest, showing as it does the advantages and the dangers of long-continued pressure at the root of the neck.

The patient was a strong healthy man, aged forty-five. He had a tumour (presenting all the characteristics of an aneurism), about the size of a Tangier orange, at the root of the neck on the right side. The aneurism was partly covered by the clavicle. The anterior wall was very thin. Pressure on the subclavian artery caused arrest of the pulsation in the aneurism, and in the radial artery of that side. The patient had noticed the tumour for about six weeks only; but for six or eight months past he had been troubled with rheumatic pains and numbness in the right arm and shoulder.

After much consultation it was determined to try pressure on the cardiac side of the aneurism. This was commenced on the 14th of October, 1871, and was at first simply "digital," being maintained, without intermission, by relays of students and others, who all took a deep interest in the case. It was soon found advisable to substitute Mr. Cole's compress for the finger. This is a T-shaped instrument, provided with a soft pad, about the size of the end of the finger, at the bottom of the perpendicular limb, by which pressure is made on the artery. This perpendicular limb is jointed to the horizontal limb (on which the compressing hand rests), and is provided with delicate springs, so that the pressure can be adjusted to the necessities of the case with the utmost nicety.

In this way the pressure was maintained, with brief periods of intermission, for four days and nights. The pressure caused considerable suffering to the patient, numbness in the hand, and an aching pain along the course of the ulnar nerve, especially where it passes behind the inner condyle of the humerus. These symptoms were relieved by the use of morphia and chloral. When the pressure was taken off, the pulsations in the aneurism and radial artery were nearly extinguished, and the bruit in the aneurism had almost subsided. It was intended to renew the pressure, but it was found that the scarf-skin was loosened, and the skin beneath it was moist and of a dusky color. Erysipelas now set in, and began to spread in every direction, but most rapidly over the aneurismal sac and towards the shoulder. The constitutional symptoms became very grave; the temperature was subject to great variation, alternating between 96° and 102°; the tongue was dry, the pulse quick and weak, and the face dusky. Sloughing commenced, and ultimately invaded, not only the skin, but the deep tissues as well in the neighborhood of the sac, involving the deep fascia, and destroying the anterior belly of the omo-hyoid muscle. From this part matter and sloughy tissue welled up, and exhibited movements corresponding with those both of the artery and chest. On the third day of these symptoms he was attacked with pneumonia of the right lung, which it was feared might be pyæmic. On the eighth day the symptoms abated, the wound began to granulate, and by the end of eight weeks it had cicatrised, and he was restored to tolerable health.

On examining the aneurism, it was found still to exist, but the sac was smaller and thicker, and appeared to have shifted somewhat towards the shoulder. On its distal side pulsation could be felt as of a large artery. Pressure on this vessel, as well as on the subclavian on the cardiac side, stopped the current through the sac and through the radial, and altered the character of the bruit. The force of the current through the sac and down the radial was considerably lessened.

It was observed that during the time the pressure was maintained, and for several weeks afterwards, the fingernails on that side ceased to grow, and that during recovery the back of the wrist and lower end of the forearm became covered with hair. The superficial veins have been constantly much more filled than those of the opposite limb.

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CHARING-CROSS HOSPITAL.

CASE OF PARALYSIS IN A SYPHILITIC SUBJECT TREATED BY LARGE DOSES OF IODIDE OF POTASSIUM; CURE.

(Under the care of Dr. Julius Pollock.)

The following case presents many points of interest. The paralysis, though strange and anomalous, was unusually extensive and complete, affecting, not only the limbs and the face, but the muscles of deglutition as well. The patient presented unmistakable marks of constitutional syphilis, which, there can be little doubt, was contracted eighteen years ago, and his body was covered with a number of scars which had probably resulted from the healing of rupial or ecthymatous ulcers. Further, he had a previous similar paralytic attack four years before. And, lastly, his recovery was complete and rapid, though, on admission, his appearance would have justified the gravest prognosis.

J. S——, a bricklayer, was admitted to the hospital on November 25th. He was a fairly strongly-built man, but of a pale and cachectic aspect. The legs and part of the abdomen and back were covered with large scars. He had considerable loss of power over his legs; he could not stand upon them; but when lying in bed he was able, with some difficulty, and very slowly, to raise his feet and to bend his knees. There was less power in the left than in the right leg. There was, he said, considerable impairment of sensation in the legs, but it was nowhere entirely absent. Tickling the soles of the feet caused no reflex movement. There was a similar condition, but infinitely less in degree, of both arms, and marked paralysis of the left facial nerve. The left orbicularis palpebrarum hung in a fold on the left upper eyelid, depressing it, and giving the appearance of ptosis; but there was no real ptosis. The left pupil was rather larger than the right; no squinting. The tongue protruded in the middle line; no paralysis of palate. Some headache, referable to no particular spot. He was unable to read for long at a time, because the lines ran together. Had considerable difficulty in swallowing, even liquids. He said: "The swallow seems to go all of one side, the same as the face, and seems to stop about the middle of the throat." A slight feeling of tightness round the chest; a dull pain in the back, and some tenderness, most marked in upper dorsal and sacral regions. About a week before admission he suffered from cramp in the calves of the legs. When he moved his legs he complained of pain up the back of the thigh, but there was no cramp of the muscles. Obstinate constipation, but no urinary affection.

There was a distinct history of syphilis, contracted probably in 1854; no history of any injury to the spine. He was in the hospital four years ago with a similar attack to this, from which he completely recovered. The present attack began a week before admission. It commenced with pins and needles in the feet, and he attributed it to his having caught cold.

This patient was treated with large doses of iodide of potassium—thirty or forty grains three times daily. He was discharged in the middle of January, after a sojourn in hospital of about two months; and at that time he could walk without difficulty about the ward, the only paraplegic symptom being a slight numbness of the legs, which was fast disappearing. The facial paralysis was slightly evident, but in all other respects the man was perfectly well.

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HOMERTON SMALL-POX ASYLUM.

HÆMORRHAGIC SMALL-POX ASSOCIATED WITH TETANUS.

(Under the care of Dr. Gayton.)

The following case presents features of an unusual character, both as regards the tetanoid symptoms and the amount and character of the eruption. It reads almost like a case of cerebro-spinal meningitis, and had it occurred at Dublin in 1866, instead of at London in 1872, it would in all probability have been so designated.

C. O——, aged seventeen, an errand-boy, was admitted on the evening of January 6th. The patient was quite well until the 3rd January, when he complained of shivering and headache. On admission, there were a few variolous papules on the face, trunk, and extremities, of a dark hæmorrhagic character, with numerous dark-purple spots on various parts of the body. He was completely unconscious; the mouth widely open; the face distorted; and the pupils dilated, the left more than the right. He was constantly shouting out, but no answer could be obtained to any question. He lay upon his left side with his legs drawn up; and when placed on his back he rested upon the occiput and the heels, the body representing a complete arch. Fluid introduced into the mouth was rejected through the nostrils. Pulse 120; lungs clear. Pressure upon the neck by pinching produced no effect. Swallowing being impossible, enemata of beef-tea &c. were given; and small doses of chloral hydrate produced slight relaxation of the muscular contraction, but otherwise did not appear to do any good. The patient remained in this condition until the afternoon of Jan. 8th, when he died.

Post-mortem examination.—On removing the calvaria, considerable difficulty was experienced owing to the firm adhesion of the dura mater. The brain was deeply congested over its whole surface, and appeared softer than natural. No fluid was found in the ventricles. On the under surface of the pons Varolii, extending along the under surface of the medulla oblongata (where it abruptly ended), was a thickish layer of concrete pus. On the posterior surface of the medulla oblongata there was a similar layer of pus, which appeared to end at the junction of the medulla oblongata with the cord, the superior two inches of which were free from exudation; but, with this exception, the posterior surface of the cord throughout its entire length was covered with an exudation like that already described. The cauda equina was infiltrated with fluid pus. The anterior surface of the cord was quite free from exudation; the cord itself was considerably congested. On examination by the microscope, numerous cells were seen; these cells were full of granules, which disappeared on the addition of acetic acid. No change was observed in the nerve-cells.

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RADCLIFFE INFIRMARY, OXFORD.

CASE OF PELVIC HÆMATOCELE.

(Under the care of Dr. Tuckwell.)

A. T——, aged thirty, admitted November 27th, 1871, with the following history and condition:—She has had four children, the youngest born two years ago, and suckled for eighteen months. Catamenia first reappeared eleven weeks ago, and have been profuse, lasting nine weeks. On the second day of their appearance she was suddenly seized, while sweeping her cottage, with severe pain in the lower part of the belly, on the left side, which was followed in a few days by the formation of a painful swelling in the same part. She was now confined to her bed, suffering from a constant bearing-down pain in the lower part of the body; from a frequent desire to pass water and difficulty in doing so; and from an irritable state of the bowels, each action being loose, but scanty and constrained. At the end of two or three weeks the painful swelling on the left side of the body subsided a little, and a similar swelling began to form on the right side, with increase of suffering and frequent vomiting. She is now in the twelfth week of her illness.

The lower two-thirds of the abdomen were distended by two large prominent swellings, occupying respectively the right and left iliac and lumbar regions, and encroaching on the hypogastrium. That on the right side was elastic, resisting, and exquisitely tender, yielding on percussion a muffled tympanitic sound; that on the left was softer and more tolerant of pressure, and loudly tympanitic. Directly above the pubes, and extending into both iliac regions, more to the left than to the right, was a hard mass which could not be followed far with the hand, but was lost on either side beneath the more prominent swellings; it was very tender on pressure, and dull on percussion. Her pulse was frequent and feeble; her temperature 100·6° Fahr.

The uterus was found, on vaginal examination, to be pushed downwards and forwards against the pubes, its fundus anteverted by a large swelling which was situated behind the uterus, and thence extended upwards and laterally so as to fill up the greater part of the pelvis. This tumour was tense, resisting, but somewhat elastic to the touch, smooth on its surface, and very tender on pressure.

On Dec. 6th, nine days after admission, the swelling on the left side had subsided; that on the right was smaller, though still very tender. Her pulse had fallen from 120 to 80; her temperature was 98°. On vaginal examination, the pelvic swelling was decidedly smaller; that part of it more particularly which lay behind and to the right of the uterus was receding, so that the uterus itself was less displaced.

On Dec. 16th all pain had ceased. The mass above the pubes could not be felt. The swelling on the left side was gone, and there remained only a deep-seated resistance on the right side, where the percussion note was less full than on the left side. On vaginal examination, the uterus occupied its natural position; a small mass of induration in the position of the left ovary alone remaining of the original pelvic swelling.

She left the infirmary convalescent on Jan. 3rd, 1872. No recurrence of menstruation had taken place during the time she was under treatment.

The treatment consisted of careful nursing and dieting, the application of linseed poultices to the abdomen, and small doses of Dover's powder to relieve the frequent and irritable action of the bowels.

The following are the points which indicate that this swelling was due to an effusion of blood, and not to products of inflammation or abscess:—1. The occurrence of the attack not in connexion with the puerperal state, but on the second day of an unusually severe menstrual period. 2. The sudden onset of the pain during active muscular exertion, and the rapid way in which signs of abdominal swelling and symptoms of peritonitis followed. 3. The condition of the abdomen, where was manifest evidence of peritonitis, more extended and reaching higher than would be expected in pelvic abscess. 4. The situation of the pelvic swelling in Douglas's pouch, as well as on either side of the pelvis. Lastly, the rapidity with which so large a swelling was absorbed and removed strengthens materially the opinion originally formed as to the nature of the disease.

With regard to the seat of the blood, the effusion was here seemingly in the peritoneal cavity. This may be inferred from the extent of peritonitis and from the way in which the vaginal tumour was apparently limited below by Douglas's recto-uterine fold of peritoneum. As to the source of the hæmorrhage, it is probable that, during an excessive menstrual flow, the blood escaped into the peritoneal cavity either from a Graafian follicle of the left ovary or from an over-distended and ruptured Fallopian tube. The other causes of these pelvic hæmorrhages in women may be excluded in this case. From the birth of her last child, two years ago, there had been no return of menstruation before the period in which she was attacked, so that there can have been no fœtus-containing cyst of extra-uterine pregnancy to rupture. Nor is it likely that in so young a woman a varicose vessel in the pampiniform plexus can have burst. Neither in the legs nor in the labia was there evidence of varicose veins being present—a point to which Bernutz draws attention as furnishing a clue to diagnosis in these cases. Again, there was no closure of any normal outlet for the blood, and, therefore, no reason for suspecting any regurgitation of menstrual blood through the Fallopian tubes. The woman was menstruating freely at the very time when she was attacked. In conclusion, the distinct reference of the first pain to the left side of the abdomen in its lower part marks this as the side on which the blood was first extravasated.

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GENERAL HOSPITAL, VIENNA.

THE BRUIT DU DIABLE.

Abstract of a Clinical Lecture by Dr. A. Duchek, *Professor of Medicine in the University of Vienna.*

There are to be found murmurs in the heart and the veins which do not take their origin in organic changes of the heart and the vessels—as, for instance, with men and animals exhausted by loss of blood and attendant anæmia. In the same degree as the loss of blood and the paleness of the skin proceed, these murmurs appear in the heart and the veins of the throat. If we touch, in severe cases, the vena jugularis with the fingers, we perceive a continued vibration, like that of a string. This vibration is not interrupted, as we observe it in arteries, but continued. On pressure on the upper part of the vena jugularis, both the perception of the vibration and the acoustic evidence of a murmur disappear, proving it to be merely venous.

The formation of the sounds in the heart is influenced by two agents: the condition of the valves of the heart and the vessels on the one hand, and by the pressure produced by the blood-stream upon them on the other. As we have, in such cases of anæmia, no symptoms permitting us to diagnose changes in the vibrating mediums, we are induced to seek the origin in the cause itself which produces vibration—namely, the pressure of the blood. In disorders attended by these murmurs (as anæmia, chlorosis, loss of blood), the propulsive power of the heart is unchanged, though its contractions are more numerous, while the blood, either in its quantity or quality, is lessened, and consequently the pressure exercised by it in the circulation is diminished too, so that the impulse given by the blood-column is not sufficient to develop in the valves or the arterial walls the amount of tension necessary for the formation of the sound, which under normal circumstances makes its appearance in these vibrating mediums, but in these cases is lowered into the acoustic impression of a murmur.

In this way we explain the murmur in the heart. It is more difficult to find its cause in the vena jugularis. Some have attributed it to the arteries; but this is contradicted by its disappearance on pressure on the jugular vein, and by its continuity. This bruit is modified by two causes. It grows stronger at regular intervals, corresponding to the pulse, being strengthened by the sound of the carotids; we call these simply moments of reinforcement. The second cause is the respiration, which, when accelerated, influences the murmur.

What is the origin of this murmur? In the heart we explain all sounds and murmurs by vibrating membranes. We shall not suppose that in the veins a whirl of blood is their cause, nor the pushing of the blood against a narrow entrance. The jugular vein behind the insertion of the sterno-cleido-mastoideus is wider than elsewhere, forming the bulbus. Beneath this bulbus are valves in the narrowest part of the vein. These valves have a most important influence upon the whole circulation of blood. The pressure in the thorax being too high, they approach one another, and in this way oppose the further entrance of blood into the thorax. The part above them must be, consequently, dilated, as we remark it in cases of stagnation in the heart, the jugular vein being distended, and the jugulum growing gradually more shallow. The impulse of the blood makes these valvulæ vibrate, and causes the murmur. When the blood flows slowly the impulse is too weak, and no murmur is to be heard, just as a slight touch is not sufficient to make a string vibrate audibly. The murmur rises when the valvulæ are half opened and put into vibration by a sufficient rush of blood. Another peculiarity is to be mentioned. The bulbus is attached to the clavicula behind the articulatio sterno-clavicularis; and thus being stretched out, it facilitates the flow of the blood. If the pressure in the thorax increases, by valvular failures, emphysema of the lungs, &c., the stream flows slowly, and therefore we do not find these murmurs attending disorders of the intrathoracic organs, and hence the general view that this bruit excludes insufficiency of the vulvula mitralis. Another consequence of this fact is, that these murmurs are not to be heard in persons affected by anæmia when they become the subjects of pneumonia, or exudative pleurisy, and that they reappear at recovery. The two necessary requisites, then, are a speedy circulation of blood and a normal pressure of the blood in the thorax. This view affords us also an explanation of the murmur growing stronger when the respiration is more hurried. The thorax being powerfully dilated, the blood rushes in strong streams towards it, strengthening the acoustic impression. When the respiration is impeded, when it must be forced, the circulation loses the necessary quickness, and the vibration of the valvulæ becomes inaudible.

These murmurs are very rarely to be found along with mechanical changes in the heart. They accompany similar murmurs at the ostia, which are always systolic, never diastolic. They may be perhaps diastolic, but only in the most severe disorders of this kind, where it is impossible to distinguish whether there is only one continued murmur or whether the systolic and diastolic murmur meet and run into one another.

Medical Societies.

APRIL—MAY.

MEDICAL SOCIETY OF LONDON.

Dr. Andrew Clark, President, in the Chair.

Dr. Routh read a communication on the Contagiousness of Typhoid Fever. The year before last a lady and her maid were travelling on the continent; the maid suffered from a mild attack of typhoid fever. On her return to England the lady (aged forty-two) took the infection, and had a characteristic attack of the same disease, her case being a severe one. The house was good, and the drains were all sound. Disinfectants were freely used. Four members of the family were attacked: the governess and the eldest girl, who slept in the room with her; the housemaid and cook, who slept together. He contended that the typhoid was imported from the continent and disseminated in this country, as there was no evidence of fresh infection.

A vigorous discussion took place, in which Dr. Thorowgood, Dr. Broadbent, Dr. B. W. Richardson, Mr. de Méric, Mr. Brudenell Carter, Mr. Hainworth, and Mr. Rogers Harrison took part.

Dr. Richardson said that he had seen a great deal of typhoid fever in Glasgow many years ago, and had never seen a case communicated. Dr. Barker, of Bedford, had a great many cases—as many as eighty. He treated them out of doors in temporary sheds. Dr. Richardson saw forty with him. It was true that one person visiting another had taken the fever, but then he had eaten and drunk there. The fever emanated from the main sewer. The authorities do right in considering the case of H. R. H. the Prince of Wales non-infectious.

Mr. Thomas Bryant made a communication and exhibited drawings illustrative of the process of cell-growth in the operation of skin-grafting. M. Reverdin, when he made his communication on this important subject of skin-grafting, propounded the following questions:—1. Was the skin graft an excitor of skin action? 2. Or were the cells themselves of the graft proliferated? Mr. Bryant contended that the grafts grew. The skin is prolonged from the graft; the borders also of the wound throw out growth. A white man was suffering from a large ulcer on the leg; a negro being in the hospital at the time, a small portion of his skin was transplanted; the ulcer decreased in size, and a piece of black skin, the size of a grain of wheat, increased very much in size. A little piece of black skin, which was well isolated, has assimilated to the white skin somewhat in appearance, having become paler in color. It was an interesting question whether the black skin would become paler or white. The cicatrix in the black man was darker than the surrounding skin. The author thought the practice of transplanting from one person to another was objectionable; it was better to take the grafts from the patient, even in children. Some skin taken from an amputated leg was placed in warm water and used for grafting. The grafts so obtained lived, but little more; they did not assimilate with the tissues around, but remained mere grafts. It might be possible to inoculate cancer or syphilis. In a case of extraversion of the bladder, a skin graft lived, but nothing more; the mucous membrane did not become skin. An ingenious instrument for taking grafts was shown.

Mr. Weeden Cooke narrated the particulars of the case of a young lady, aged twelve, who had suffered from a Tumour of the Tongue ever since she was two years old. The tumour appeared after the measles, and had, when it was removed a few days ago, attained the size of a large nut. It was soft and without a capsule, and seemed to be an exuberant growth of the normal tissue of the tongue. He had treated a case of tumour of the tongue some years back by means of a ligature, but the strings took fourteen days to come away, and although disinfectants were used, the discharge was very offensive. In the present instance chloroform was given, the patient gagged, the tongue drawn forward by means of a ligature, and the tumour removed by the knife. The ligature broke away, and the bleeding was excessive, coming principally from three vessels; the actual cautery at once stopped the bleeding, and the general bleeding was stopped by iced water. The wound of the tongue has almost absolutely healed.

Dr. John Chapman then read a paper on the Pathology and Treatment of Neuralgia and its Kindred Disorders. He arranged the whole class of neuralgias into six groups, as follows:—1st. Those cases in which there is apparently no complication of any kind, and in which the pain, having no obvious cause, is referred to some part of the periphery, seemingly healthy. 2nd. Those cases which, though marked by the absence of any morbid phenomena at the seat of pain, yet have an obvious or assignable eccentric cause, the pain being referred either to the seat of cause, or to some part of the periphery more or less remote from it. 3rd. Cases distinguished by morbid phenomena of the voluntary muscular system, chiefly, but not necessarily at the seat of pain. These phenomena consist of cramps or tonic spasms, twitches, jerks, and other convulsive movements. 4th. Cases presenting, besides the pain and disorderly muscular action just mentioned, some one or more of the following phenomena: pallor, coldness, and anæsthesia of the neuralgic area, partial paralysis and atrophy of the affected nerve, amaurosis, paralysis of various cerebral nerves, dilatation of the pupils, partial atrophy and enfeeblement of voluntary muscles, constipation of the bowels, deficient menstruation, certain indolent ulcers to which the epithets "neuralgic and cold" have been applied. 5th. Cases which, while presenting any of the phenomena characteristic of each of the preceding groups, are distinguished by symptoms of excessive secretion, the glands or glandular surfaces, which are excessively active, being generally seated within or near the region of pain. These symptoms comprise lachrymation, excessive nasal secretion, ptyalism, bronchorrhœa, nervous flatulence, diarrhœa, excessive secretion of urine, seminal emissions, leucorrhœa, and unilateral sweating or sweating over circumscribed areas. 6th. Cases which, while presenting some of the characteristics already described, also present symptoms of local inflammation, which manifests itself in various shapes, the peculiarity of the phenomenon in each case being dependent on the special character of the structure in which it appears. Dr. Chapman affirmed as a general therapeutical principle that those agents which can be used with the greatest effect in order to excite a sedative influence on the nervous centres, the hyperæmia of which gives rise to the pain and complications in question, are precisely those which are likely to exert the greatest remedial power. He believes that the best method of cure consists in modifying the temperature of the spinal region by the application of heat and cold. The paper concluded by a general explanation how to apply both cold and heat along the different parts of the spine, in order to produce the specifically sedative effects intended in each case, by warnings to avoid certain dangers which are incident to the practice of the method by those who, without the exercise of adequate caution and study of the principles which underlie it, presume to use it as if it were a mere empirical nostrum, and by a brief discussion of several cases exemplifying in a signal manner the efficacy of the method in question in treating neuralgia, as well as its kindred disorders, and especially in inducing sleep.

Mr. Henry Lee made a communication on "Some Cautions to be observed in the Use of Calomel Vapor Baths." Mr. Lee was the first to introduce calomel baths to the notice of the profession, and before doing so had labored to find out the cause of failure of the cinnabar and grey oxide of mercury. The cinnabar was decomposed by heat, and gave off sulphurous acid, which irritated the lungs. The grey oxide was decomposed, and, absorbing oxygen, became the binoxide, and acted more powerfully. Calomel, when used with water as he recommended, was, he thought, perfect; the skin was acted upon, and the lungs were not irritated. During the fumigation of dry calomel, hydrochloric acid was given off, which was very irritating to the lungs, but no bad results followed if water were used as well. In the case of a young woman in good health and well nourished, who was suffering from a syphilitic ulcer of the throat, twenty grains of calomel were volatilised in a teapot, and inhaled. The skin became cold and livid, the lips blue, and the pulse small. On a post-mortem (for the woman died), the lungs were emphysematous, and there was effusion into them. The other organs were healthy, and the fumigation, no doubt, caused the death; but if a little vapor of water had been present no harm would have happened. In another case, a man inhaled thirty grains of calomel from a teapot for four nights, for the cure of a hoarseness which had lasted two years, and which had resisted a great variety of treatment. The hoarseness was cured without salivation, but the man has suffered ever since from a cough and a pain in the chest. The lungs are not diseased, and no tubercle is present. With regard to the bath, Mr. Lee had not seen bad results, except from preexisting disease. He does not salivate, but gets a slight tenderness of the gums.

Dr. Richardson then read a paper on "the Science and Art of Healing Wounds." After alluding to the great diversity of opinion existing in the profession on the subject of treating wounded surfaces, he illustrated from clinical observation the results of healing, in extreme cases, by the first intention, and then he put the inquiry—Why, if in one case of the extreme kind named there could be cure by the first intention, there should not be such cure in the majority of cases—why, in short, should success by this method of cure be the exception instead of the rule? Two sets of causes stood in the way, one set remediable, the other irremediable. The remediable obstacles to recovery by the first intention were—want of care in bringing divided surfaces into perfect apposition; the two free use of water in dressing; the too prolonged exposure of wounds to the air; too much manipulation of the surface of the wound; the leaving of long ligatures within the wound; the imperfect closure of wounds from the air; the too hasty removal of dressings; and, lastly and most important of all, error of judgment on the part of the dresser in respect to the question whether there be, in the case to be treated, sufficient continuity of surface to warrant the attempt to heal by the first intention. The irremediable causes preventing healing by the first intention were—nervous lesions, influencing the vascular supply of the injured part; the accidental introduction into the wound of decomposing or other foreign matter, or the generation of organic poisonous product within the body. These causes lie apart from the science and art of dressing wounds, and may be left for a special and future discussion. For the promotion of healing by the first intention, the author contended that the dressing employed should have four distinct qualities: it should be colloidal, elastic, impermeable to air, and styptic. Healing by the second intention formed the next part of Dr. Richardson's communication. Here again the rules named in relation to healing by the first intention were considered, and it was maintained that the same colloidal dressing was as effective in curing by the secondary as by the primary process, the difference of application being that the whole of the exposed surface was to be treated with the colloidal fluid. After giving a series of cases of rapid healing by the second intention, cases in some of which bone had been exposed, and even a joint laid open, and after touching briefly on the addition of iodine to colloidal solution in some particular cases, the author placed before the Society a summary of his argument in a series of short propositions, and concluded by insisting that medical men live to cure all they can, and to try to cure everything until they succeed, through whatever tribulation they may pass in the labor.

In the discussion which followed, Mr. W. Adams said that he considered himself under great obligation to Dr. Richardson for attempting to lay down definite and fixed rules for the management of wounds. He had followed out his plan of treatment in many cases with good results. He amputated the foot of a young man, sawing through the bone; recovery took place in fourteen days.

Mr. Bryant said that the most important point in the treatment of wounds was to determine whether first or second adhesion was to be looked for. Torsion of arteries was most remarkable in producing healing by the first intention. Pressure and adaptation of surfaces were most important.

PATHOLOGICAL SOCIETY OF LONDON.

Mr. John Hilton, President, in the Chair.

Dr. Payne exhibited a large Hæmatoma among the muscles of the thigh. The patient, a boy aged nineteen, who was the subject of heart disease, was troubled with swelling of the right leg, and subsequently a large tumour developed on the front of the thigh. At the post-mortem the tumour was found to consist entirely of blood-clot. It had undergone the same series of changes as the clot in a vessel. The outside was membranous, while the inner parts were white and crumbling.

In answer to Mr. Hulke, Dr. Peacock (who had had the care of the patient) stated that the patient was the subject of aortic obstructive and mitral regurgitant disease. There were clots also in the left ventricle and in the left auricular appendix. The kidneys were extensively diseased.

Dr. Murchison inquired as to the condition of the muscular fibre round the clot. These hæmatomata were not uncommon in cases of fever, and the muscular fibre was always found to be the seat of granular degeneration.

Mr. Hulke quoted the case of a man who had had a succession of blood tumours amongst the muscles of his limbs, consequent, in every case, upon some slight muscular exertion. He also mentioned the case of a girl who had what to all appearance was a simple hæmatoma removed from her thigh. This tumour recurred in six or seven months, and the girl died of medullary cancer. This case was instructive as showing that a large extravasation may mask cancer.

Dr. Murchison, in reply to Mr. Hulke, stated that the cause of hæmatoma in fever cases was the rupture of the muscular fibres amongst which it occurred.

Mr. Henry Arnott called the attention of the Society to a case of Scrofulous Disease of the Testicle, as an appendix to the samples of scrofulous disease of the elbow-joint and scrofulous masses on the brain which he had brought before the Society on a previous occasion. The testicle in question was characteristic—large, nodulated, and indurated. Under the microscope the organ was found pervaded by a fibro-corpuscular growth almost identical in structure with the scrofulous masses from the brain. The brain-tumours had so much the appearance of gummata that he had made careful inquiries for a history of syphilis among the patient's family. There was none, but the scrofulous diathesis was apparent. He concluded by calling the attention of the Society to the close histological connexion—almost amounting to identity—that existed between the chronically inflamed, the syphilitic, the tuberculous, and the scrofulous testis.

Dr. Kelly exhibited (for Mr. Bradley, of Manchester,) a specimen of a case of Syphilitic Disease of the Pharynx and Larynx. The patient had died in the County Gaol at Manchester. He sniffed a good deal, and could swallow only with great difficulty; but, owing to old disease of the lower jaw, it had been impossible to open his mouth sufficiently to make an examination of the pharynx. He died of hæmoptysis nine months after his admission to the gaol. After death the soft palate (which was composed entirely of cicatricial tissue) was found adherent to the posterior wall of the pharynx, completely shutting off all communication with the posterior nares, excepting by a small opening in the centre of this cicatricial curtain, which had resulted apparently from the ulceration of the uvula. There was no trace of the epiglottis to be seen. There was a large cavity in the lung.

The President mentioned a similar case which had occurred at Guy's Hospital. Tracheotomy was first performed, and then—life being sustained the while by nutritive enemata—the already existing hole was gradually enlarged by incisions and dilatation. This occurred five or six years ago. The patient is still alive, and wears the tracheotomy-tube; she can eat a mutton chop without much difficulty. In these cases Mr. Hilton would always advise tracheotomy as the first step of treatment. He knew of one case where the incision of the palate was attempted before tracheotomy, and, the blood finding its way into the trachea, the patient was suffocated.

Dr. Galton exhibited a portion of Perforated Intestine from a case of typhoid fever. The main interest of the case consists in the fact that one of the perforations was through the apex of a diverticulum.

Dr. Clifford Allbutt and Dr. Wilks both agreed that perforation might, and often did, occur, not only in mild attacks of fever, but also with slight special symptoms. Dr. Wilks remarked that when death occurred from typhoid we were never surprised at finding perforation, even though there had been no grave symptoms to indicate it during life. He thought that the duration of the fever given in text-books, of twenty-one to twenty-four days, was probably too short. A duration of five weeks, or more, was not uncommon, and many of the cases of relapse, with which all were well acquainted, were probably wrongly so styled.

Dr. Clifford Allbutt exhibited a series of microscopic sections from the cerebro-spinal centres, to demonstrate the pathological anatomy of Hydrophobia. The specimens were taken from the cerebral convolutions, from the central ganglia, the medulla oblongata, and the cord. Throughout all these centres were found the same morbid conditions, but in different degrees, and these were as follows:—1. Evidences of great vascular congestion with transudation into the surrounding tissues. In all the grey centres the vessels were seen in various degrees of distension, their walls in many cases being obviously thickened, and here and there were seen patches of nuclear proliferation. There was a diminished consistence of some of the parts, particularly of the medulla. This seemed to be due to serous infiltration and soddening. 2. Hæmorrhages of various size, and in many places a refracting material was visible outside the vessels, due apparently to coagulated fibrinous exudate. 3. Little gaps caused by the disappearance of nerve-strands which had passed through the granular disintegration of Clarke. In addition to these appearances in the nervous centres, an enlarged spleen had been found in both cases. The parts seemed to be affected in the following order as regards severity:—(1) medulla, (2) the cord, (3) cerebral convolutions, and (4) central ganglia. This was in accordance with the symptoms during life—viz., 1. Reflex irritability in the region of the medulla with no tetanic spasms. 2. Increasing irritability throughout the cord with semi-tetanus. 3. Delirium.

Mr. Hulke inquired as to the condition of the kidneys in these cases. In two cases of hydrophobia which had come under his observation the kidneys had been acutely inflamed. The affection of the kidneys in these cases, and the enlargement of the spleen in Dr. Allbutt's cases, pointed to blood-poisoning as the cause of the symptoms.

Dr. Clifford Allbutt also exhibited a specimen showing Syphilitic Disease of the Small Arteries of the Brain. There were masses in the brain rather harder and rougher than the rest of the brain-tissue, and in these masses the arterioles were found enormously thickened, and the perivascular canals infected with gummatous matter.

Dr. Greenhow exhibited a Tumour which had been found "free" in the peritoneal cavity of a female patient who had died in the Middlesex Hospital. The tumour was ovoid in shape, rather bigger than a hen's egg, smooth and polished on the surface as a billiard ball, and of a drab color. There was no stalk or hilus to show that it had ever been attached anywhere. When cut open it showed an arrangement of concentric laminæ, and under the microscope it appeared to be composed simply of fibres running nearly parallel, and occasionally branching and dividing. No vessels were anywhere to be seen, and in the centre was a small calcareous nucleus. The tumour had been found in a sort of pouch, which it had apparently formed for itself close to the cardiac end of the stomach, from which, when the abdomen was opened, it "shelled out." Two other similar but smaller tumours were found, one of which was attached to the omentum and contained no nucleus. A similar tumour had been exhibited by Mr. Shaw, and described in the Sixth Volume of the Society's Transactions, which had been removed by him from a hernial sac, whither it had travelled and prevented the reduction of the hernia. Mr. Shaw cut down upon the tumour, and "it rolled down between the patient's legs like a billiard ball."

Mr. John Wood had (in conjunction with Dr. Wilks) examined Mr. Shaw's specimen. They were of opinion that in many instances these tumours owed their origin to the appendices epiploicæ, which, becoming detached, and undergoing calcareous degeneration, formed the nuclei. With regard to the subsequent development, there was a difference of opinion between them. Dr. Wilks thought it was due to the gradual growth of fibrous tissue around the nucleus, but Mr. Wood was of opinion that layers of fibrin were simply deposited around the nucleus from the peritoneal fluid. He had met with two such tumours occurring in hernial sacs. He had diagnosed their nature, and managed to reduce them. Although these bodies were quite free in the peritoneum, they nevertheless set up no peritonitis. He was convinced in his own mind that his theory as to their origin and growth was correct.

Dr. Cayley remarked that the tumour in question was covered with a layer of peritoneum, which could be peeled off. It could not, therefore, have been formed free in the peritoneal cavity. The laminæ were composed of fibrous tissue.

Mr. Gay had found smooth laminated tumours in the veins just like the one before the Society. The tumours to which he alluded were quite distinct from phleboliths.

Mr. Hulke considered these tumours to be subperitoneal formations. He agreed with Virchow's theory, that they were formed from the appendices, the stalks of which drag out till the vessels are obliterated and the appendices drop off.

Mr. Arnott agreed with Mr. Wood, because neither nuclei nor vessels were visible in the fibrous tissue.

Mr. Lawson narrated the case of a patient whose thigh had been amputated in 1867 for a large blood-cyst occurring close to the popliteal space. The patient returned in June, 1871, with pain in the stump, which was followed by the development of another blood-cyst at its extremity. Death occurred somewhat suddenly with symptoms of dyspnœa. After death cancer was found invading the right lung, and a blood-cyst was found in the stump containing cancer-cells in its interior.

Mr. Lawson also exhibited a tumour growing from the choroid coat of the eye. The patient had had symptoms of glaucoma. Iridectomy had done no good, and the eye was ultimately removed. The specimen was mounted in glycerine jelly, to which a little creasote had been added.

Mr. Norton exhibited a growth from the femur, for which the patient's thigh had been amputated. The tumour was encapsuled, and did not invade the substance of the bone. Microscopically it resembled enchondroma.

Dr. Greenhow exhibited some Lungs containing several large cavities, each of which was quite smooth in the interior and lined with false membranes, indicating that the condition which had given rise to them had been completely arrested. The patient died of capillary bronchitis. At the same time Dr. Greenhow exhibited a lung, taken from a patient who had suffered from repeated attacks of pleurisy, in which hardly any lung-tissue, probably so-called, remained. The lung was contracted, tough, and airless.

Dr. Liveing exhibited a case of Syphilitic Cirrhosis and Amyloid Disease of the Liver and Kidneys. The patient, a young man aged twenty-one, had died exhausted from phthisis and diarrhœa of a year's duration. At the post-mortem examination the bones of the calvaria were found extensively diseased. The liver and kidneys were found in the condition above mentioned, large, smooth, and puckered; and several ulcers were seen on the mucous surface of the large intestine. During life the urine had been abundant and pale, and contained granular casts. Niemeyer states in his text-book that in cases of albuminoid disease of the kidneys the urine is scanty and dark-colored.

Dr. Moxon stated that he had seen dark-colored urine with this condition of kidney.

Dr. Gowers exhibited a case of Cancer of the Liver. The liver had been removed from a young female patient in the Hospital for the Paralysed and Epileptic. She had been admitted for symptoms of nerve-pressure—pain, loss of power, and wasting of the muscles—in the left leg. She had also a large tumour of the liver, which presented during life mainly the features of cystic disease. It had been growing about four months. At the post-mortem the liver was found enormously enlarged, and uniformly infiltrated with remarkably soft encephaloid cancer. In places there were large, soft swellings which gave to the hand the feeling of fluctuation. There were also one or two regular cysts in the liver. Some of the lumbar glands were found infiltrated with encephaloid cancer, and nodules of cancer were also found in both the breasts.

Mr. Jonathan Hutchinson exhibited some small Tumours which had grown on the leg of a healthy woman, just below the knee, between the bones. They had been growing for six months, and at first were supposed to be syphilitic, and presented, indeed, all the characters of a syphilitic node in the muscles. They failed to yield to iodide of potassium, and as they showed signs of extension, the leg was amputated. Distinct masses were found infiltrating two or three muscles, and the bones in the neighborhood were apparently inflamed. There was a large infiltration of deposit in the tibialis anticus, and another smaller and more circumscribed in the peroneus longus. Microscopical examination had as yet thrown little light upon their nature.

Mr. Sidney Jones remarked that a similar case had occurred some years ago among Mr. South's patients; growths similar to those under investigation having invaded the scapular muscles.

Dr. King exhibited a case of Aneurism of the ascending part of the Arch of the Aorta which had been punctured by the sharp end of a fractured rib, and had thus caused the patient's death. There was considerable erosion of the upper part of the sternum and the first rib, and the latter had been broken some months before the patient's death by a blow. The upper part of the broken ends of the rib had united, but at the lower part a sharp end of the outer fragment had pierced the aneurismal sac. At the post-mortem examination, plugging of the middle cerebral artery, with softening of the left corpus striatum and a portion of the left hemisphere of the brain, was also found.

Mr. Jabez Hogg exhibited a specimen of Madura Foot. The patient from whom the mass had been removed was a young man aged eighteen. Some years previously he had struck his foot against a sharp stone, and six months ago he was admitted to the hospital with a small abscess on the sole of the foot, at the seat of the former injury. On changing the poultice one day several of the dark sooty truffle-like bodies were found; the disease was diagnosed, and the diseased portion was excised. Mr. Hogg had examined these sooty bodies, and was sure that they were the result, and not the cause, of the disease. They were algoid in structure, and in no way resembled the mycelium of the truffle.

Dr. Moxon exhibited a case of Subacute Yellow Atrophy of the Liver. From an examination of the case he had come to the conclusion that acute yellow atrophy was the acute form of a chronic condition. The patient was a woman aged thirty-four. She was admitted to the hospital with jaundice and slight enlargement of the liver. For the first five weeks there was little change in her condition, but at the end of that time all the symptoms of acute yellow atrophy supervened, and she rapidly died. The liver presented the appearance of two distinct kinds of change—the one a light rhubarb yellow, the other a dark red.

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CLINICAL SOCIETY OF LONDON.

DR. GULL, President, in the Chair.

MR. BRUDENELL CARTER then proceeded to read a paper on a case of Paralysis of the External Rectus Muscle treated by tenotomy and localised faradisation. The patient was thirty-three years of age, apparently in good health, with no history of syphilis or of any disorder of the nervous centres. She applied at St. George's Hospital on May 16th, 1870. The left eye was then so much rolled inwards that only the outer third of the cornea was visible, and was almost immovable in this position. The patient had been in the same condition for more than five months, and she ascribed her malady to some rough usage from her husband. She was ordered to take thirty grains of iodide of potassium daily, and this was continued for nine weeks; at first with very slight benefit, which reached its limit at the end of three weeks. On July 19th the tendon of the left internal rectus was divided, and localised faradisation with the primary current of a Stöhrer's battery was afterwards regularly applied to the external rectus twice or three times a week, by a pair of small rheophores designed for the purpose and exhibited to the Society. Under this treatment the muscle gained sufficient strength to roll the eye to the middle of the palpebral opening when the other eye was closed. On November 22nd, no further progress having been made, both internal rectitendons were divided, with the result of completing the cure. The eyes were movable to the normal extent in all directions, and retained their parallelism in all positions, no trace of any squint remaining. Mr. Carter said that he had been led to devise this method of treatment in consequence of the very imperfect results obtained from operations for shortening or readjustment of the paralysed muscle. The patient was introduced to the Society.

Dr. J. W. Ogle thought that the interesting feature of the case was the question as to the cause of the paralysis. Paralysis of the external rectus was not uncommon from the most trifling causes, as, for example, indigestion. It might occur (in the female) during suckling.

Mr. Holthouse remarked on the perfect recovery of contractile power in the external rectus, as evidenced by the complete motor power which the patient had over the eye. Had the plan of cutting a piece out of the paralysed muscle been pursued he could understand that the shortening of the muscle so brought about might have the effect of pulling the eyeball to its normal position; but that procedure could not, as in this case, have any effect in restoring the motor power of the muscle.

Mr. Lawson, *apropos* of the pathology of the paralysis, remarked that syphilis was a frequent cause of paralysis of the muscles of the eyeballs, but paralysis of the external rectus was more often due to trifling reflex causes than was the case with the other muscles, because of the intimate relations which existed between the sixth and the sympathetic nerve.

Dr. Althaus agreed with Mr. Lawson. In his experience paralysis of the external rectus was often due to cold. He had often applied faradisation for the relief of cases like the one under discussion.

Dr. Glover asked if Mr. Carter had used the subcutaneous injection of strychnine in cases like this.

Mr. Carter, in reply, said that he himself had been in much doubt as to the cause of the paralysis. He would remark that it was of five months' duration, and therefore could not be looked upon as due to any passing cause. Further, as the patient had taken large doses of iodide of potassium for nine weeks without relief, he considered that syphilis was out of the question. In applying the faradisation, he had followed the advice of Duchenne, and had gone as close to the affected muscle as possible. He had never employed hypodermic injections of strychnine.

Dr. John W. Ogle read a paper "On the Temperature in Certain Affections of the Nervous System, and especially in Tetanus." Dr. Ogle first alluded to a case of traumatic tetanus which he had described to the Clinical Society the previous session (which had recovered under the use of chloral, belladonna, wine, ice to the spine, &c.), and which had exhibited tolerably regular or periodic daily variations of a remarkable kind in the heat of the body—variations not corresponding by any means with pyrexia, with any rise in the frequency of respiration, or of the pulse, and having no apparent relation to the food taken, the degree of sweating or the amount or force of muscular spasm. He then described other cases of tetanus in which the evening temperature had been found to be remarkably high compared with that of the morning. Of these, one was a case which he had received from Dr. Keen, of Philadelphia, in which, out of fifteen days that the temperature was taken, there were nine in which the evening temperature was much higher than that of the morning, being in several instances more than 1½° and in some cases 2° higher; the pulse being the while often as low as 60 per minute. A second case was one sent to him by Mr. Dixon, of Preston, in which during six days that the temperature was taken, there was a great evening increase of temperature over that of the morning, being on one occasion nearly 3° higher. A third case was one recently published by Mr. Poland; and a fourth one published by Mr. Croft. Dr. Ogle did not consider that the great nightly exaltation of temperature was attributable to remedial agents used, though he recognised the fact that certain agents notably affected temperature, and quoted numerous instances in which it had been demonstrated experimentally both on man and animals, that temperature could be both increased and diminished, instancing (and giving authorities) the decrease of temperature from chloral, bromide of potassium, quinine, digitalis, alcohol, mercury, lead, &c., and quoting experiments in which nicotine, curare, strychnia, vomica, veratria, quinine, and phosphorus were injected into the veins of animals. Examination of the results of such experiments and trials showed very great diversity and discrepancy, and Dr. Ogle suggested that research into the action of agents (both in health and disease) in modifying temperature might well and fitly be undertaken by the Clinical Society, and would prove a field worthy in every way of their cultivation. He alluded to the observations which his friend Dr. Fox, of Clifton, was engaged in on this subject. Referring to the modifications of temperature of tetanus, and to the possibility of the existence of variations in these modifications, according to the character and nature of the tetanic attack, Dr. Ogle remarked that it would be a question whether such variations have any relation to vascular changes in the spinal cord, or to such textural changes in the central nervous system as have been observed in certain fatal cases of this disease by Rokitansky, Wedl, Wagner, Demme, Fleckner, Eisenmann, Oppolzer, Wunderlich, and, more recently, Clark and Dickinson. Dr. Ogle said he thought it was a matter of consideration how far such histological changes were due to vascular congestions or to increased blood-temperature induced in the course of the disease. In connexion with the high temperature of tetanus, he alluded to the views which had lately been promulgated, especially in Germany, as to the condition termed fever—a condition which was supposed to result from an unwonted state of the central nervous system, by Wunderlich, Virchow, and many others. He specially drew attention to the researches of Tscheschichin, who looked on fever as the result or a morbidly increased activity of the spinal centres in consequence of a weakening or paralysis of the moderating portions of the brain, by which a number of chemical processes are increased to an extent which is never attained under normal conditions of the functions of the brain. He observed that, whatever part the central portions of the nervous system may play in the increase of temperature in disease, we must of course allow that the vasal nerves, as well those which contract the vessels as those which actively dilate them (if such there be), are intimately concerned, and that an arrest of the natural loss of heat by radiation may lead, by causing its accumulation, to an increase of general temperature as much as an actually and positively increased production of heat. Dr. Ogle then alluded to other cases ordinarily looked on as non-pyrexial, in which the evening temperature was very much higher than that of the morning; for example, those of diabetes, published by Dr. Foster, of Birmingham. (The examination of temperature in cases of diabetes by himself had given different results). It was to be remembered that in many cases of diabetes the central nerve-structures were at fault. He alluded to cases of meningitis in which the exceedingly high temperature was noted; one noticed by Dr. Bradbury, of Cambridge; also to cases of general paralysis of the insane, for which he was indebted to Dr. Mickle, of the Derby Asylum; and finally, to a probably exceptional case of chorea which he had lately treated, and in which the temperature for twenty-one days was noted morning and evening, the latter being almost always much higher than the former. Dr. Ogle referred to the great importance which the thermometer, as supplying a measure of expenditure of nerve-force and tissue-waste, was assuming in our clinical wards, thus competing with the stethoscope, microscope, and test-tube. He hoped shortly to have the opportunity of offering to the Society some remarks on the temperature of the body in paralysis.

In the discussion which followed,

Dr. Althaus observed that his countryman, Dr. Hermann Weber, had recorded a case in which a great rise of temperature had followed an injury to the cervical spine. It would be interesting to hear if, in the cases brought before the Society by Dr. Ogle, the cervical portions of the cord were particularly affected.

Dr. Hermann Weber had observed the temperature in one case of tetanus very carefully. He had particularly observed that the temperature underwent a regular diurnal range, being highest in the evening, and that there was no relation between the administration of medicine and the rise in temperature. The amount of spasm in this case seemed to be greatest during the night. There was no relation between the amount of spasm and the temperature.

The President agreed with a remark that had been made (by Dr. Barclay) that the use of the thermometer for clinical purposes was perhaps rather excessive at the present time. He mentioned a case of fracture of the fifth cervical vertebra which had come under his notice, in which the temperature on the exposed parts of the abdomen was observed immediately after the injury to be 109° F.

Mr. Cooper Forster read two cases of Popliteal Aneurism: one of a man at Guy's Hospital, in which for two months pressure, with and without chloroform and flexion, were used, with, at last, a cure of the disease; and the other of a gentleman, in which digital compression was used for three hours and a half, and at the end of that time complete cure was affected. Mr. Forster believed the latter case to show the shortest time on record in which a cure had been affected by any means. The first patient pointed to the necessity of perseverance in the treatment to ensure success. Mr. Forster did not bring these cases forward as any novelty, but rather with the idea of eliciting from surgeons their views as to the advisability or otherwise of persevering in the plan of compression or flexion without the use of the knife. He alluded to the difference in the Scotch and Irish schools; and, though by no means a follower implicitly of either one or the other, he still believed that, in cases of popliteal aneurism, if the surgeon has confidence in his treatment, the cases must be very few in which compression would not be found sufficient for the cure of this disease; and stated that he had never yet tied the femoral artery for the cure of this form of aneurism, though once or twice the tediousness of the recovery of the patient almost induced him to do so.

Mr. Ernest Hart remarked that these cases indicated some very important points in surgical pathology. The first efforts in the ways of curing aneurisms were directed to obtaining complete closure of the artery. All plans adopted, however, caused an intolerable amount of pain, and so the Irish surgeons adopted means to slacken the current of blood through the vessel, and so to favor the deposit of fibrin. Mr. Hart believed that the rapid mode was the best; that hence chloroform was a most valuable ally, and that numberless cases are records of cures effected rapidly, in which the excessive pain given has been disregarded

Mr. De Morgan had been led to modify his ideas as to the treatment of aneurisms in consequence of the great variety of results that followed digital compression. He quoted a case that had been under the care of the late Mr. Moore at the Middlesex Hospital, in which digital compression was adopted for aneurism of the ulnar artery, and the manipulation very thoroughly performed. The patient died afterwards from renal disease; no deposition of fibrin was found, and, strange to say, no arterial outlet was discovered.

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OBSTETRICAL SOCIETY OF LONDON.

Dr. Braxton Hicks, F.R.S., President, in the Chair.

Dr. Alfred Meadows related a case of so-called Ventral or Abdominal Pregnancy. The patient was twenty-two years of age, and was admitted under his care into the Hospital for Women. The abdomen was enlarged to at least the size of pregnancy at full term, though she had only arrived at about the sixth and a half month. The fœtal heart could be heard. At the end of a week symptoms of collapse supervened from internal hæmorrhage, and the patient died. The fœtus was found free in the abdominal cavity, its only attachment being by the umbilical cord to a kind of placental mass formed at the fimbriated extremity of the Fallopian tube. Dr. Meadows advocated a more frequent resort to gastrotomy in such cases, not so much with a view of saving the child, or of rescuing the patient when collapse had supervened, but in order to anticipate that collapse, and, in fact, to remove the growth as soon as we could arrive at a true diagnosis. He submitted that possibly, in cases such as he had related, it might not be necessary to remove the placental mass, as this might gradually be absorbed by a slow physiological atrophy after that for which it existed was removed.

Dr. Protheroe Smith said there was no doubt in his mind about the propriety of performing gastrotomy in such cases, but a difficulty suggested itself in the previous diagnosis.

Dr. Graily Hewitt believed that the operation of opening the abdomen ought to be performed when hæmorrhage from rupture of an extra-uterine pregnancy threatened to be fatal; but the great difficulty was the diagnosis. With reference to Dr. Meadows's proposal to treat such cases apart from hæmorrhage by such an operation, that was another matter, and, in considering it, it should be borne in mind that a certain proportion of supposed Fallopian pregnancies, as stated by Kussmaul, were really cases of pregnancy in a bicorned uterus, and such cases often did well if left to themselves.

Mr. Spencer Wells said that the propriety of operating when a woman was dying of bleeding into the peritoneal cavity, or of operating when her life was not in immediate danger, although extra-uterine fœtation had been ascertained to exist, were very different questions. In the one case it might be the clear duty of the surgeon to try to save a patient at any risk from inevitable death. In the other case he would give full weight to the consideration that a spontaneous termination of extra-uterine fœtation was not very uncommon. The structure might remain in the body harmless for many years, or pass away through the rectum, the vagina, or the abdominal wall.

Dr. Greenhalgh gave brief particulars of seven cases of extra-uterine fœtation: three recoveries and four deaths. Of the former, in one the fœtal bones were discharged through an abscess in the left groin, in another a full-grown fœtus in an advanced stage of decomposition was extracted through an incision in the roof of the vagina, and in the third the liquor amnii was drawn off by a small trocar and canula. Dr. Greenhalgh said that, owing to the great fatality of such cases, he was of opinion that gastrotomy might, in many cases, give the patient a better chance than by permitting the development of the fœtus and secundines to progress, leading to the risk of rupture, and probably fatal internal hæmorrhage or other grave termination.

The President said his opinion, based on many cases, was, that a number recovered from severe internal hæmorrhage, while a greater number gave no serious anxiety to the patient or medical attendant. If, then, these cases were not so fatal, he should prefer not to interfere till urgent symptoms arose. Practically the opportunity for such an operation as that suggested seldom presented itself. The records of cases in the Society's Transactions showed the exceeding difficulty which would have attended the removal by gastrotomy.

Dr. Playfair thought the difficulty of diagnosis at an early stage almost insuperable.

Dr. Brunton detailed a case of fibrous enlargement of the uterus successfully treated by ergot of rye.

Dr. Tilt said that, in addition to ordinary cases of uterine fibroid tumours, he had met with two cases in which the womb had become fibrous in its totality. He could not believe that large doses of ergot would have prevented the increase in a case he narrated.

Dr. Playfair said that this case could scarcely be claimed as one of the class he had spoken of in his paper "On the Spontaneous Absorption of Fibroid Tumours of the Uterus." This case seemed to be a case of general fibroid enlargement of the whole organ, a pathological condition with which he was not familiar. He did not state that fibroids were absorbed in consequence of medical treatment, but that in exceptional cases fibroids disappeared spontaneously. The process was probably similar to the involution of the uterus after delivery.

Dr. Playfair read a paper on the Treatment of Empyema in Children. The author described the peculiarities of pleurisy in children as contradistinguished from the same disease in the adult. He then referred to the change of opinion which had of late years been observed with regard to the operation of paracentesis. This operation in ordinary serous pleurisy evidently stood on a very different footing from the same operation in empyema. In the former we only sought to relieve the distension by removing some of the fluid and allowing the remainder to be more readily absorbed, while in the latter the chance of absorption was diminished to a minimum; and it would be a great gain if we could effect continuous drainage of the pleural cavity, and at the same time effectually exclude the entrance of air. The author then described the method of drainage by Chassaignac's tube, with illustrative cases. He then described the method of cutaneous subaqueous drainage advocated by himself, and related the history of three cases successfully treated by it. The results of these were very satisfactory, and contrasted remarkably with the cases treated by pneumatic aspiration by Bouchut. The paper was illustrated by drawings of the chests of five children, taken by Dr. Gee's cystometer, which showed how much greater the chest deformity was when air had been allowed freely to enter the pleural cavity.

Dr. Hilton Fagge felt bound to mention to the Society that he had recently had a case in which the same method had been employed, and with results not entirely so satisfactory. The pus had, in this instance, made its way by the side of the india-rubber tube, and continued to discharge. It was of great importance that the tube should be tightly grasped by the skin, and for this reason it was better to remove the canula before introducing the india-rubber tube, which should be of the same diameter.

Dr. F. T. Tayler mentioned a case of empyema treated in a similar manner. The chest was tapped with a syphon trocar. An india-rubber tube was affixed to the canula and carried into a basin of water. The canula and the tube were retained twenty-four hours, and then a piece of elastic catheter, with a smaller india-rubber tube, was inserted through the canula, which was withdrawn. The tube was retained for fifteen days.

Dr. Sedgwick approved very much of the plan of subaqueous paracentesis. He had himself occasionally during the last fifteen or sixteen years carried out the same principle. He had used a canula, the tube of which projected externally and much beyond the shield, on which he slipped a long india-rubber tube with the other end dipping into a dish of water. The plan of introducing the india-rubber tube into the chest was much better than leaving the canula in. Dr. Sedgwick had adopted the same plan in a case of paracentesis abdominis, where the patient was very weak and the abdominal walls were as thin almost as parchment. The rapidity of the flow was entirely under control by means of pressure on the tube.

Editorial.

CONGESTIONS CONSECUTIVE TO THE LIGATURE OF ARTERIES.

M. Brown-Sequard appends a note to one of the recently published parts of the *Archives de Physiologie*, discussing the Congestions that occur consecutive to the Ligature of Arteries—a subject which has received little attention, but which seems to have sufficient practical bearing to render it worthy of consideration. He states that in December, 1868, M. Prompt announced to the Société de

Biologie that the kidney became hyperæmic when the renal artery was tied. M. Moreau, in the discussion that followed this communication, stated that he had obtained similar results in the case of the spleen and intestine after the ligature of the splenic and mesenteric arteries. More recently, M. Teillais, in a treatise on Simple Chronic Ulcer of the Duodenum, reports that he ligatured the vena porta of a dog, and ascertained that two hours and a half afterwards the liver was in a state of great congestion. M. Brown-Séquard's experiments made on dogs, rabbits, and guinea-pigs are in full accordance with these statements, and show that, even when all or nearly all the arterial supply of the kidney, the spleen, or a portion of the intestine, has been cut off, the district supplied by the arteries ligatured become remarkably congested, and he has obtained the same result in the case of the liver on tying the portal vein.

These facts, M. Brown-Séquard remarks, are analogous to many others that are well known—such, for example, as the flow of blood from the inferior portion of a ligatured artery, if opened on the distal side of the ligature, and the elevation of temperature that is often observed to take place after the ligature of the principal arterial trunk of a limb. In the case of the limbs he is inclined to advance the same theory in explanation of all these phenomena. It is to the effect that the application of the ligature paralyses the vaso-motor nerve-filaments that ramify on or in the coats of the artery tied, producing a corresponding paralysis of the ramifications of the vessel; as a consequence of which the blood of the *collateral* vessels, finding a passage widely open for it in the parts supplied by the ligature, flows thitherwards, and produces both the congestion and the concomitant elevation of temperature. In the case of the kidney and of the spleen, however, another explanation must be offered. In both cases the arterial supply may be *entirely* cut off, yet the kidney, if watched after the operation, may be actually seen to swell; and if the renal artery between the ligature and the kidney be divided, hæmorrhage occurs, which M. Brown-Séquard states lasted during the eight or ten minutes he permitted the animal to live. It is obvious that a *reflux* of blood must here have taken place; and this is due to the fact that the blood throughout the body is subject to considerable pressure during life, occasioned by the elasticity and contractility of the vessels; but if a vaso-motor paralysis exists in any organ—as the kidney, for example,—and if the *vis a tergo* of the blood have ceased in consequence of the interruption of the blood-current in the arteries, the diminution of resistance in the vessels of this organ will cause the blood to regurgitate thither through the veins, and produce congestion.

M. Brown-Séquard remarks in passing, that those experimenters who believe that they have demonstrated that the heart can continue to beat in spite of the stoppage of the circulation through its vessels consequent on ligature of the cardiac arteries, have not taken into consideration the fact that a venous reflux occurs, which fills the vessels of the heart in consequence of the vaso-motor paralysis caused by the application of the ligature.

However this may be, a difference is thus shown by M. Brown-Séquard to exist between the case where a main artery of a limb is tied and that where all the vessels distributed to a definite region—as to the kidney—are ligatured. In the former case the venous reflux cannot take place in consequence of the continuation of the ordinary current of the blood from the arteries to the veins through the collateral channels; whilst in the latter case such reflux must certainly occur. In both instances, however, it may well happen that, the quantity of blood passing in a given time into the part where the vaso-motor paralysis exists being less, whilst the vessels themselves are dilated and consequently capable of containing more blood, this has time to become surcharged with carbonic acid; and we have thus one cause at least of the convulsions and other nerve troubles that are sometimes observed immediately, or almost immediately, after the ligature of one of the primitive carotids in man.

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PHYSICS OF THE EXTRUSION OF THE WHITE CORPUSCLES.

The remarkable corroboration that the statements of Waller and Cohnheim respecting the passage of the white corpuscles of the blood through the walls of the capillaries have received by physiologists generally, and the important bearing these observations have upon the question of the formation of pus and the fibrinous exudation of inflammation, confer special interest on all experiments and investigations tending to elucidate the physical principles by which the escape of these corpuscles is effected. At first sight nothing could well be conceived more improbable than that the formed or morphological elements of a fluid contained in a system of elastic tubes should be able to make their way out, whilst the limpid fluid itself should be retained. Not only is it difficult to imagine by what agency the first tendency to pierce the wall is brought about in the corpuscles in the absence of any visible holes in the membrane, but it is still more difficult to explain why the rest of the fluid does not pursue the same path. It is clear that it cannot be exclusively attributed to variations in the pressure of the fluid against the walls of the vessels caused by the action of the heart; for, as this is exerted upon the fluid as a whole, it is inconceivable that it should cause the extrusion of certain particles alone, without the operation of some other force.

An inquiry into the nature of this force has lately been undertaken by Dr. Richard Norris, of Birmingham, who has published two valuable papers upon it: one of which appears in the Proceedings of the Royal Society, the other in the Transactions of the St. Andrews Medical Graduates' Association. Dr. Norris points out that all the membranes which enter into the animal body may, from a physical point of view, be divided into two orders: the very fine structureless homogeneous films, which must be regarded as simple *cohesion membranes*, in contradistinction to the second order of coarse membranes, to which certain mechanical arrangements—as interlacing fibres—are superadded, which have the effect of increasing the strength. Films of collodion, gelatine, india-rubber, and soap may be regarded as examples of the first class of membranes; and these are susceptible of two states—the fixed or rigid condition, and the contractile or elastic state, dependent upon the presence of the principle of "flow," which principle may be operative in every shade and degree, from perfect liquidity to absolute rigidity. Dr. Norris takes the soap film as the best illustration of the class of homogeneous cohesion films possessing in the greatest perfection this principle of "flow," and as best exhibiting phenomena which he has generalised under the term "progressive cohesive attraction." He describes the production of a soap-bubble, and points out the capability it possesses, under the influence of progressive adhesion, of perfecting any absence of continuity that may exist in its structure; so that after its complete detachment it may be perforated by any wetted body, which may again be withdrawn without the bursting of the sphere. Again, he shows that if a soap-bubble be allowed to impinge on a smooth rigid surface, it becomes so drawn down to the plate as to assume a hemispherical form; but if for the rigid surface a delicate plastic film be substituted—such as may be taken up by a ring from a solution of soap,—the soap-sphere immediately *penetrates* the film, and arranges itself so that half is on one side and half on the other. If pressure be now applied to one side, the bubble can be caused to protrude still further through the film, and it may even ultimately be made to pass through the film without any rupture occurring in either.

Without entering further into Dr. Norris's ingenious experiments, it is obvious that we have here a very close similarity to the phenomena accompanying the extrusion of the white corpuscles. In both cases the conditions essential for a rigid or a plastic body to pass through a colloid film are present; and these he has shown to be—firstly, an intimate power of cohesion, either mediately or immediately, between the film and the body; secondly, a certain amount of pressure from within; thirdly, power in the substance of the film to cohere to the surface of the body during its passage; and, lastly, cohesive plasticity of the particles of the material of which the film is itself composed, so that the breach in it may again become united as it descends upon the opposite surface of the body which is being extruded. It may be asked, however, why the white corpuscles are not constantly escaping in large numbers from the vessels; to which Dr. Norris replies that as it is only under certain conditions that the soap-spheres attract each other—namely, when free liquid is cohering to their surfaces,—so with the corpuscles, which do not unite with each other or with the capillary wall unless their normal osmotic relations are disturbed, the exosmotic current setting in excessively causing their surface to become coated with previously-contained substance, when they become instantly attractive of the capillary wall.

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INFLUENCE OF NERVES ON THE CIRCULATION.

Some years ago Professor Dogiel, of Kasan, constructed an apparatus—essentially on the principle of Volkmann's hæmodromometer—by means of which the force or rapidity of the current of blood in the vessels could be estimated. By its means he was enabled to show that various circumstances effected changes in the rapidity of the blood-current in such arteries as the carotid and crural; and he gave the results of his observations on the alterations induced by division of the cervical sympathetic, vagus, and other nerves. The general conclusion at which he arrived was, that the *variations* in pressure and the rapidity of the current of the blood in the larger vessels were due not so much to changes in the vigor of the contractions of the heart as to alterations in the amount of resistance that the current experienced in the smaller vessels. In other words, that the changes were due to peripheric rather than to centric causes. In a paper contained in the last part of Pflüger's *Archiv*, Dr. Dogiel gives the results of his observations in regard to the influence of the sciatic and crural nerves on the circulation of the blood in the lower extremities. The animals employed were the dog and the frog; and the mode of experimentation was that the mean rapidity of the blood in the carotid and crural arteries and in the crural vein was ascertained; the sciatic or crural nerve was then dissected out and divided where it leaves the pelvis, yet so that both extremities could be easily subjected to the action of electric currents. The animals experimented on were either narcotised by the injection of morphia or poisoned with woorara till the voluntary muscles were completely paralysed, artificial respiration being maintained in the latter case. It was not forgotten that, under the full toxic influence of woorara, paralysis of the vaso-motor nerves takes place.

From the first series of experiments recorded, it appears that, after section of the crural or sciatic nerves and excitation of both the lower and the upper extremities of the cut nerves by electricity, a retardation of the blood-current and elevation of the blood-pressure in the crural artery occurs. Similar variations were also coincidently observed in the carotid, but not in the crural artery of the opposite leg. The retardation of the current was shown clearly not to be dependent upon the increase of the blood-pressure. But it might be due either to contraction of the small arteries of the limb under the influence of the vaso-motor nerves, which probably run in the trunk of the crural or sciatic nerves; or to the contractions induced by the electrical current in the smooth muscles of the arteries; or to the contraction of the striated musculature of the lower extremities supplied by the above nerves; or, lastly, to chemical and physical changes in the blood traversing the vessels. To determine to which of these circumstances the changes observed were owing, Dr. Dogiel instituted several other series of experiments. From these it appeared that the retardation of the current did not occur on excitation of the sciatic and crural nerves if the animal had previously been subjected to the full action of woorara or morphia, and therefore that such retardation was not due to excitation of any vaso-motor nerves causing contraction of the small arteries, and consequent obstruction of the current in the crural artery. It was easy to demonstrate also that the changes did not result from alteration in the force or frequency of the heart's action. On the whole, he believes his experiments tend to show that the retardation of the swiftness of the blood-current and the augmentation of the pressure, in healthy animals, are simply due to the contraction of the general musculature of the limb. This view is in accordance with the well-known increased flow of blood that takes place from a vein of the arm in venesection when the muscles of the limb are brought into active contraction; for each contraction of the muscles occasions not only a diminution in the lumen of the small muscular arteries which run in the transverse or longitudinal direction of the muscular fibres, but also in the large branches of the arteries; whilst, on the contrary, with dilatation and consequent retardation of the blood-current in the muscular arteries there is a reduction in the rapidity of the current in the muscular veins and in the main veins of the limb.

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INTRACRANIAL CIRCULATION.

Some time has elapsed since the publication in England of any researches on the cerebral circulation, Mr. Durham's, we believe, being the last. During the past year, however, three essays have been published in Germany by MM. Jolly, Althann, and Pagenstecher, a *résumé* of which is given in a recent number of the *Centralblatt*. M. Jolly carefully fixed a manometer into the skull of the animals he experimented on, the oscillations of the mercury or saline solution contained in which were recorded by an apparatus similar to Marey's sphygmograph. With this instrument he found that there was usually a positive pressure on the side of the brain, which rose with expiration and fell with inspiration. In dogs the oscillations amounted to from 25 to 145 millimetres of the *water*-manometer; as maxima in rabbits to from 7 to 75 mm.: but it could also become negative. The pulse caused a variation of from 5 to 10 mm. M. Jolly proceeded to examine the variations in the cerebral pressure produced by injections of a one-half per cent. chloride-of-sodium solution into the carotid under various pressures, and found that considerable modifications occurred; a rise of from 18 to 52 mm. in the cerebral pressure resulting from injection made under a pressure of 120 mm. of mercury. A maximum can, however, be obtained beyond which no further injection into the vessels causes any increase. Compression of the carotids exerts only an inconsiderable influence in diminishing the intracranial pressure; but compression of the jugular veins produces a prolonged and equable elevation of pressure, which again diminishes when the pressure is released. Excitation of sensitive nerves increases the intracranial pressure considerably; but only in consequence of increased respiratory movements, since it was very small in curarised animals, and appears to be chiefly due to slight excitation of the vaso-motor centres. Irritation of the cervical sympathetic at its cranial extremity caused on several occasions distinct augmentation of the intracranial pressure, but the effect of irritation of the vagus was very variable and uncertain. No change was observed in the vessels of the pia mater on pressure of one or both carotids, when examined through a piece of glass luted into a trephine hole, which would appear to show that the conditions for the establishment of the collateral circulation are more favorable in animals than in man, since in him ligature of a single carotid will sometimes cause fainting or temporary paralysis. Examinations made through the same window showed that the vessels of the brain underwent considerable increase in size when a saline solution was strongly injected into the carotids. That a certain degree of independence exists between the circulation of the two sides is interestingly shown by the fact that, if a saline solution be injected into one carotid, the retinal vessels can be made to appear quite pale on that side, whilst they preserve their natural color on the other side. When the injection is driven with great force, considerable exophthalmos occurs, with contraction of the pupil. On asphyxiating an animal, the intracranial pressure rises considerably, with obvious hyperæmia. After a time the pressure falls, with coincident anæmia. M. Pagenstecher trephined the skulls of dogs, and injected a mixture of wax and tallow, melting at 122° (?), under a pressure of about 100 mm. of mercury, between the bone and the dura mater. After the death of the animal, the quantity of wax contained in the skull was compared with the volume of the cranial cavity itself. He believes he has been able to show that the cause of the symptoms of pressure are referable to compression of the vessels. The experiments appear to have been very painful in non-narcotised animals, probably owing to the forcible detachment of the dura mater from the bones; and epileptiform convulsions, opisthotonos, and emprosthotonos, with gradually increasing coma, were constantly observed when the injection was rapidly made. The changes in the pupil resulting from the variations in the amount of pressure produced by the injection are carefully given.

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ASCIDIANS AND VERTEBRATA.

THE discovery by Kowalewsky of the existence of a structure similar to that of the chorda dorsalis in Ascidians during a certain period of their development, and the consequent important connexion established between the Mollusca and the Vertebrata, has led to a considerable number of researches being undertaken upon the same point by various observers, which have been recently noticed by M. Boll in the *Centralblatt*. Kowalewsky's researches were undertaken chiefly upon Thallusia mamillata and Ascidia intestinalis. The Ascidians, it may be premised, are true hermaphrodites, being capable of self-fertilisation. Kowalewsky applied semen to ova taken from the same individual, and found that about an hour after the artificial impregnation had been accomplished cleavage commenced in the latter. The ova are somewhat complicated in structure, consisting of a vitellus surrounded by a gelatinous layer containing yellow cells, and the whole invested by a thick, hard capsule. The remarkable peripherically situated yellow cells proceed from the ova-bearing follicle, and subsequently form the mantle of the animal. The central cells of the mulberry mass break down, and a cavity (Baer's or the segmentation cavity) is formed, so that the embryo now appears as a hollow ball. A flattening takes place at one part, and an involution here occurs, giving the mass the form of a cup. The border or mouth of the cup contracts, forming the anus. The interior of the cup forms the cavity of the intestine, and the space between the inverted cup and the general integument forms the perivisceral cavity. Whilst these changes are going on, other processes are also occurring which indicate the relationship of the Ascidians to the Vertebrata. Upon one side of the embryo, but only at the fore-part, two dorsal ridges or laminæ appear, which ultimately unite in the middle line, and thus form a tube for the nervous system running parallel to the intestinal tube. This nerve-tube is only visible in the anterior part of the embryo. Posteriorly it disappears; and here a cord makes its appearance between the intestine and the wall of the body, composed, like the chorda dorsalis, of large cells, from which the tail of the larva is developed. The nerve-tube becomes more spherical, and a temporary development takes place of organs within the tube that seem to represent the eye and ear. In the meanwhile the chorda dorsalis develops backwards, presents indications of segmentation, which soon however disappear, and the cells fuse into a hyaline mass, upon which Nogine, in a memoir not yet published, is stated to have discovered a series of ganglia. The chorda forms the axis of the tail, which atrophies within a few hours after the escape of the embryo from the egg. The intestine grows forward in front of the great nerve-mass, and opens on the dorsal side of the body, its anterior part fusing with the body-wall, and dilating to form the branchial cavity.

The remarkable points of resemblance between the development of Ascidians and of the Vertebrata, it will be seen, consist—first, in the formation of the dorsal laminæ and their coalescence to form a nerve-tube; secondly, the presence in the body of two parallel tubes, of which the lower is the intestinal tube and the upper the nerve-tube; and, lastly, the possession of a chorda dorsalis. The more recent researches of Kupffer, of Metschnikoff, and of Ganin, essentially agree with those of Kowalewsky. Dönitz, however, who has been chiefly engaged with the development of the Clavelina lepadiformis, strongly opposes many of the statements of Kowalewsky, especially in regard to the nature of the so-called chorda dorsalis and the development of the primitive organs. It is, of course, possible that the discrepancy may be attributable to specific differences in the animals examined.

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CALORIFICATION AND METAMORPHOSIS OF TISSUE.

SOME valuable observations have just been published in the *Centralblatt*, by Dr. H. Senator "On the Relations between the Generation of Heat and the metamorphosis of Tissue in the Body." He shows that under ordinary waking conditions, with the body at rest, and the stomach not actively engaged in digestion, the generation and loss of heat do not undergo any considerable variation. Even when no food has been taken for a considerable period, though the development of heat and the excretion of carbonic acid diminish, it is to a less extent than might be expected. Thus a dog weighing 12 lb., and fed on a pound of horse-flesh and a little lard per diem, gave off 12, at most 14, calories (one calory raising a kilogramme, or 2 lb. 2 oz. of water, 1° cent.) per hour, and from 52 grs. to 56 grs. of carbonic acid; after two days' fasting, it still gave off 11·6 calories and 51·7 grs. of carbonic acid. Digestion, as has long been well known, increases the excretion of carbonic acid; and Senator demonstrates also that it raises the temperature,—the two, however, not being quite proportionate, the augmentation of temperature being relatively the greatest. In the second hour of digestion the dog gave off no less than 21 calories, and excreted 77 grs. of carbonic acid; showing an increase of 60 per cent. in the former, and of 45 per cent. in the latter. This difference, no doubt, arises from the circumstance that although active disintegration and metamorphosis of the tissues and materials ingested are taking place, all leading to the evolution of heat, yet these changes do not at once proceed to the formation and elimination of the ultimate products—carbonic acid, water, and urea. M. Senator next shows that when the body is artificially cooled there is probably no increased generation of heat, though the amount of carbonic acid excreted is greatly augmented—a point of great importance, as showing that the amount of carbonic acid excreted cannot be regarded as a measure of the amount of heat generated in the body; or, as he puts it, under certain circumstances, the excretion of carbonic acid can increase without increase in the production of heat, and without the performance of mechanical work. M. Senator then proceeds to give the results of his researches on this subject in animals in which a febrile state had been artificially produced. He finds that during the first two hours after pus has been subcutaneously injected there is no marked increase either in the elimination of heat or of carbonic acid; and even at the height of the febrile state, when the temperature in the rectum reaches to 40°–41° C., the amount of heat eliminated, as estimated by the calorimeter, and the amount of carbonic acid produced, are but little if at all augmented. When increase of the excretion of carbonic acid does occur in fever, it is always proportionately less than the augmentation in the discharge of urea, which is constant and considerable. From this and other considerations he concludes that during the height of the febrile attack certainly not more but rather less fat is consumed than during a corresponding period of fasting; and notwithstanding the partial increase in the metamorphosis of tissues and blood components that occurs in fever, yet, upon the whole, the forces set free are considerably less than in health with sufficient food.

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DEVELOPMENT OF THE GREGARINIDÆ.

M. E. van Beneden publishes a paper in the *Académie Royale de la Belgique* containing the results of his observations on the Gregarinidæ, which are important as explaining an obscure point in the history of the development of these lowly organised animals—namely, their development from Amœbæ, a change which was originally stated to occur by Lieberkühn. Van Beneden has followed, in the case of the Gregarina gigantea, the entire series of metamorphoses undergone by the masses of protoplasm proceeding from the psorospermia into a fully developed gregarina measuring two-thirds of an inch in diameter. He found in the intestine of the lobster small masses of protoplasm destitute of nuclei, finely granulated, and performing continuously ordinary amœboid movements. These bodies he compared to the Protamœba primitivia or Protamæba agilis of Haeckel, and classifies them with the true Gymnocytodes of that author. Bodies of precisely similar appearance are also found, however, which are incapable both of locomotion and of change of external form. In these a homogeneous marginal zone and a granulated internal mass come to be distinguished. The Gymnocytode invests itself with a membrane, and is thus converted into a Leptocydote (of Haeckel). Each cytode now thrusts out two arms, which are prolongations of its substance, and each of which is destined to become a Gregarina. The first one that makes its appearance is the largest, and is characterised by its marked granulation and great mobility. It detaches itself from the cytode and becomes free. Before this occurs, however, the smaller arm is thrust forth, which is not granular and is motionless. After the first or mobile arm has separated from the cytode, the second arm becomes as mobile as the first was, and absorbs the whole body of the cytode into itself, just as the embryo of a mammal absorbs the contents of the yelk-sac. The two protoplasmic arms thus produced from a single cytode—Pseudofilaria, as Van Beneden terms them—move with extraordinary vivacity in the intestine of their host. After the lapse of a certain time the movements become less lively; the pseudofilaria become shorter and thicker; nucleoli, and subsequently a nucleus, make their appearance in their interior. At a still later period a homogeneous cortical layer (membrane) is differentiated from the granulated contents, at one pole of which strongly refractile granules collect, and the whole organism now appears as a well-defined small Gregarina, which soon augments in size.

Medical Annotations.

"Ne quid nimis."

TERMINATIONS OF NERVES IN THE VAGINA.

A PAPER has recently been read before the Imperial Academy of Sciences of Vienna on this subject, by M. Chrschtschonovitsch. The animals examined were man, the rat, dog, and rabbit, the last being by far the best adapted for demonstrating the relations of the finer branches of the nerves. The mode of preparation consisted in placing strips of the membrane, removed immediately after death, in a one-half per cent. solution of chloride of gold for about half an hour. The fragments were then washed for from twelve to twenty-four hours in distilled water, after which they were transferred into a nearly saturated solution of tartaric acid, which may be gently warmed. The almost black specimens were now hardened in alcohol for two or three days, and then imbedded in a mixture of wax and oil. Sections were made in various directions, and examined with the aid of glycerine. It will be seen that this mode of preparation nearly coincides with that, as recommended by Dr. Klein, and which, from personal inspection of the sections, we can pronounce to furnish excellent results. The mucous membrane of the vagina of all animals contains numerous branched cells, resembling connective-tissue corpuscles, arranged just beneath the laminated pavement-epithelium with almost the same regularity as in the cornea, whilst others are scattered irregularly through the membrane. The nerves penetrating through the muscular layer to the mucous membrane consist of large bundles of medullated fibres, which here and there contain groups of ganglion-cells. From these smaller trunks are given off, still medullated, which enter the proper mucosa, or rete Malpighii; as these pass towards the surface the medullary sheath is gradually lost, and the fibrils either bend back or join with a fibril from a neighboring trunk, forming a very superficial plexus—very close in rabbits, less close in dogs,—or apply themselves to the wall of one of the small vessels ascending to supply the papillæ of the membrane. Some few of them, destitute of medullary sheath, may be seen ascending between the epithelial cells; but the author agrees with Hensen and Klein, and is in opposition to Kühne and Eberth, in stating that the nerves do not join with the branched connective corpuscles mentioned above. The smooth muscular-fibre fasciculi of the membrane are surrounded by a very rich plexus of non-medullated nerve-fibres, from which individual fibres are given off that exhibit here and there granule-like enlargements, and penetrate between the several muscle-cells. He thinks it probable these last are encircled by the ultimate fibrils, but has not been able to find any closer relation to the nucleus of the muscle-cells, as stated to occur by Klebs, Frankenhaüser, Arnold, Lipmann, Henoique, &c. Following the branches given off to the epithelium from the subepithelial non-medullated plexus still more minutely, he finds that they break up in the deeper layers into a plexus of fibres encircling the epithelial cells, amongst which are large branched corpuscles that blacken more intensely with the chloride of gold, suggesting their nervous nature, and which are identical with those described by Laugerhaus in the rete Malpighii of the skin. In the dog, fibrils run up to the most superficial layer of the epithelium, where they appear to terminate by an enlargement of considerable size.

SOME OF THE SEQUELÆ TYPHOID OF FEVER.

A TEDIOUS convalescence is the rule in cases of typhoid fever of a severe character; and it is sometimes protracted by a want of reparative power in the intestinal ulcers, which, so long as they remain uncicatrised, are causes of exhaustion and may at any time become sources of danger. As a case in point, a convalescent from typhoid fever recently died at the end of seven weeks from perforation. But typhoid fever is often followed by various local affections which, although not dangerous to life, tend to complicate convalescence and to retard the patient's progress to health; and about these a few words may be said. In the first place, the sequelæ, as they are called, of all fevers are not the same; each form of fever is apt to be followed by its own train of accidents. For example, tuberculous diseases of the lung are more frequent after attacks of small-pox and enteric fever than after those of scarlatina and typhus fever; and bedsores, erysipelas, and circumscribed suppurations, which are frequent sequences of exhausting diseases of all kinds, are nevertheless more common after an attack of typhoid than after one of typhus fever. These local affections do not always depend upon the same or a similiar combination of causes. Bedsores may, of course, be readily accounted for by the effects of pressure in diminishing the circulation, and consequently the nutrition of a portion of the body in a patient whose vital powers, as a whole, have been already much reduced. Sometimes, however, numerous small abscesses or localised inflammations affecting the areolar and other textures make their appearence; and their development is often to be accounted for by that depraved state of the blood and system which is tersely expressed in the phrase, often enough used by the poor, "dregs of a fever"; but these disorders occasionally, no doubt, depend upon the blocking up of some small vessels with morbid particles derived from a distance, and the consequent changes in the tissue-area nourished by the obstructed vessel. On a large scale we sometimes see this in the local dropsy of a lower extremity from blocking of its principal vein. In rare cases symptoms of systemic blood-poisoning may follow the introduction of matters into the circulation from bedsores and suppurative lesions. As might be anticipated, symptoms referable to disease of the nervous system are not uncommon after typhoid, especially those of a functional character, such as various disturbances of innervation, neuralgia, and so forth; to say nothing of defective mental power, and occasional loss of memory of facts, or even of a language. These disorders of innervation, besides local pain, occasionally induce local congestions, simulating inflammation of the parts to which the affected nerves are distributed. Among the causes of a prolonged convalescence, we ought to remember also the defective digestive power of the stomach, apparently due to a granular degeneration, and loss of the secreting cells of the gastric glands. In scarlatina, according to recent researches, this deficient digestive power of the stomach is due to an atrophy or destruction of the gland structures from a peculiar inflammatory process taking place in them. Among the causes of the deficient blood formation and wasting that occasionally continue so long after an attack of typhoid we may reckon the depression of the vegetative system, the shrivelling of the mesenteric and follicular glands, and the pigmental deposits in the intestinal villi.

A GERMAN FELD LAZARETH.

To the "St. George's Hospital Reports" an excellent account of the working of a German Feld Lazareth has been contributed by Mr. W. Ewart, who became connected with one of the German lazaretto hastily organised to receive the victims of the 31st of August. Mr. Ewart's experience resolves itself into five *momenta*. 1. The general treatment of the wounded and sick. 2. The German and the French bullets compared in their action. 3. The mode in which German surgeons conducted operations. 4. The mode in which fracture of the thigh was treated. 5. The nature of the water-treatment as seen at Stenay. Mr. Ewart is very clear as to the wisdom of treating the wounded in the open air—the only course, indeed, that in many cases was possible. Interesting details as to the attendance, regular and volunteered, on the sick are succeeded by an account of the "Eiterbecken" and the "Irrigator," two implements in which the ambulances abounded for treating wounds. The former served admirably to collect the pus; the latter to inject antiseptic lotions into the suppurating cavities; while carbolic acid and oakum proved a most successful application. Great differences were observed by Mr. Ewart in the liability to sickness of the several nations represented in the German armies, the Prussian being decidedly the strongest, the Saxon the weakest. Then, as to the projectiles used by the belligerents, Mr. Ewart confirms what we have already noticed—to wit, that while velocity is in favor of the French bullet, weight is in favor of the Prussian. The extent of injury caused by the Chassepôt bullet was in direct proportion to the resistance offered; on the other hand, wounds from spent balls were much more common in the French than in the Prussian ranks. Reluctance to operate was, Mr. Ewart says, characteristic of the German surgeons; excessive alacrity of the French—the primary amputations among equal numbers of French and Germans being far more common among the former than the latter. The section which Mr. Ewart devotes to the German treatment of fracture of the thigh is most interesting; but would be hardly intelligible unless given in his own words, for which we have not space. We can, therefore, only recommend it to the reader. His concluding remarks on Dr. Ducluzaux's water-dressing by continual irrigation are also well worth attention. The wounded limb is placed on a slightly inclined surface, furnished at its most depending edge with a tin gutter, which empties itself below. The water drops from a funnel-shaped vessel suspended above the bed, the flow being regulated by squeezing a perforated cork tightly into the neck of the funnel. By this means, says Mr. Ewart, "the inflammatory symptoms and the local swelling were subdued with the greatest rapidity, and the wounds seemed to be guarded against all deleterious influences." Mr. Ewart might, with much advantage, republish his notes in a less fragmentary form. As it is, they are interesting in their tenor, and effective in their style.

PARIAN FOR HOSPITALS.

One of the great things to be accomplished in hospitals is the provision of walls of impermeable material in the wards. It has been found that absorbent surfaces, taking up and retaining the organic matters that are constantly present in hospital air, become sources of danger to the inmates. Not only are infectious diseases liable to be spread in this way, but surgical cases are prone to be attacked with various complications. For example, in a ward of the New York City Hospital, where hospital gangrene had appeared, removal of the furniture and patients did not prevent fresh patients from being attacked. Closing of the ward and whitewashing had no effect. The plastering was then removed and fresh plaster applied, but still cases occurred; and then, as a last resource, the walls were taken down and rebuilt, which effected the desired end. Sometimes it has happened that absorbent walls, fouled and stained with organic matters, have failed to give rise to any apparent injury to health until they had been disturbed by cleansing and scraping. All sanitarians have been impressed with the necessity of providing the walls of hospital-wards with some substance impermeable to fluids and gases, and it has been generally considered that parian cement met these requirements. Notwithstanding its great expense, parian has consequently been used in all our best hospitals of late construction. There is much reason to fear, however, that it does not entirely fulfil its purpose in this respect; at any rate the parian in some of our hospitals has become stained and dirty-looking, and no amount of washing will remove this. Whether the staining have resulted from the absorption of materials condensed upon the parian in the moisture of the ward-atmosphere, or whether the discoloring be due to some structural change in the parian itself, does not appear to have been accurately determined yet. Still, the disagreeable fact remains; and it may make us hesitate about incurring the cost of providing parian for our hospitals until we know the causes of the change to which we have alluded. In the meantime, some have suggested glazed tiles, which are very effective-looking and cleanly, as a substitute; but their employment is open to the objections that they are very expensive, and that the substance by which they are cemented together is not impermeable; and, considering the small size of these tiles, the aggregate number of interstices to be filled up would be too great. After all, perhaps the best thing to do is to paint the walls with several coatings of any color, protected with a very thick layer of varnish. This would probably be impermeable, and admit at the same time of being thoroughly cleansed and purified from time to time.

RAPIDITY OF THE CIRCULATION IN THE VEINS.

A TRANSLATION of a paper contained in the Bulletin of the St. Petersburg Academy of Sciences appears in the *Journal of Anatomy and Physiology*. It contains the results of the experiments of MM. Cyon and Steinmann, who have recently found that the amount of blood which flowed in a given time through the jugular or crural veins of dogs, narcotised with opium or chloral, was nearly the same as that which flowed through the corresponding arteries. The velocity of the current in veins is therefore much greater than that calculated by Volkmann. The rapidity of the current in the veins undergoes variations similar to that in the arteries, so that it is much diminished in the jugular when the carotid of the same side is compressed, but is increased by compression of the carotid of the opposite side. Division of the spinal cord lowers the blood-pressure and thus lessens the rapidity of the current. Irritation of the cord generally raises the blood-pressure in dogs only to a slight extent, and as it causes contraction of the arterioles, it generally lessens the rapidity of the current. Sometimes division of the cord is followed by a slightly increased rapidity of the current. Irritation of a sensory nerve produces general contraction of the arterioles, and temporary contraction succeeded by dilatation in the part supplied by the nerve. When the tibial nerve was irritated the rapidity of the current in the crural vein was increased on one occasion so much that Cyon thought it due to local dilatation of the arteries occurring at the same time as a general increase of blood-pressure. At other times the rapidity became greater as the blood-pressure sank, apparently from the contraction of the arterioles, which had slowed the current in spite of the increased blood-pressure having given way to dilatation. If no rise of blood-pressure occurred, the stream in the veins was slowed by the contraction of the arterioles. Stoppage of artificial respiration in curarised dogs increased the rapidity of the current, and it only began to diminish when they were becoming asphyxiated. If the tracheæ of dogs which were not narcotised was closed when their lungs were distended with air, the rapidity of the current in the veins became rapidly diminished in consequence of venous congestion.

ARTIFICIALLY-INDUCED EPILEPSY IN GUINEA-PIGS.

IN the course of some recent experiments on the establishment of artificial epilepsy in guinea-pigs, Dr. C. Westphal has been over the same ground as that explored by M. Brown-Séquard, and fully corroborates the results at which the latter observer arrived. He adds, however, some new and interesting facts. Thus he found that if one or two slight blows on the side of the head are given to a guinea-pig, they are sufficient to bring on an epileptiform attack, after which the animal again recovers its liveliness, or it remains heavy for some time, and then exhibits a kind of rotatory movement, like those shown by Schiff to occur in rabbits after lesion of the crus cerebri. If the blow be too violent, the animals dies with or without convulsions, but always with arrested respiration, the heart continuing to beat for some minutes, and life may be preserved, though usually only for a time, by artificial respiration. If the animal survives the blows, a similar epileptigenic zone is created as in the guinea-pigs treated on M. Brown-Séquard's method by lesion of the medulla oblongata at certain points, or section of the sciatic; and, as in these last cases, the zone is near the angle of the lower jaw. Before the zone is well established, and four weeks are usually required for this purpose, the animals betray the presence of some irritation at this part by frequently scratching it. After it is established, slight pinching will induce tonic and clonic spasms, through the sensibility of the skin is here diminished. The rapidity with which the zone can be established may be increased by striking the animal's head on successive days, and the excitability of the zone endures for a period varying from six weeks to six months. The condition is hereditary. M. Westphal set himself to ascertain the nature of the changes induced by the blow or blows. That the condition is not produced by any alteration in the integuments is shown by the circumstance that the fits occur when the exposed skull is struck. Careful examination of the brain also furnished no clue to the mystery. But on examining the medulla oblongata and spinalis cervicalis, and even sometimes dorsalis, he found small hæmorrhages both in the white and grey matter to be of constant occurrence, and there was usually also hæmorrhage into the sac of the dura spinalis. These small hæmorrhages appear to induce the primary attack of epileptiform convulsions, and the changes occurring around them to lead to the formation of the epileptiform zone; but *how* or *why* is not yet capable of explanation.

CONNEXION OF NERVES WITH PIGMENT CELLS.

AN essay recently appeared in the *Monthly Microscopical Journal*, by M. Pouchet, in which he endeavored to show that the chromoblasts of various flat fish are composed of masses of sarcodic substance containing pigment granules or fluid and a nucleus, and that they are in direct connexion with the finest branches of the nerves. In a paper contained in the same journal of the present month by Dr. Lionel Beale this view is contested, Dr. Beale stating that in *no* case do the finest terminal ramifications of the nerve-fibres end in the manner described. Dr. Beale's observations were made with very high powers.

GLYCERINE AS A SOLVENT FOR SUBSTANCES USED IN SUBCUTANEOUS INJECTION.

AT a late meeting of the K. K. Gesellschalft der Aerzte in Vienna, Dr. Moritz Rosenthal, one of the *docenten* in the Allgemeine Krankenhaus, advocated the use of glycerine as a medium for the solution of the various substances used for subcutaneous injection, and also exhibited specimens of such solutions. It is of the highest importance that the glycerine used be chemically pure, and free from all traces of fatty acids. By gradual elevation of temperature, the glycerine can be made to take up into solution a large number of certain alkaloids—e.g., chinin, morphin, curan, and also opium extract, and to retain them dissolved for, at all events, a year (this being the duration of the experiment) in a complete and clear solution.

Certain other substances—e.g., thein, caffein, and preparations of iron, are, on the cooling of the solution, reprecipitated.

Of disulphate of chinin, twenty grains are soluble in one drachm of glycerine, this being in the proportion of 1 to 3. The same amount of the solvent will take up ten grains of muriate of morphia or of opium extract. Corrosive sublimate, of which five to six grains are soluble in one drachm of glycerine, must be diluted with water. One drachm of the solvent will take up only one grain of curare. The glycerine solutions of chinin, subcutaneously applied, were found of especial benefit in intermittents.

All of these solutions were stated to remain during the summer months perfectly pure and free from all traces of vegetable moulds.

In cases where the patient is very sensitive, these solutions can be diluted with water, and are stated to be especially adapted for injections in the region of the back, of the sides of the chest, and of the nates.

EXCITABILITY OF THE CENTRIPETAL FIBRES OF THE SPINAL CORD.

Dr. Dittmar has recently undertaken, under C. Ludwig's direction, investigations upon the excitability of the substance of the spinal cord. He has employed rabbits in his researches, which were subjected to the action of woorara, and various sections of the spinal cord were made in various directions; and, as the general result of his experiments, he thinks it is certain that there is a system of fibres in the substance of the spinal cord which, though they do not belong to the nerve-roots, are capable of responding to the action of direct stimuli, and can transmit the impulses thus generated along the whole length of the spinal cord up to the medulla oblongata, where they undergo reflection into motor nerves.

INFLUENCE OF DEFICIENCY OF LIME AND PHOSPHORIC ACID ON THE BONES.

Dr. Weiske, continuing his former experiments on the effects of the withdrawal of certain constituents of the body from the food, has selected a goat, and has supplied it with food deficient in lime and phosphoric acid. He gives the results of his experiments in the *Zeitschrift für Biologie*, just published. They are remarkable, and show that the withdrawal of these elements from the food proves injurious to the animal, and may, in fact, ultimately lead to death; but that no change in the composition of the bones can be shown to have occurred, and that the bones do not appear to be rendered thereby more friable.

A certain M. Donac has recently laid before the French Academy of Sciences a project for liquefying dead bodies and transforming them into a syrup without color or smell. According to his calculations a moderate-sized man could be got into six bottles. The size of each bottle is not stated, but the *Paris Journal* appears charmed with the idea, and exclaims, "What an opening for the exercise of filial piety!"

Correspondence.

"Audi alteram partem."

THE THEORY AND TREATMENT OF CHOLERA.

To the Editor of The Lancet.

Sir,—It is perhaps unnecessary at the present season to occupy the attention of your readers with any further remarks on the true theory and treatment of cholera. But before the discussion is closed, it may be useful to direct notice to the following important admissions, which, so far as can be gathered from his remarks on the subject, are expressed or implied in Dr. Johnson's communication to The Lancet for March, in reply to my late paper "On some Physiological Errors connected with Cholera."

1. That temporary albuminuria is not *conclusive* evidence of blood-poisoning.
2. That cyanosis is not a *diagnostic* sign of cholera.
3. That the secretion of urine by the fœtus is not "*even more scanty* than that of a patient in cholera collapse."

When Dr. Johnson pleads that my so-called "merciless criticism" of his statements respecting a certain "interesting physiological correlation," to which he has referred at some length, is unjust, because a lately published paper on the subject (Aug. 1870) has been selected instead of one of a much earlier date (Feb. 1866), he simply succeeds in illustrating the truth of my remark, that the unsoundness of his theory "becomes more apparent after every fresh effort on the part of the author to retrieve his position."

"Our failures may be as instructive as our successes" was the sad but hopeful comment of Dr. Johnson himself on the treatment of cholera at King's College Hospital in 1866, for no temporary success of a theory can justify any continued failure in treatment; and it is reassuring to find that the lesson learnt on that occasion has not been forgotten. For although the opinions of the distinguished Professor of Medicine at King's College respecting the nature of the disease are still apparently unchanged, yet his opinions respecting its treatment have since undergone a marked and progressive change for the better, in consequence of which he no longer advocates "the necessity of a cathartic method of treatment in the widest sense of that term"; and his theory is now very far from being in strict accordance with his practice. "The good effects of heat" were, we have been assured by Dr. Johnson in his Notes on Cholera in 1866, "unquestionable"; whilst cold water was decidedly injurious, in consequence of its "diminishing the vascularity of the mucous membrane, and thus interfering with the excretion of the morbid poison." It would be, of course, impossible for me to fix the exact date at which the decisive change in Dr. Johnson's opinions on the treatment of cholera occurred. It is sufficient to know that a very great and a very praiseworthy change of opinion has lately happened, and that he has at last kindly consented to be guided by the experience of others in the revision of his plans for the future treatment of the disease. With reference to this subject, it is interesting to note that in place of hot liquids to drink, and of hot injections into the rectum, he is, *for the present*, inclined to give an abundant supply of cold water; and we have been quietly led to infer that castor oil may, *in most curable cases*, be safely omitted.

We are quite content to rest and be thankful, since so much has been gained in the warfare against a fatal error. "The copious imbibition of pure cold water," with perhaps the addition, which has been lately recommended, of a little phosphoric acid, "will suffice for the cure of most curable cases"; and when the pathology of the disease is better understood, it may probably be admitted that the application of castor oil and *hot liquids* to the alimentary mucous membrane during choleraic collapse is painfully suggestive of the application of petroleum to a house on fire.

I am, Sir, your humble servant
WILLIAM SEDGWICK.

Park-place, Upper Baker-street, December 11th, 1871.

⁂ This controversy must really end now.

PREVENTION OF MAMMARY ABSCESS.

To the Editor of THE LANCET.

SIR,—Will you kindly allow me space in the columns of your journal for the following remarks.

During the last fifteen years' practice in the colonies I have been somewhat amused by reading the many theories put forth by my professional brethren at home as to the proper treatment of "mammary abscess." The remedies advocated are almost as numerous as the cases to be met with. During the last few years, it has very frequently occurred to me as to whether we could not devise some means of preventing the formation of such abscess altogther, instead of waiting until inflammation and suppuration has actually taken place. Physiology has taught us that the chief causes of mammary abscess occurring soon after parturition is the obstruction within the milk ducts. In this communication I propose to deal with the cause, leaving the effect to more skilful hands to deal with, and, in doing so, endeavor to prevent the obstruction above referred to. It is almost needless for me to say that mammary abscesses are very tedious and difficult to cure, and seldom pass without leaving behind evils which continue for a long time; and where there has been a free use of the lancet the breast becomes so mutilated as to render futile all future attempts to suckle the infant.

The remedy I have found excellent is a free use of olive oil as an emollient and stimulating external application. The mode I usually employ is to cut a piece of lint the size of each breast, allowing it to come well under the axilla, leaving a hole for the nipple to pass through for the infant to suck. Before applying the lint, saturate it well with the best olive oil, and envelope the breast; next cut a piece of oiled-silk the shape and size of the lint, and cover this over the breast so as to prevent the oil from soiling the linen. In all cases let this be applied not later than the second day after the accouchement. As the lint becomes dry, moisten with more oil. Let this be applied until the patient is able to get about. I have tried it in many cases, several of which were primipara, and in no single instance has it failed. Besides, it not only prevents the obstruction of the milk-ducts, by its heating and emollient properties stimulating the capillaries around the base of the gland, but it also causes the milk to flow from the excretory ducts involuntarily (tubuli lactiferi), or, what is better understood in the phraseology of nurses and midwives, by causing the breasts to run. I am of opinion that if the oil is properly applied, and at the proper time, there no longer remains cause to fear the formation of mammary abscess. What I formerly treated with repeated nauseating doses of tartar emetic, thereby debilitating the patient much more than necessary under such circumstances, I now treat with the olive oil, without the use of so depleting a drug as antimony.

Trusting the foregoing remarks may be of some interest to my brother practitioners all over the world.

I remain, Sir, yours faithfully,
W. R. G. SAMUEL, Surgeon.
Member of the New Zealand Institute.

Wanganui, New Zealand, Aug. 20th, 1871.

PARIS.

(FROM OUR OWN CORRESPONDENT.)

AT a recent meeting of the Société de Chirurgie a very interesting discussion took place on the subject of tubercular disease of the testicles, the orators not being agreed as to the proper method of treating the disease, and especially as to the advisability of taking away the testes. M. Chassaignac took a decidedly conservative view of the subject. He stated as his opinion that whenever there was a mere abscess, particularly an encysted one, of the testicle, amputation should not be performed. Whenever the general condition of the patient suggested the idea of amputation, an exploring puncture should first be made, and if pus were withdrawn, the best course would be to make an incision, and drain when the incision alone did not suffice. Mutilation of the testicle ought never to be performed. M. Demarquay took an opposite view. He limited M. Chassaignac's proceeding to cases of purulent fistulæ, when only suppuration existed. But where tubercles were in existence, he took an opposite course. He would not use draining in cases of manifold fistulæ. The patient was harassed by the numerons appliances; he could neither work nor walk; and, finally, the surgeon had to take away the testicle at the patient's urgent request. Yet he would confine the operation of amputation to cases of infiltrated tubercle of the epididymis and testicle, in which the disease acts on the whole system. M. Chassaignac remarked that even in cases of manifold fisulæ a single draining-tube would suffice. M. Tillaux sided with M. Chassaignac, and would only operate in cases of general wasting of the patient. M. Giraldès said that, on considering the morbid pathology of tubercular testicle, he considered draining to be insufficient. He had never had a fatal case of amputated testicle. M. Lefort said that he also had never had any fatal case. He thought it was wrong to proscribe amputation in cases of tubercular testicle unattended with tubercles in the lungs. M. Chassaignac wound up the discussion by asking his colleagues to bring forward their various statistics in connexion with the sequelæ of amputated testicle.

Paris, Dec. 31st, 1871.

RESECTION OF THE ŒSOPHAGUS.

To the current number of Langenbeck's *Archiv* Professor Billroth, of Vienna, contributes a most interesting and suggestive paper bearing the title, "Ueber die Resection des Œsophagus." He states that some time ago, after a post-mortem examination of his first patient affected with carcinoma of the œsophagus, the possibility suggested itself of making a resection of this part of the alimentary tube. The fact that the lymphatic glands in the neighborhood of the diseased part are not generally affected, and the partial success which had hitherto attended the operation of œsophagotomy in this disease, together with the analogy of external urethrotomy in cases of gangrene or ulceration of the urethra, seemed to lend support to such an idea. The passing, moreover, of bougies through cicatricial tissue was far preferable to the manipulation of such instruments in a tube with ulcerated and weakened walls.

On April 21st of last year, a large dog was put under the influence of chloroform, and a piece, about an inch and a half in length, was cut out of the whole circumference of the œsophagus. The lower end of the divided tube was then fastened by a couple of sutures to the skin at the margin of the external wound. Up to the 27th of the same month the animal was fed with milk through a tube passed into the wound, but on and after this date the tube was passed *viâ* the mouth. A week after the operation the sutures were removed. By the end of June the fistulous opening had completely closed, and the process of healing would have been quicker if it had not been that the dog, like human patients, dissatisfied with "milk diet," purloined the more solid food of neighboring victims to science. After the closure of the œsophageal fistula, which took place at the end of June, the tube was daily dilated by a bougie of the diameter of a large index-finger. After the healing of the wound the dog was in capital condition, eating meat, potatoes, &c., but the variety of fare was not allowed to extend to bones. On July 26th the animal was killed with cyanide of potassium; and all that was found as a trace of the operation was an annular scar, scarcely half a line in width, and, moreover, easily dilatable.

Foreign Gleanings.

DIGITALIS AN ANAPHRODISIAC.

M. GOURVAT is publishing, in the *Gaz. Méd. de Paris*, a series of papers on the action of digitalis. In a late number (Dec. 23rd, 1871) we find the following:—"When digitalis or digitaline is administered for some time to a man in full possession of sexual powers, the latter become gradually weakened, the propensities disappear, the secretion of the liquor seminis diminishes by degrees, and may at last vanish altogether. These results are explained by the antiplastic and lowering action of digitalis. The antiphlogistic properties of the drug are the secret of its good effect in spermatorrhœa. With women, digitalis and digitaline excite strong, regular, and intermittent uterine contractions, and control metrorrhagia; hence digitalis is employed in exciting abortion. (Tardieu.) It is probable that digitalis acts as an anaphrodisiac in women also; inducing, by long-continued use, impotence and sterility. In men it hinders the secretion of the liquor seminis, and in women it may interfere with the development of the Graafian vesicles, the propagation of the species being thus doubly retarded.

PURE CHLOROFORM.

At a late meeting of the Academy of Medicine of Paris, M. Personne, a distinguished pharmacien, referred to alterations taking place with time in chloroform; these alterations he attributed to an impurity which he conceived to be chloric oxycarbonic ether becoming decomposed by the influence of light. To prove the truth of this assertion M. Personne showed a sample of chloroform which he had himself carefully purified, and which, exposed for the last two years to the action of light, has remained intact, and does not emit acid vapors. The author thinks that the presence of the chloric oxycarbonic ether in chloroform of commerce is due to the employment of impure alcohol called phlegm. Such things did not happen when chemists made their own chloroform with absolute alcohol according to Soubeiran's process.

AN OPHTHALMOSCOPE FOR THE SIMULTANEOUS USE OF TWO PERSONS.

At a late meeting of the Academy of Medicine of Paris, M. Gavarret exhibited an ophthalmoscope of this kind, constructed jointly by himself and M. Sichel, the son of the late distinguished ophthalmologist of Paris. The instrument may be of use when teacher and pupil wish to examine the same patient together, or when the eye is to be investigated in consultation. It is constructed on the principle of the reflection of luminous rays on the hypothenusian surface of rectangular prisms. Each observer looks through a different aperture of a box furnished with a mirror and an internal prism; and M. Gavarret states that, from the experiments made by himself and M. Sichel, the reflected image is the most distinct.

AN IMPROVED LITHOTRITE.

M. Reliquet, well known by his work on the Operations performed upon the Urinary Organs, has presented to the Academy of Medicine of Paris (January 23rd, 1872), a new lithotrite, having peculiar teeth in the male and relative recesses in the female branch. After describing the instrument (figured in *Gaz. Hebd.*, February 9th, 1872,) M. Reliquet says: "In short the action of this lithotrite is such that each time the branches are brought together the whole detritus they contain, after the lateral pieces are broken off, is wholly reduced to fragments, all of which are sufficiently small to be washed off by the evacuating catheter. Thus each sitting removes more sand than happens usually, and the number of operations is materially lessened."

TYPHOID FEVER NOT GENERATED BY PUTRID GASES.

In the *New York Medical Journal* (1871), Dr. Roberts defends the above-mentioned position, and thinks, with Sir Thomas Watson, that, however powerful the emanations from drains &c. may be, they cannot generate typhoid unless the dejections in the drains be charged with matter originating from typhoid patients. The author cites New York, Naples, Edinburgh, and London as proving his point. The emanations from the Thames, for example, in 1858 and 1859, did no mischief. Livingstone mentions a putrid marsh which did not harm his companions nor spread disease among the inhabitants; not to speak of animals, as eels, pigs, and birds, which thrive on putrefaction.

THE SPINAL PARALYSIS OF CHILDREN; INFANTILE PARALYSIS.

MM. H. Roger and Damaschino have just completed a series of papers on this complaint (*Gaz. Méd. de Paris*, Dec. 23rd, 1871), and, at the conclusion, lay down the following propositions:—"1. The alteration peculiar to infantile paralysis is a lesion of the spinal marrow, which causes the atrophy of muscles and nerves. 2. The seat of this lesion is the anterior part of the grey substance of the medulla, where softened portions of spinal substance are seen. 3. This softening is of an inflammatory nature; in fact, simple myelitis. 4. Infantile paralysis should, therefore, be called spinal paralysis of children, and be classed among the affections of the spinal marrow, as depending on myelitis."

THE CAUSE OF SIMPLE HYDROCELE.

M. Panas publishes eight cases in the *Archives Gén. de Méd.* (January, 1872), from which he concludes that this affection is caused by partial epididymitis, as he generally found the epididymis more or less indurated in all his cases. Of course he ascertained that the patients had had neither orchitis, syphilis, nor gonorrhœa; that they did not labor under tuberculosis, and that the corresponding testis was perfectly sound.

EXPLOSIVE BULLETS.

M. Thierry states, in the *Gaz. des Hôp.* (December 30th, 1871), that he does not believe such bullets were used in the last war. Those who suspected this foul play were influenced by the extensive mischief inflicted upon the trunk or limbs. But M. Thierry has had under his care a case of severe shattering of the upper part of the thigh, in which the pain was very severe, the crushing of parts very distressing; but the ball, on being found, was of the ordinary kind.

NECROSIS OF BONE IN STUMPS.

Messrs. Chipault, of Orleans, and M. Cazin, of Boulogne, have sent to the Surgical Society of Paris preparations showing that, in one case the shaft of the tibia, and, in the other, the diaphysis of the humerus, after amputation for gunshot wounds, perished for a great length owing to the loosening of the periosteum. The necrosed portions were removed, the patients did well, and the spared periosteum secreted much bone.

THE LUNG TEST.

M. Poncet showed, at a meeting of the Lyons Société des Sciences Médicales (Oct., 1871), the lungs of a fœtus prematurely born at the Hôtel Dieu. The child had cried, breathed, and lived an extra-uterine life of ten hours; but the lungs sank completely in the water, as if no respiration had taken place. Such cases, though rare, have been mentioned by Hoffmann and Heister, and show the insecurity of the lung test in forensic medicine.

PERI-UTERINE PHLEGMON.

Some have doubted the reality of this malady, but Dr. Gallard has shaken scepticism by bringing an actual preparation before the Academy of Medicine of Paris. He thus gave the material and palpable proof of the existence of this chronic abscess, the fact having long ago been pointed out by Valleix, Gosselin, Aran, Bernutz, and Goupil.

News Items, Medical Facts, &c.

The Relative Efficacy of Hyoscyamus, Bromide of Potassium, and Chloral in Mania.—Dr. John A. Campbell, assistant medical superintendent of Garland's Asylum, Carlisle, read a paper on the above subject at a meeting of the members of the Medico-Psychological Society, held at the Royal College of Physicians, Edinburgh, in November last, which subsequently appeared in the January number of the *Journal of Mental Science*. Dr. Campbell, in cases of insomnia, gave these three medicines for some time in the same doses to the same patients, with a view to arriving at a tolerably accurate idea of their relative efficacy, and the following are the results of his experiments:—Both chloral and tincture of hyoscyamus are sure sedatives to maniacal excitement, the former acting more quickly and more certainly as a sleep-producer of the two. Bromide of potassium, though to a certain extent a sedative to maniacal excitement and hypnotic, is not a sufficiently powerful sedative to allay intense excitement, or to procure sleep where great insomnia exists. A two-drachm dose of tincture of hyoscyamus is not quite equivalent to thirty grains of chloral. Two and a half drachms would probably be as nearly an equivalent as could be given. Bromide of potassium appeared to Dr. Campbell to be useful only where the excitement or insomnia is of a slight character.

Bleeding in Pneumonia.—When a medical man has to combat this affection, he either confines himself to expectation, or gives large doses of brandy, or takes blood from the arm, according to the school to which he belongs. Of course some regard is to be paid to the particular patient, and to the nature of the prevailing epidemic tendencies; but it still remains very melancholy that such enormous diversity should exist—a diversity which, to a lay observer, would look quite monstrous. However this may be, we think it right to refer to the essay of Dr. Simorre, practising in a country place of France, inserted in the *Tribune Méd.* of Paris, January 14th, 1872. Here a series of cases are related, wherein the patients were bled copiously at the very outset, again in four hours, lesser quantities of blood being taken at longer intervals two or three times afterwards. No other treatment than demulcent drinks. All the patients recovered.

Alcoholism in America.—Vigorous efforts are being made in America to restrict the ravages of intemperance, and to promote asylums for dipsomaniacs. In New York a Bill parallel to our own Permissive Liquor Bill has been introduced. Missouri has made an enactment, empowering interested persons to recover damages from the seller of alcohol for injury sustained by relatives, in consequence of the drink sold. Illinois lately passed a similar Act; while the Governor of Newfoundland, in his speech at the opening of the Legislature, recommended the Permissive Bill which has for several months past been agitated by the anti-confederates. Dr. Dalrymple's movement, as his recent tour in the United States enabled him to show, will derive much illustration and reinforcement from the practice and precept of our American cousins.

PRINTED AND PUBLISHED BY

WM. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery,
Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 6. NEW YORK, JUNE, 1872.

ABSTRACT OF Clinical Lectures ON SURGERY.

Delivered at Guy's Hospital during the Session 1871-72.

By J. COOPER FORSTER, M.B. Lond., F.R.C.S.,

Surgeon to the Hospital and Lecturer on Surgery.

LECTURE III.

ON FOUR CASES OF DISEASE OF THE KNEE-JOINT.

GENTLEMEN,—During the past ten days I have, as you are well aware, performed the operation of amputation through the thigh four times. In each instance disease of the knee-joint existed. The clinical aspect of these cases teems with suggestions, all of them important in the daily round of practice; while the pathological aspect, appealing as it does more directly to your eye with a prodigal liberality of facts, is at once most embarrassing to the lecturer in the abundance of its riches, and interesting to the student in the variety of its store.

With four such cases under notice at once, it becomes a serious question how best to utilise the information at our disposal. To secure this end I propose first to read to you the history of each case, and make a few observations in passing on special points which seem to require notice; we can afterwards go shortly into more general questions of pathology and practice which the review of such a series of cases of joint disease will enable us to do.

CASE 1.—John W——, aged twenty-nine, a dairyman, living at Brixton, was admitted on September 20th, 1871. His family history is good, and no history of tumour of any kind is known of amongst his relatives. No rheumatism or gout in his parents. The patient has had a winter cough for two or three years, though he has not been troubled with much expectoration; a year ago, however, he spat-up phlegm streaked with blood two or three times. He wasted also a little at this time. He has never been laid up with rheumatism, but has occasionally had "a touch of rheumatics" in his shoulders.

On admission the man stated that a month ago he was quite well, and could walk as well as anyone. He went to bed one night in perfect health, but during sleep his left knee became swollen, and the next morning it was nearly the same size as it is now (Sept. 20th), and he was quite unable to walk or to bear any weight upon it. It was red, hot, and very painful. He had received no injury of any kind to his knowledge in his previous day's work. The joint continued to increase slightly in size up to the time of his admission.

He is a spare and delicate man, with hollow cheeks and phthisical look. There is evidence of some consolidation at the right apex, but without any disorganisation, of the lung. The heart's action is irregular, with loud systolic, hoarse murmur, equally loud beneath the left as beneath the right clavicle, and also extending downwards to the apex.

His left knee is flexed at an angle of 120 degrees, and he cannot voluntarily either flex or extend it more than this; but the joint can be flexed to a greater angle by another person. The parts are swollen, the skin being white, tense, and shining. There is some thickening of tissues about the joint, and there is also increased heat of it. On more careful examination, it is found that some fluid exists in the joint cavity; but the patella and condyles of the femur have a normal relation to each other, while the lower margin of each condyle, as it rolls upon the head of the tibia, can be fairly defined. The greater part of the swelling about the joint is, however, altogether below the femur, and apparently in the head of the tibia; especially is this to be noticed over the inner side of the internal tuberosity. Here there is a large, somewhat elastic, rounded protuberance, bulging inwards and greatly distorting the outward form of the joint. At the most prominent spot there is some fluctuation, while all over the knee there is more or less pain on pressure; at some spots this is very severe. The tibia below its head does not appear to be expanded. The left knee measures at its largest part 14 inches, as compared with 12 inches, the measurement of the sound knee at about the same spot. The glands in the groin are not enlarged.

The joint was placed on a back-splint with a foot-piece, and he had some iodide of potassium given him, and ten grains of Dover's powder every night. In three weeks' time a slight reduction in size had taken place in the joint (½ in.) At the end of October, however, it was again 14 inches in circumference, and had increased to 14¼ inches by Nov. 20th. A grooved needle was introduced into the inner swelling on Nov. 6th, and a little greyish, mucus-like material was withdrawn on it. This, when examined microscopically, was found to consist of epithelium-like cells undergoing a fatty change.

On Nov. 27th the joint presented the following appearances as compared with those noted on his admission:—It is still flexed at an angle of 120 degrees. The joint is much distorted, the tibia being rotated outwards, and the patella displaced in the same direction, apparently resting quite on the external condyle of the femur. There is fluctuation on the inner side of the patella, and also to its outer side and below it. Over the inner tuberosity of the tibia there still exists the smooth rounded eminence noticed on his admission, and the shaft of the femur above the condyles is expanded, measuring roughly 3 inches to 2¼ inches on the other side. His temperature keeps daily at 99·5°.

Amputation was performed on Nov. 28th. Lateral skin-flaps were made, and the muscles cut through circularly. The vessels were secured by torsion. The whole operation was conducted under carbolic spray.

The joint contained a large quantity of curdy, yellow, semi-coagulated lymph. The whole of the synovial membrane was swollen, gelatinous, and vascular; more especially was this the case at the angles of reflection, where the membrane passed from the soft parts on to the bone. The bones were much displaced, the tibia being rotated outwards and pulled somewhat backwards, while the femur was turned in the opposite direction. The entire mass of the swelling over the inner side of the joint was caused by this rotation of the femur and consequent prominence of the internal condyle. Over the articular surface of the latter the cartilage was eroded and rough bone exposed. A corresponding patch of the articular surface of the inner tuberosity of the tibia was in a like state. Indeed, only at this spot did the bones appear to have come in contact; but by the displacement there was increased pressure here between the bones, so that the tibia was hollowed out, forming a shallow cavity for the internal condyle. The cartilage over the other parts of the articular surface was yellowish and of less consistence than normal. Microscopically, the cartilage cells were all in an early state of fatty degeneration. The crucial ligaments also, though entire, were soft and easily cut through.

I will now show you the parts which have been described, and you may see the great thickening of the synovial membrane and the softening of the ligaments. You will also notice that the bones themselves are not greatly diseased.

The especial interest of this case to me lies in the fact that I made an error in diagnosis. My impression was, that we had to do with a *growth* in the head of the tibia. I arrived at this conclusion—and not only I, but several most distinguished surgeons who saw the case with me—from the history which the patient gave us of enlargement of the joint within the last month or two, and because during the time of his sojourn here the circumference of the joint had gradually increased in size, this increase being chiefly local (as it was thought, in the head of the tibia), and a large rounded prominence gradually projected on the inner side of the joint. I may also tell you that there was no displacement of the leg from the femur when admitted, though the tumour to the inner side of the knee was then well marked. Latterly there has been much distortion, the foot being rotated outwards. You may now see that not the least sign of new growth existed, but that the whole of the tumour, forming what was supposed to be probably a myeloid formation, was produced by a dislocation of the femur partially off the tibia, but more particularly by an extraordinary amount of inward rotation of the thigh, which caused the internal condyle to be very unusually prominent. Look at and examine the parts well, for unless one had seen them and the patient it would be hardly possible to believe that such a condition of things could so simulate a growth.

CASE 2.—Thomas F——, aged twenty-two, an oil-ware-houseman, was admitted on the 16th of November. He states that his mother has had rheumatic fever, but there is no tendency to consumption or gout in his family. The object of these remarks is of course to show that our patient has no marked hereditary predisposition. In many cases this is a very important observation to make, and I advise you strongly always to attend to it in drawing up reports for others, or in taking notes for your own benefit. This man was in Naaman ward nine years ago, under my care, for cleft palate, and he was operated upon at that time. He kept strong and hearty till two years ago, when his right knee-joint became stiff very slowly. He was soon unable to move it, and the knee began to swell, but to his knowledge it was never red or hot. He walked about on the leg without any pain till three months ago, but since then he has had nothing but pain night and day, and the limb starts much when he drops off to sleep. He was in Lazarus ward fifteen months ago, under Mr. Birkett. The limb was swollen then just as it is now, but he had considerably more use in it. Splints were applied while in the hospital, but he has not continued their use since he left. Latterly he has had a cough, and spat up some blood a month ago.

He is an unhealthy-looking man, with sallow and pasty complexion, and with a large mass of hypertrophied glands behind the sterno-mastoid on the right side, and also to a less degree on the left. He talks very indistinctly, from the fault in his palate, though the operation for uniting the edges of the cleft has succeeded to a great extent. I may observe on this point that, unless the operation for cleft palate be performed at an early age, hardly any advantage accrues to the patient in the way of enabling him to talk more plainly. It is a question whether surgical aid should in such cases be rendered in early life, or whether it should be deferred till after puberty. The late operation while it has many advantages both during its performance and in the after-treatment, yet has this one great drawback, that articulation is very little if at all improved. Knowing this, I advise you, should such cases come before you, not to operate at the very earliest periods of life, but to wait till the child is about two years old; after that age has been reached, the sooner the operation is done the better. Such was not always my opinion; but, as the result of a larger experience, I have during the last twelve years modified my views on this point. I admit that judgments deliberately formed and expressed should not be hastily changed, but the further teachings of a number of years would vindicate the right of anyone to make such a change.—To return to the report before us. There is evidence of old lung consolidation at one apex, but no signs exist of any recent disease. The diseased limb is much wasted, the measurement being two inches and a half less round the thigh of the right than of the left leg, three inches above the joint. Be very accurate in your measurements, gentlemen; never use such relative terms as "larger" or "smaller," but always give the exact amount in inches or lines. The question of size is oftentimes a most important one, and will influence a diagnosis or determine a certain line of treatment. The skin is normal over the joint. The cavity is distended with fluid. Fluctuation can be felt on each side of the patella, which bone is easily felt, and movable, though with pain to the patient. The slightest attempt at movement of the limb gives excruciating pain. The bones are all in their proper relative positions, though the inner side of the head of the tibia is prominent. The bones are not enlarged beyond the joint. The limb is kept in a nearly extended posture. There is considerable enlargement of the glands in the groin. Urine not albuminous, specific gravity 1018. Since admission his chief complaint has been of pain. The temperature in the axilla has been daily over 101°; and his pulse is quick, 108 to 112 per minute. The operation of amputation was performed by lateral skin-flaps, with a circular division of the muscles, and the vessels were secured by torsion.

On examining the diseased joint, of which a section has been made for you, you will notice these conditions: that the synovial membrane is thickened wherever it can be found, and that is only at the sides of the joint where reflected from the bone on to the soft parts; that it is very vascular, and covered with yellow flaky lymph; and that the cartilages of the joint have all disappeared, leaving a vascular surface of bone. Notice also that this vascular part is in marked contrast with a dull-yellow patch of large extent on the articular surface of the external condyle of the femur, and also with a smaller one on the corresponding surface of the head of the tibia. A section of the bones made vertically shows these patches to be continuous with a dull-yellow patch which extends for some way (half an inch) into the cancellous tissue of the bone, and which is marked off from the rest of the bone by a red or vascular margin. This is what used to be called tubercle in the bone. I shall not call it so, seeing that tubercle is so very rare in bones; we will call it rather degenerating inflammatory product. Notice also that the vascularity of the bone has quite disappeared for some way up the shaft, and is replaced by a yellow, fatty-looking, soft, cancellous structure, filled with purulent contents and fat-globules.

Case 8.—Samuel G——, aged twenty-three, a ship's porter, living in the Borough, was admitted on the 24th of August, 1871. The patient's father died of consumption at the age of thirty-seven. His mother is healthy. He had small-pox three years ago, with which exception he has always been well and hearty till six months ago. He then received a blow on the neck, which caused an abscess to form on the right side. A week or two afterwards, without any previous injury, he experienced, in walking, pain in his right knee. No swelling was to be noticed. He managed to get about for a fortnight, and was then admitted into a provincial hospital. While in this institution an abscess formed on the outer surface of the lower third of the thigh. He left in ten or twelve weeks, with his leg bent a little, but able to walk, though with some pain, on the ball of the toe. Three or four days later he was admitted to Guy's Hospital in the undermentioned state:—The right leg is bent at an angle of about 100° with the thigh. The knee is uniformly swollen, and the patella immovable(?). Motion is impaired in the hip and ankle-joint on this side; while at the knee flexion is good, but extension very limited. Circumference of the diseased joint, 14¾ inches; of the sound one, 12¼ inches. The calf of the right leg is 14½ inches, as compared with the other, which is only 10½ inches. The skin of the knee and calf is natural. An opening exists on the outer side of the thigh, three or four inches from the joint, and a good deal of thick discharge comes from it. His general health seems fair. He has irregular scars of suppuration in the cervical glands on both sides of the neck.

The limb was placed on a splint and gradually straightened, and ultimately was placed upon a piece of perforated zinc; and a gum-and-chalk bandage was applied to keep the parts motionless and in place. The bandage was taken off on Nov. 27th, and the joint found in much the same condition as when he came in; discharge very profuse, and pain on the least motion excessive.

On Dec. 5th (the day before the operation) this was his condition:—The leg is straight, with the toes slightly turned inwards. The joint is still uniformly distended with fluid above and to the outer and inner side of the patella; below this bone all the tissues are much thickened and resistant. The patella is separated by fluid from the condyles, and can be easily felt beneath the skin. Pain is chiefly complained of when pressure is made upon the head of the tibia. No displacement backwards of this bone, and no enlargement of it or of the femur beyond the joint. The skin and subjacent tissues along the inner and outer aspects of the thigh are thick and brawny for some inches upwards from the knee-joint. Temperature 97·5° pulse 104; urine normal, sp. gr. 1010.

Amputation was performed by anterior and posterior muscle-flaps. The sciatic nerve-trunk in the posterior flap was cut short with scissors, and the vessels were secured by torsion. The operation was conducted under a spray of carbolic lotion.

On opening the cavity of the joint below the patella, some dirty-brown flaky fluid ran away. The synovial membrane is everywhere thickened, and, as you may see, is of a brown color and gelatinous consistence. The ligamentum mucosum and ligamenta alaria show this change remarkably well. At the lower part of the knee the tissues outside the synovial membrane appear to be in a very similar state. The semilunar cartilages are also soft and brown; and the crucial ligaments, though entire, are too readily cut with the knife. Over the internal condyle is some absorption of cartilage, as also over the corresponding part of the inner tuberosity of the tibia. Elsewhere they are normal, or nearly so.

The section of the bones shows slightly increased density near to the articular surface; otherwise they are healthy. A large collection of pus existed immediately above and to the outer side of the patella. This appeared to be in a synovial cavity, but not in the general joint cavity; probably some adhesions had shut off a pouch at this part, in which the fluid had accumulated.

In the soft parts around the joint several sinuses existed; one of which communicated with the joint by an aperture in the popliteal space; whilst a second passed through this region, and ran down the calf of the leg behind the tibia, forming a wide channel, which terminated about two inches from the heel: all communicated with the exterior by the opening on the outer side of the thigh.

In this case you will see, if you look well at the section of the joint, that, as in the first case, the disease is one of the synovial membrane chiefly, but that the bone also is attacked, and that in a way not to be easily recognised by the eye, though quite capable of manifesting itself by symptoms before the removal of the parts. The report says the bones show slightly increased density, and this, when attention is drawn to it, is quite apparent. Now, what does this mean? Well, I take it that it is the expression of inflammatory mischief, which has gone on to the formation of new bone. You may, perhaps, think that the bone looks healthy enough, and so it is. There is nothing in these parts not quite consistent with a good and useful limb; nevertheless, as we know that inflammation has existed here, and as, when contrasted with normal bones, we find these thicker and heavier, we are justified in concluding that they have suffered the same process, though, perhaps, to a less extent than the synovial membrane—that they have, in fact, under the stimulus of inflammation, undergone a nearly normal process, and have become hypertrophied. You will also notice as a feature of the case the very extensive disease of the soft parts which existed, one sinus being very wide and extending from the lower third of the thigh nearly to the heel.

CASE 4.—Lydia M——, aged twenty, was admitted Nov. 16th, 1871. Her family history is good. She is married, and has had one child eight months ago, which died three months after birth. She is now two months advanced in pregnancy for the second time. Fifteen years ago she had a fall on her right knee. It immediately became much swollen, but without pain or redness. The swelling has never since then quite gone down, neither has she ever been free from pain in the joint when exercised in the slightest degree by walking. She has, however, managed to get about with the aid of a stick. Eight years ago it became rather more than usually painful, and she was in another hospital for six weeks. The knee was lanced over the inner part of the head of the tibia, but no matter came away, and the opening quickly healed up. For four weeks the limb was put on a splint and painted, but this treatment was entirely discarded on her leaving. Thus she went on till eighteen months ago, when she slipped on some orange-peel, and fell on the bad knee. This caused her much pain, and the swelling increased. It has continued more painful ever since, and she has ceased to be able to walk on it. During the last fortnight, without any additional blow, she has had much more pain, especially on the outer side of the joint.

On admission her general health is good. The diseased knee measures 13¼ inches, the sound one only 12¼ inches in circumference. The right knee is bent at an angle of 115°, and cannot be straightened more. There is much thickening, and the skin is white and shiny. On manipulation there is no fluid in the joint, but exceeding tenderness over the external tuberosity of the tibia and head of the fibula. The patella lies on the outer articular surface of the femur. The long axis of the tibia runs up behind the long axis of the femur, and considerably to its outer side, so that the condyle of the femur is prominent on the inner side, and the outer tuberosity of the tibia on the outer side. Since her admission her one continuous complaint has been of pain, especially over the outer side of the joint. She sleeps hardly at all during the night, according to her own statement, and the sister of the ward confirms this.

In consequence of this severe pain, she was very desirous of having her leg off, and I therefore, after consultation with my colleague, Mr. Birkett, decided upon amputation. That operation you have just seen me perform; it was done in the usual way in which I perform all amputations in this region when possible.

I now pass you round a section of the joint, and you will see that there is no fluid in the cavity, but that the small interval between the bones is filled up by a soft pulpy material, which forms a uniting medium between the tibia and femur. The patella is pushed entirely on to the external condyle, and between it and the femur adhesions have formed round the margin of the patella, leaving the greater part of its articular surface free to form the wall of a small cavity in conjunction with the opposing surface of the condyle. This space contains some blood and lymph. The cartilage of all the bones is very nearly, though not quite destroyed, and the bone surfaces beneath are tough. The epiphyses are united, but distinct. For two or three inches above and below the joint the bones are soft, and have undergone some amount of interstitial absorption. In other words, they have become atrophied from disease, and the cancellous spaces are filled up with an opaque fatty material, such as used to be called strumous deposit. You will see that both in the femur and tibia the epiphysis easily breaks down, and is bloodless; while at the junction of the epiphysis with the shaft the bone is, on the contrary, very vascular, though the latter is also in a commencing state of the same degeneration.

The special point in this case that may here be remarked upon is, that our patient was two months advanced in pregnancy, and thus the question naturally arose as to whether she was in a fit state for operation. Many would have decided this in the negative, but I have seen so many facts which point in another direction that it seemed to me that her condition was no bar to operative measures. Bear in mind that she is at a very early period of utero-gestation, and if I had decided to wait she would have been committed to much pain and suffering for the remaining seven months of her pregnancy, and probably for some six or seven months longer till she had weaned her child. I had then to put the shock of the operation and the suppurative drain, but an immediate decrease of pain, on one side of the scale, and much and prolonged anguish on the other; to form, if possible, some estimate as to the probable results on her general health in either case, and to select that method of procedure which promised most favorably. You know how it was decided, and for the reason, as I tell you, that during gestation the reparative powers of a woman are very great. A most apt illustration of this point has only lately been in the same ward with this patient, in the case of Ellen N——, aged twenty-four, who was admitted on October 24th, being then five months pregnant, with an oblique fracture of the femur just above the knee-joint. We had much difficulty in getting and maintaining apposition of the fragments. However, this was accomplished, and she left the hospital walking on the limb on December 1st, or rather under six weeks from the date of her admission. Such cases as these are by no means unfrequently seen, and upon the like are founded the observations that have been made.

The general questions connected with the pathology of joint disease remain for consideration in my next lecture.

LECTURE IV.

ON FOUR CASES OF DISEASE OF THE KNEE-JOINT.

(*Continued.*)

Gentlemen,—You will not have forgotten that, at our last meeting, we were discussing the question of disease at the knee-joint as exemplified by certain cases which were at the time in the wards, and which most of you had seen submitted to amputation for the relief of that disease. To-day I purpose taking the same examples and continuing the subject, though my remarks will be, as I promised in my last lecture, of a more general nature, and will apply to the entire class of cases which involve the joint, whether primarily, by attacking its synovial membrane, or secondarily, by damaging contiguous structures.

In the four cases you will notice that the duration of the disease was in three out of the four a lengthy one—in No. 2 two years, in No. 4 fifteen years, and in No. 3 six months. In the first case only was it of very recent origin—viz., a month before his first appearance at the hospital. On this point our patients only confirm the general rule of history in all such cases—viz., that disease which necessitates the sacrifice of a leg is not that which comes under the notice of the surgeon immediately on its commencement, but rather that which is insidious in its onset and neglected in its treatment.

Many of you who are only here for a time, or who perhaps only see the patient just as you have seen these, when about to undergo the operation, fail to work out the problem of disease from the first beginnings to its end, and you do not realise other than the facts which come under your immediate notice. The patient may *tell* you that he has been ill for a certain number of months or years, that the joint has been injured by a strain or blow, or that the disease has come on quietly, as here, without any such exciting cause; but these facts, being matters of history only, do not stand out prominently as features of the disease. You look upon them more as interesting adjuncts. But is this a correct view? Does a history of a case add no more than interest to it? Does it not, or rather ought it not to picture forth the disease itself, each item in the one being expressed in the other? The two, it is evident, *must* be *exactly* correlated, could the patient, on the one hand, go with sufficient detail into previous facts, or we, on the other hand, minutely into the pathology of the case.

Stating things roughly, most of the instances of knee-joint disease have a history such as this. Inflammatory mischief is first set up in the joint, or in the neighboring articular ends of the bones; in other words, there is either ostitis or synovitis. If there is much pain, the patient may now lie up and rest the limb for a while, when it will probably get a little better. If pain is absent or slight, he walks about on the diseased member, and makes matters worse. In any case time is not allowed for complete repair; and so, though with occasional efforts at a restoration of the normal state, pathology will (so to speak) register the periods of rest, still the course of the disease is, on the whole, a progressive one for the worse. By-and-by the joint is so much damaged that work is no longer possible, and then comes the application for admission to a hospital. Prolonged rest is now enforced. Effusions, thickenings, heat, redness, &c., all probably disappear or become much less, and the joint assumes a respectable shape; but all this takes three, four, or six months, and by this time the patient is tired, and thinks he is well, or he goes out to make room for a similar or worse case. He sets off with a splint on to keep the knee quiet; but soon finding this inconvenient he leaves it off, and begins to do hard work with the unprotected and much damaged joint. All the old symptoms very quickly reappear, and in a few months he is back again at the hospital, and is patched up again, but only to relapse for the third and fourth or more times whenever he goes to work. In this way the case will generally last four or five years, and ultimately he will come under notice with fluid in the joint, with a thickened, gelatinous synovial membrane, with erosion of the cartilages and absorption of the bone beneath them, or perhaps with extensive disease of the bone, and only early joint disorganisation. By this time also his whole system will be shattered too; and if it so happens that he was not seen at the commencement of the disease, before it had told on his general health, it will be written down in the report (in defiance of the patient's own statement oftentimes that he was well and hearty some years ago) that he is "of a strumous disposition," or that "he is a scrofulous-looking man." The facts on this point in the cases before us are, that one is described as pasty and unhealthy-looking, with enlarged glands in the neck and groin, and with a history of blood expectoration; a second is phthisical-looking, and has also spat blood; a third has old cicatrices in his neck; and only in the fourth are the external appearances of favorable import. I admit that such observations may be used with perhaps equal force to indicate constitutional taint; nevertheless I have no hesitation in saying that in nine cases out of ten I do not believe in hereditary conditions, such as struma or scrofula, as a cause of joint or bone disease; indeed I think it is altogether the other way—viz., that these patients are originally healthy as they say they were, and that the pain, anxiety, inability to work for or to take food, and want of proper exercise, one or all of these so pull them down that the general or constitutional manifestations are the result of the local trouble. So in these cases, in the same way, glandular enlargement and hæmoptysis I believe to have been the consequences of a long-continued joint disease.

In Case 4, the section of the joint shows beautifully the progress of the mischief. The cartilages and synovial membrane are gone, indicating the original disease, the bones have then become united by a pulpy, semi-fibrous material which may represent an attempt at repair by false anchylosis; but, as I have already pointed out to you, this effort on the side of normal processes to put the parts in a healthy condition is at best a feeble one, and quite unable to withstand the much more powerful influence of inflammatory action set up by a violent blow, such as a fall on a hard surface. The period of active mischief dates, you may remember, from such an injury to the already weakened part, and probably the disease with atrophy of the bone which you see is the pathological registration of the circumstance. Further, it may be impressed upon you again that possibly slight causes produced exaggerated results from the altered state of the tissues. Next you will observe that in some of the reports it is noted that the limb is wasted; this implies one of two things, either absorption of the fat or atrophy of the muscle. The former may occur as part of a general emaciation and be rapidly produced, but not so the latter. Muscular atrophy shows that the joint has not been used for a long time. Ordinary uncomplicated synovitis will go on for years without much wasting—that is to say, synovitis in repeated attacks; but it is only when serious disease of the bone or cartilage exists that muscular degeneration is markedly present.

I beg you to remember this as one of the symptoms which will have some weight with you in modifying your treatment; at a first glance at your patient you may know that, with such wasting of the muscles, that case is much more likely eventually to require operative measures than one without this symptom. Another point of importance is that two of the four cases had a bent position of limb, while the other two had a straight one. The difference entirely depends upon the previous treatment they had undergone. You know quite well that the retention of the limb in a straight posture is much more favorable for after treatment than the other; that it is, indeed, the only reasonable mode of practice, and you must invariably endeavor to carry it out. No matter what the disease, or what its stage, or what the constitution or wishes of your patient, at all hazards you must put the limb in an extended position, or try to do so. If the disease has advanced too far you will not be able to effect this, and in such instances the position of the limb alone will often determine you on operating at once, without waiting to see what repair may do.

All cases of joint, as, indeed, of most other disease, are seen in their earlier phases in private practice, while only the more advanced cases are ordinarily found in the hospital wards; and as most of you are to take your part in general practice, remember that the early stage of knee-joint disease is the one in which care must be exercised to keep the limb straight. It is straight in the first place, and all you have to do is to keep it so. You well know that the flexors of the leg are more powerful than the extensors, and directly the joint is not used the more forcible tractors bend the joint. Exactly the same thing takes place in the arm, and this we see daily when the hand is closed in death. Remember these facts at the bedside, and in early joint disease base your treatment on them. I am sorry to have to say it, but it is a truth, that the greater number of bent knees are due in the first instance to the surgeon being too lazy, or perhaps too regardful of the wishes of his patient, to put on a back splint. He does not wish to make a fuss of what is considered a slight thing, or he is told that the bent position is more comfortable; and so the slightly flexed posture becomes gradually more bent, and permanently so; anchylosis, either fibrous or osseous, may take place, and what might otherwise have been made a useful limb has to be amputated or excised at the risk of life. Do not therefore pay any attention to the wishes of your patients on this subject; it is a delusion to think that the extended position of the leg is more painful, it is generally much less so than the flexed condition; it is more irksome no doubt just at first, but they soon get used to that, and very shortly see that, even in bed, they have much more freedom of motion than if huddled up without a splint and with pain at the least shake. I must, however, caution you that there is often a popular prejudice against straight splints, especially in uncomplicated cases of synovitis. Patients will say to you, "Ah, Sir, my leg has never been well since you put it up in them splinters; it has always been stiff." This, however, must not influence you in the matter of duty, which must here, as elsewhere, be performed quite regardless of what others say or think.

In two of the four cases in the description of the appearance of the joint you will find that attention is drawn to the prominence over the inner condyle of the femur on the inner side, and over the outer part of the head of the tibia on the outer side. It was supposed, however, that a similar deformity in the two (Nos. 1 and 4) was brought about by different existing conditions. In Case 1 you remember we were under the impression that we had to deal with a new growth in the head of the tibia, while in Case 4 the deformity was properly recognised as being due to displacement of the tibia from the femur, consequent on joint disease. Now that we have inspected the parts in both, we see that the diseased conditions were not dissimilar, but that a joint affection was present in either case. By comparing the two, it becomes possible also to solve the reason of our being led into error in the one case and not in the other, and I am afforded the opportunity of teaching you a most valuable lesson.

In Case 1 the deformity was so extreme that it at once suggested some new formation, and we, as it were, at once started with a somewhat biased mind in favor of a particular view. Other points in the case chimed in with the diagnosis, such as the age of the patient and the brief duration of the disease, and strengthened the opinion that had been formed. Looked at, however, by the after-light now thrown upon it, I must tell you that such facts should not have been allowed to distract attention from a much less dubious symptom as telling against our diagnosis, and that was the rotation of the foot. In Case 4 the distortion was not extreme, and the turning of the toes outwards was duly recognised and valued, and no difficulty was experienced in coming to a correct opinion.

You will, however, remember that it is quite possible for a new growth to exist, and also to produce some dislocation by a secondary invasion of the joint. It follows from this that no one symptom should ever be picked out from its surroundings, and the others made to agree with it; each must be properly weighed and estimated on its own ground, and not till this has been done may the probabilities of the case be summed up by a general review of all. This is the lesson which I wished to teach.

To proceed: in two of the four cases an observation is recorded to this effect, that "the patella is movable"; and with reference to this point it is worth while adding that the freedom of this bone does not necessitate early or trifling disease, as you must have seen in the limbs removed. If, however, the patella is *fixed* when all muscular action is done away with (mark well the qualifying clause), then there is beyond doubt advanced disease of the joint, with destruction of its cartilage.

We next come to the all-important question as to the reasons which induced me to operate in these cases, rather than to wait for nature's efforts under the influence of rest and generous diet. I have already touched upon it briefly in considering the individual cases, but it will not be amiss to impress my motives once again upon your notice in a rather more direct way.

Taking the cases together, three symptoms decided the question, though in Case 4 the first of these was absent. They were—first, the unhealthy aspect of the patient; secondly, the inability to move the joint; thirdly, the acute pain suffered in each instance. In the case of Thomas F—— this last symptom was so severe that the sister of the ward said to me before the operation, "Your patient has suffered most extraordinary pain both night and day; he will be so thankful to get rid of his leg." Indeed, this pain induced me, almost within an hour of his admission, to say that he must have something done.

With Case 4 the pain was also most extreme, and on this account she came into the hospital to have the leg off. But I must confess that such was her comely look and well conditioned state, and her freedom from all appearance of anxiety, that I felt inclined rather to doubt her statements; I could not believe that she was suffering so severely as to justify an operation, without some attempt at reparation by nature's efforts and rest. From the sister's report, however, it seems I ought not to have doubted her, for she absolutely hardly ever obtained more than a few minutes' sleep at one time, and the section of the joint shows quite enough disease of bone to account for more than she admitted. Pain is a most difficult thing to estimate as to the amount in different individuals; indeed, no comparison can be drawn between two or more cases. One man will die from pain which would exert but little injurious influence on another of less sensitive nervous organisation. Each case must therefore be judged on its own merits, always remembering the fact that a placid countenance may not invariably mean a freedom from anxiety and suffering. The structure which is the cause of severe pain when disease attacks a joint or its neighborhood is the thin layer of bone immediately next to the cartilage. Inflammation of the synovial membrane will give some pain, but it will not often be extreme. The articular cartilage, being destitute of vessels and nerves, can give no pain, though it used to be taught to the contrary. It is only the thin layer of bone, as I say, next to the cartilage on which the latter is placed, and from which it receives its nourishment, which in joint disease produces pain.

Having decided on an operation in each case, why did I then prefer amputation to excision? is the next point which comes before us. In Case 1, premising that the disease was a new growth, the idea of excision was of course never entertained; and had it been, the pain and enlargement of the joint would have necessitated amputation, and seeing that the man had heart disease and old phthisis as well, I should not have felt justified in proposing anything but that operation.

In Case 2 I had no choice; the man himself became his own adviser and repudiated the idea of excision; he said "I'll have it off." I have not yet found out the reason of his decision, but I remember that at the time he was in the hospital before, a boy was in the same ward who had had excision performed, and who subsequently had his leg amputated. A boy himself at the time, the sight of his comrade's suffering had doubtless led him to think that his might be a similar fate, and to avoid any such risk he would have it off at once. I did not wish to press the point, because of the enlargement of his glands and the feebleness of his reparative powers; I felt that, whatever the chances might be for him if amputation were performed, the alternative operation would certainly ensure him a more prolonged convalescence, and that very likely he might eventually lose his leg. The appearances pointed out to you remove all doubt as to the wisdom of my choice. In Case 3 the disease of the soft parts was too extensive to admit of excision affording much prospect of success; moreover, his general health was also against it. In Case 4 I wished to excise the joint; however, the patient and her husband between them took a very decided view of the case, and would allow nothing but amputation, and I am only thankful that she did not accede to my request, for there is no doubt that had I commenced the operation I should have had to abandon it.

Mind you, with the same amount of disease in the elbow-joint, I should still have performed excision, and my reasons you have heard in a former lecture.

In conclusion, one or two points in the operation call for notice. You will have observed that, except in one instance, where disease of the soft parts prevented its application, two lateral skin-flaps were made in amputating, and then a circular section of the muscles to the bone. My colleague, Mr. Poland, as your saw the other day, performs the ordinary flap operation. It is impossible, and indeed unnecessary, to institute any comparison between the two; either procedure would have been equally suitable, as far as the mere amputation was concerned, in all my cases. I prefer, however, my way because we practice torsion here; and I believe on this plan the twisting of the vessels is rendered much more easy of accomplishment. In muscle-flaps the vessels are *slit* often for some little way along their channel, and so torsion becomes a little difficult in its application. Remember this in practice; for no student leaving Guy's Hospital will, I apprehend, ever use, except, may be, in very exceptional instances, any other method of arresting hæmorrhage than this. I sometimes fear, indeed, that some of you may not know how to apply a ligature to an artery on the living subject.

But I have other objections to the flap operation as usually performed; and one of these you saw during the last amputation. I had to cut out a piece of the sciatic nerve from the flap. This is a proceeding first, I think, suggested by Mr. Hilton and it is done with the idea of obviating any liability to what is called "irritable or painful stump." All nerves, when they are cut across and the divided ends separated, become what is styled bulbous —that is to say, new tissue is thrown out in and around their sheaths from inflammatory action; therefore, in this respect, the circular and high division of the muscles would be no better than the other. But in the ordinary amputation, unless the nerve be cut across a second time, and dissected out of the flap—a method which I rather object to,—the bulbous ends become involved either in cicatricial tissue, and thus are pressed upon and made painful, or they are bound down at the extreme end of the stump, and become liable to pressure from various causes. A circular division of the muscles at the bottom of the wound allows the nerves to become swollen out of harm's way; and therefore they produce then no symptom.

We have been remarkably free from such after ill-effects as irritable stumps in this hospital, and I attribute it entirely to the cutting away of the nerve or making a high section of it. When, on the contrary, this is not looked to, perhaps in a few weeks, or it may be months or years, according as the scar-tissue contracts rapidly or the reverse, or pressure is brought to bear upon the stump soon or late, the patient will return to you complaining of agonising pain, and with tonic contraction of the muscles of the limb as its result, and oftentimes with some indolent ulceration about the cicatrix. The only treatment then available will be to submit the sufferer to another operation, opening up the stump and dissecting out the offending nerves.

In all these cases torsion was used, and in one of them—the first case in four years—some bleeding occurred a few hours after the operation; possibly this may have come from a twisted vessel, but I do not think so, and for the following reason. We have been using lately a fine spray of carbolic-acid lotion directed over the stump during the twisting of the vessels, and I have frequently noticed, and my dressers with me, that the backs of our hands, over which the spray plays in common with the flaps, have been quite benumbed at the termination of the operation; and if this be so with regard to parts covered with skin, much more so, I apprehend, will it be the case when the spray is brought into contact with the exposed muscles and vessels. It thus becomes easy to see that very probably some small vessels contracted at the time of the operation and ceased to bleed; but when the parts became warm again, and the muscular fibre relaxed, then blood could flow a second time from their open ends by simply pushing away by the force of its current any slight clot that might have formed during the period of closure. If you accept this explanation of the bleeding in this case, you will be ready to admit that any like instance with it must not be used as a fact against torsion by the supporters of any other custom. If the hæmorrhage occurs as the result of overlooking a vessel, it is quite certain that, surrounding conditions, such as cold or spray, being the same, the oversight would be just as likely to occur with one method as another. You must separate in your minds an occurrence like this, which is only a return of bleeding from temporarily closed vessels, within a few hours of, and the immediate consequence of the operation, from cases of true *secondary* hæmorrhage, such as occur after ligature at the time of its separation, or possibly shortly before. An instance such as this has not occurred to me once during the time that I have practised torsion, now some four years and more.

In one or two of our cases we had a little difficulty in arresting the bleeding at the time of the operation by its means; but this is a matter depending partly on the number of vessels discharging blood, and partly on the general health of the patient. With respect to the former element, in Case 4 it so happened that only one vessel required twisting, that one being the femoral; and a like absence of small vessels in the flaps has occurred to me once before. On the other hand, if the powers of the patient be feeble, there is often a trouble arising more from a general oozing from the whole surface than from any definite point that can be seized and closed.

Mr. Lister has discarded torsion, and now uses gut ligatures carbolised, which are cut off short and left in the wound. It is supposed that by some means they become absorbed, or possibly even revitalised by blending with lymph and new tissue which unite the flaps. I must confess, however, that I do not feel inclined to abandon torsion; for a method that can be applied for four years to all cases of hæmorrhage from wounds that have come under notice indiscriminately, which is quite free from any necessity of selection of fit and proper cases in order to ensure its success, and which, moreover, has never to my knowledge during that period been followed by *secondary* bleeding, requires no advocacy by word of mouth. The fact need only be stated to demand its trial by those who have not yet attempted it, and to ensure a continuance of the method by those who have commenced its use.

You will have noticed that I now perform all amputations under the antiseptic method with carbolic acid and all its paraphernalia. Last year I had eleven amputations of the thigh—all done without any such precautions; now all are performed with them. I hope by this means to arrive in a year or two at some definite conclusion on the subject, and to form some rule for future practice. Possibly, if hæmorrhage again takes place soon after the operation, we may have to abandon the spray for some other means of disinfecting the surrounding atmosphere.

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ABSTRACT
OF THE
Croonian Lectures
ON
DISEASE AND ITS REMEDIAL TREATMENT.

Delivered at the Royal College of Physicians, March, 1872.

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LECTURE I.

MANY writers have tried their hands at defining disease. An exact definition, or at least one that shall meet with universal assent, is perhaps impossible in the present state of knowledge. Opinions differ with regard to points of fundamental importance, and the limits of health and disease are not well marked. Mr. Simon considers nearly every disease to be the normal result of a special cause, the only thing abnormal in each case being the presence of the cause; and that in the process itself there is nothing supernatural or mysterious. According to this view the biography of every disease comprises some specific cause and certain resulting phenomena—vital, chemical, mechanical,—which produce the signs by which we recognise the disease. For instance, in scabies, or tinea tonsurans, the cause is obviously the parasite; and the phenomena resulting, certain local inflammatory conditions fairly characteristic. Both of these must be included in the idea of the disease, since neither alone suffices. What is meant by the term "scarlet fever"? A specific contagious particle gives scarlet fever, and causes certain phenomena. But the poison alone is not the disease; neither are the phenomena, which, or others apparently identical, are sometimes combined in a person not suffering from this exanthem. Here the notion of the disease will obviously include both symptoms and cause. So, too, with acute rheumatism, and so with the neoplasms, tubercle, cancer, &c. Thus we may define disease as a complex of some deleterious agency acting on the body, and the phenomena, actual and potential, due to the operation of that agency.

The difficulties besetting the question, What is disease? have not yet been explained away. Are we to regard as diseases congenital malformations, inherited tendencies, and the degenerative changes that take place in various organs as we grow old, and that ultimately cause death? Here disease and health blend, and the boundary line is theoretical, if not arbitrary. Congenital malformations may be looked on as diseases from a double point of view—as the results of abnormal causes, and as the starting-points, in many cases, of grave morbid processes. In inherited proclivities, it might be argued, a different cause is at work from that which produces the resulting disease in those of healthy ancestry, and therefore cause must be eliminated from our conception of disease. But probably in each case the essential cause is the same, the system in the one being more ready to respond than in the other. Secondly, in some other forms of disease, especially forms of inflammation and tumours, it may be said that the cause soon ceases to exert any specific influence, and so the morbid phenomena alone must be held to constitute the disease. We are, it is true, apt to consider and speak of these processes apart from their cause. Nevertheless, in those forms of inflammation to which we give a specific name, we recognise the cause as essential to our conception. Examples of this might be multiplied indefinitely. This view can only be called in question in the case of those inflammations that are due to the local operation of mechanical and chemical causes; but even there the cause modifies the result, especially when that cause is in constant operation. A somewhat similar difficulty surrounds the subject of morbid growths, especially those that are innocent in nature.

Thirdly, our conception of disease involves not only the pathological changes springing from morbid causes, but these involve others, and those others a tertiary series, and so on continuously. Examples of such complex consequences are to be seen in rheumatism, syphilis, &c. Where is the line to be drawn between the primary and secondary disease? Every day we speak of as diseases conditions, such as ascites, jaundice, dyspepsia, which are really symptoms of some deeper-seated affection; and if this conception of disease be the true one, every separate consequence should be deemed a disease, taking into consideration its cause as well as its presence. When the consequences are numerous and complex, as in scarlet fever, which are we to regard as constituting the primary disease? The answer is—those morbid conditions which spring directly from the operation of its contagium, and not those which survive the disappearance of the contagium, save those which, having originated under its influence, are in process of amendment at the time of its dissolution. The practical difficulty of determining the exact period at which the primary disease ceases to operate does not involve the essential truth of the proposition. Remains of previous disease in the body which have become inert—as calcified hydatid or tubercle—can only be regarded still in the light of diseases when they act mechanically or otherwise as the cause of other phenomena. The definition given already is thus as applicable to disease in the more comprehensive sense of the word as to the parts of each disease taken separately.

Without attempting anything like a complete classification of disease, certain groups may be enumerated into which most diseases with which we are acquainted seem naturally to fall. These are (1) functional diseases, the result of changes of intimate structure and chemical changes unknown to us. (2) Those originating in excess, deficiency, or improper quality of nourishment. (3) Those due to retained excretions. (4) Those due to the action of poisons, vegetable, animal, and inorganic. (5) Those originating in mechanical causes, as narrowing of tubes, &c. (6) Parasitic diseases. (7) Inflammatory affections, tubercle and other proliferating neoplasms. (8) Diseases originating by contagion.

The mechanical group, which is the most simple, may be illustrated by the hypertrophy and dilatation of the left ventricle secondary to aortic constriction, and the corresponding changes in the portion of the bowel above a stricture. In all such cases the muscular wall becomes hypertrophied, not in consequence of any conservative instinct, but according to the ordinary physiological law that muscular tissue tends to become more powerful within certain limits according to the force required. It is conservative by accident only, and neutralised almost from the beginning by the weakening dilatation which accompanies it. In an aneurism of the aorta, opening by a small orifice into the vessel, the deposit of fibrin, far from being an effort of nature to remedy the disease, is an accidental consequence of the stagnation of blood in it, and of the roughness of the wall. It does not occur in the equally dangerous fusiform and saccular aneurisms, and is due to the same process which in a vein not uncommonly leads to death. In the case of parasitic diseases there is no provision for the elimination or destruction of the cause of the disease; many of them increase and multiply indefinitely, and the only way Nature relieves herself is by the accidental discharge of those on the surface, with the epidermis constantly shed off, or by the death of those occupying internal organs.

Poisons coming from without which are easily diffusible, entering by the alimentary canal or lungs, or by inoculation, pass directly into the veins; but probably all that consist of not readily soluble particles enter indirectly through the lymphatic system, and get into the blood only after a comparatively longer period. Of this syphilis affords an illustration, and so also probably do all the contagious diseases. Those contagious particles which enter by the lungs do not seem to reach the air-cells, but become attached to the mucous surface, and pass into the lymphatics, and thence into the bronchial lymphatic glands. In cholera and enteric fever the contagium passing into the stomach and bowels is absorbed doubtless by the columnar epithelium, and passes, not into the vessels, but into the lymphatics. Occasionally, it may be, the contagium passes directly into the blood, and is more rapidly diffused.

The blood itself is an ever-changing fluid. It constantly tends to get rid of deleterious matters which have entered it, but we cannot expect it to get rid of matters accidentally introduced so rapidly as of those naturally entering it, as urea or bile. The circumstances determining removal seem to be the solubility and diffusibility, or elective affinities with certain organs, as lead tends to be deposited in the muscles of the arm and intestine. The germs of tubercle and carcinoma in the circulation manifest an elective affinity for certain organs. The same is the case in the various forms of contagious diseases. One poison affects one part, another another, and this whether recovery or death be the final issue. In many of these cases it cannot be contended that there is any special eliminative effort of nature to get rid of the matters acting injuriously on the system. It is certainly not so with lead; and if it be contended that in the diseases of limited duration the contagium escapes from the blood through the connective tissue to the cutaneous and mucous surfaces, it may be objected that Nature is clumsy for good, and powerful for evil; for, instead of neutralising the poison on the threshold, she lets it run riot in the system, and only allows it to escape after it has effected all the harm possible, and the power of imparting disease to others has been increased a millionfold. In some diseases, as tubercle, cancer, syphilis, hydrophobia, glanders, there is no attempt at elimination.

In conclusion, it may be admitted that poisonous matters of some kinds tend speedily to be discharged; when they have affinities for certain parts, they tend mainly to be deposited there, irrespective of good or evil, and subsequent morbid processes depend partly on position, structure, and functions of the organs in which the deposit has taken place, partly on the nature of the deposit, whether mechanical or vital, and in the latter case on whether it acts as a ferment and rapidly influences the whole system, or whether it takes root in the tissues and produces proliferation or a tumour.

Lecture II.

The art of prescribing, though unquestionably very important, rather hinders than advances therapeutical knowlege. Such rules as those of Dr. Paris—"Pharmacologia"—are more adapted for those who believe that they understand the action of medicines in relation to processes of disease than for investigators. In proportion as we combine we deprive ourselves of the power of obtaining precise acquaintance with the individual drugs we employ. But the difficulties in ascertaining the effects of medicine on disease are almost insuperable, and not limited to those which arise from the art of prescribing. One of the greatest sources of error lies in the fact that all diseases tend to run a certain course, varied by constitutional peculiarities and the severity of the attack; and we are apt to ascribe the favorable result to our remedies, and the unfavorable course to the disease. In chronic illness slight variations are put down to the credit of medicines employed, of which they are often quite independent. The danger is increased when we fail to recognise accurately the nature of the disease we are called upon to treat. How often is a specific fever cut short by an emetic, which fever never was; the action of an hypertrophied heart reduced, when only functional disturbance was present. Again, various forms of hygienic treatment are usually combined with medical treatment, and the resulting improvement ascribed to the latter when it is really due to the former. But perhaps the greatest impediment of all is offered by the various theories of disease which spring up from time to time and influence both our judgement of the nature of diseases and our mode of action; such, for instance, as is exemplified in the bleeding, stimulating, and eliminating plans of treatment. Each theory received the credit of the recoveries that were certain to occur under, or in spite of, the corresponding plan of treatment, no attempt being made to ascertain the average duration of the disease, the percentage of deaths, or the frequency of secondary affections. Yet we find it easier to believe in change of type of disease than in change in our own opinions; and theories are invented to account for such a change, founded on imperfect knowledge, and never shown to be true in any large sense.

Functional diseases constitute a large proportion of the maladies with which we have to do. In a large number, the nervous system, and especially the vaso-motor portion, is involved; but how, and whether primarily or secondarily, is beyond our knowledge. We remain perfectly ignorant of the fundamental causes on which the phenomena depend, and of their intimate pathology. It is not to be wondered at, therefore, that the curative treatment of functional diseases should be for the most part empirical and unscientific; and, when scientific, all the science is bestowed on counteracting or relieving secondary phenomena or symptoms; and here, it must be admitted, we are far from being powerless for good. Yet on many we have no influence, and on many—as chorea—our curative power is illusory. The principles of treatment of diseases due to the action of poisons, and of excessive or deficient or improper nourishment, are better understood, but we often fail to bring them into successful operation from many causes. Of the class of diseases due to the retention within the system of matters originating in the waste of tissues, and destined normally to be removed by the excretory organs, the principles of treatment may be simple, but the means of carrying them out are by no means equally simple. It is often impossible to restore the organ to a healthy state. This is less important, however, in the case of the liver than in that of the kidney. In the latter, besides the usual measures, it is important to check as far as possible the formation of urea &c.; hence the diet should be carefully regulated, and not allowed to exceed the actual requirements of the system.

Mechanical diseases we can only hope to relieve or cure by the means which Nature herself employs, often in a blind manner, and productive of more harm than good. Our best chance of relief lies in moderating rather than stimulating her efforts—in relieving violent spasm by sedatives, rather than by increasing the muscular effort. The impaction of a calculus in the ureter or of a gall-stone in the bile-duct illustrates this. Nor is the case of the heart in valvular disease very different. The dilated and hypertrophied ventricle acts more powerfully, though with less effect, and soon runs into purposeless violence, palpitates, and acts irregularly. Our object must be, not to stimulate the heart, but to allay its irritability, and for this quiet of body and peace of mind are more efficient than any medicine. In justice to Nature, it must be admitted she sometimes makes a compromise with these mechanical diseases: the gall-stone becomes lodged in a cyst—the stricture may be partially destroyed by ulceration; but these are processes we can neither initiate nor aid. Aneurism, another of the mechanical diseases, can only be cured by the deposition of fibrin within the sac —a process which probably no medicine is capable of bringing about, except that which does so by promoting tranquility of circulation.

Parasitic diseases affect different portions of the body, and necessarily need different kinds of treatment. Wherever we can reach them we have the power of destroying them, and it is very questionable whether their eradication is facilitated by any indirect treatment, tonic or other. It is equally doubtful whether the parasites which are out of our reach can be affected by the remedies which have been administered in some cases with alleged benefit. It is hardly more reasonable to expect iodide of potassium or kamela to influence echinococci than to endeavor to remove itch by a course of brimstone and treacle, or ringworm by diuretics.

In no subject has fashion had more influence than in the treatment of inflammation. The obvious differences between the process in different forms and organs is a justification for some of the differences in treatment. The measure of success which has attended the most different systems indicates that the essential course of the disease, its progress towards recovery, was independent of the specific measures employed, which probably effected nothing beyond a little delay or hastening of the progress. So much has this sceptical view found its way that a considerable number have adopted the expectant or "do-nothing" treatment, especially Dr. Hughes Bennett, of Edinburgh, and if statistics mean anything they mean that his success has been remarkable. During the last ten years, Dr. Bristowe has given up the special treatment of pneumonia, and patients have got as rapidly well, and have recovered in as large a proportion, as when special treatment was adopted; and during the last few years he has refrained from alcohol, except during the period of convalescence with equal success.

Acute rheumatism, like pneumonia, arises from exposure to cold, but differs in its less definite course. There is strong reason to believe that each local inflammation acts as the starting-point of fresh complications. Numerous systems of treating it have sprung up, and each has been attended with a fair amount of success. It is even more difficult than with pneumonia to judge of the influence of treatment. A specific remedy must be one that curtails its course, and prevents heart disease; but a little experience shows that no such remedy has yet been found. The value of statistics in settling the point at issue is doubtful. The tables of Sir. Wm. Gull and Dr. Sutton have been rearranged by Dr. Fuller to prove that they are wrong. Dr. Bristowe's experience with regard to acute rheumatism is the same as with pneumonia. In many other inflammatory affections, in common catarrh, psoriasis, carbuncle, and herpes, for instance, it is doubtful if we can do more than in acute rheumatism or in pneumonia. Psoriasis is especially regarded as a curable disease, but its course is generally intermittent, and many reputed cures by arsenic &c. are probably due to the fact that the patient was put and kept under its influence until the normal time for the disappearance of the rash.

Yet there can be no doubt that we have it in our power to influence very materially the course of these and many other inflammatory affections. If we cannot cure, in the sense of stamping out the disease, we can at least improve general health, prevent exposure, and relieve symptoms or complications, and so by judicious exercise of art, give relief, hasten convalescence, and save life.

LECTURE III.

Effusions of various kinds—of blood-serum or of the products of secretion—fall naturally into one or another of the groups of diseases enumerated in the first lecture, but their treatment deserves special consideration. Its chief difficulty resides in the fact that their causes are, for the most part, in permanent operation, organic, and incurable. Organic disease of the kidneys, emphysema of th lungs, valvular disease of the heart, are condition not only incurable but progressive, and permanen cure of the resulting dropsy would seem out of the question. Still many inconveniences and dangers may arise from the mere accumulation of dropsica fluid, and its removal, even temporary, is desirable by puncture or by action on the various emunctories by which fluids are usually removed from the system. Of these the lecturer has most faith in diaphoresis, least in diuretics; never having seen anasarca removed by diuretics alone. In no case of localised œdema do diuretics appear to have any effect, and yet if they are potent to relieve general dropsies they ought to be able to relieve local dropsies; if able to remove œdematous fluid from both legs, surely they should be able to remove it from one. That purgatives can remove much anasarcous fluid is beyond doubt. During the cholera epidemic of 1854 a man with general anasarca one morning called attention, with much satisfaction, to the disappearance of his dropsy; in a few hours he died of cholera. Diaphoresis the lecturer has used with great success, but he has as little confidence in medicinal diaphoretics as in diuretics. The hot-air bath, however, has yielded excellent results in dropsy from both renal and heart disease. But in these, as in so many other cases, health-producing hygienic conditions co-operate with the special treatment, and are concerned in the result.

Of dropsical effusions within cavities ascites is one of the most important. Here it is very doubtful whether diaphoretics, diuretics, or purgatives have any beneficial effect alone. If those measures will remove fluid from the peritoneum or the pericardium, they ought to be capable of removing it also from an ovarian cyst or a hydrocele. They probably do good, in cases where benefit follows their employment, by improving the general health. Uncontrollable diarrhœa is a not uncommon attendant upon ascites, and is, for the most part, of bad augury. But in most cases of ascites, improvement of the general health by tonics, iron and quinine, yields good results. Ten cases treated thus were communicated by the lecturer to the Clinical Society, in nine of which the dropsical effusion was lost after tapping. In cases of general dropsy also tonic treatment is of great benefit.

Neoplasms form a large proportion of the diseases we are called upon to treat. Some disappear spontaneously; others are influenced by medical treatment. Most innocent tumours are quite beyond reach of remedies taken into the system. Iodine long enjoyed a reputation for the cure of goitre, but when it was discovered to cure goitre, it was discovered equally to cure the male of his testicles, the female of her breasts. Long experience has shown that the belief that it could influence the latter is altogether illusory, and it may be asked whether anyone believes that goitrous necks are less common in Switzerland than before iodine was discovered to be a specific. The lecturer has seen no case of goitre in which its use was followed by any satisfactory evidence of amelioration.

Passing over the malignant growths, we come to tubercle, in some respects a malignant disease, but differing from the diseases allied to carcinoma in the remarkable proneness of its growths to undergo degeneration and become inert, and in the occasional arrest of the disease. Drugs, probably, have no direct influence upon it, but there seems no reason to doubt the effect of cod-liver oil and tonics, which improve the patient's general health, and so favor the tendency of the disease to die out.

Syphilis forms an important link between tumours and the exanthemata, but differs from both in its inherited communicability. No treatment in the primary stage will prevent further symptoms. Mercury and iodine seem to have an almost specific influence upon it, but often with (and sometimes even before) the discontinuance of the medicines the symptoms recur. On the assumption of the nature of the disease and its diffusion from fresh centres, given in a previous lecture, it is easy to believe how, after the most thorough apparent cure, the germs of the malady may be left behind in some remote corner, ready to prove the starting-point of a fresh outbreak. The transitory nature of the influence of the most powerful remedies upon it resembles the action of some other specifics, as quinine and arsenic in ague, colchicum in gout, which seem to have little or no power to prevent a recurrence of the maladies over the individual attacks of which they exercise so marked an influence.

For the treatment of the specific infectious fevers innumerable methods have been recommended. Of late years the conviction has become more and more prevalent that we are unable by our remedies to cut any of them short. We can only sustain the patient, and prevent or relieve unfavorable contingencies.

All these diseases are obviously preventable. Few, if any, arise *de novo;* and our power of preventing their spread is increased by a knowledge of their actual history and the sources of their contagion. Such, for instance, is the case with syphilis; and we cannot but lament the educated bigotry which is opposing, with so much earnestness and success, the efforts of sanitary authorities to check a disease which is not only a source of the direst misery to those who contract it through their own misdeeds, but which visits the sins of the fathers upon the children to the third and fourth generation. When the origin of cholera was unknown, no efficient mode of staying its spread could be proposed. Now that we know that the contagium passes almost entirely by the evacuations, and that contaminated water or food communicates it, the requisite measures become apparent. But although we are powerless to cut these diseases short, it is probable that by careful attention during their earliest stages we may render the attack milder; and throughout its course we may often diminish its severity by attention to hygienic measures, and may do much to relieve functional disturbances, whether an essential part of the disease or accidental complications. Again, in most there is a tumultuous development of protoplasm, attended with the production of much heat and rapid destruction of the albuminous tissues. A temperature much exceeding the normal is certainly injurious, and increases the severity and danger of the disease from which it results; and its reduction by the tepid or cold bath is often of great advantage. The retention of effete matters in the blood cannot but be detrimental, and benefit may result from stimulating the sluggish excretory organs to increased activity. It has been assumed, partly from the tendency of the period of greatest severity of the attack to be attended by some discharge termed "critical," partly from the fact of the occurrence of a rash on the skin or mucous membrane in which the contagium thickly resides, and from the fact that many poisons pass out by certain paths, that there is a natural elimination of poisonous material, and hence that a cure may be promoted by the encouragement of that elimination. The theory of elimination is an old one, which has obtained special interest from Dr. Johnson's views on cholera. It must be admitted that if cholera depend on a poisonous something which multiplies in the blood, and thus causes the characteristic symptoms of the disease, and if recovery depend on the separation of this at the mucous surface of the bowels, it would be desirable to adopt measures to promote such separation, and undesirable to do anything calculated to arrest it. It may, however, be doubted whether this chain of hypotheses is sound. The modes in which the organised particles constituting contagium become introduced into the blood, and their numbers augmented and kept up in it, have been already considered, and it has been pointed out that their residence in it must be of short duration, since they cannot be detected in the circulating blood by the microscope, and since the blood is rarely capable of inoculating the disease upon healthy persons. This renders it most improbable that the characteristic symptoms are due to the presence of the poison in the blood. In carcinoma and syphilis it is certain that so long as the germs are retained within the vessels they cause no symptoms. It is not denied that the blood becomes much affected in many of these diseases; but it is contended that these changes are not, except in slight degree, specific, but are due to the rapid destructive processes going on in the tissues, and the insufficient removal of their products. It seems clear that the characteristic symptoms of fevers do not arise so long as the specific poisons are merely circulating in the blood, and that poisonous or foreign matters in the circulation tend rapidly to leave it. Whether their escape into and operation on the solid tissues is beneficial, or injurious, or neither, depends on other circumstances. It is so with inorganic poisons, with substances given as medicines, and with the germs of syphilis and tubercle. Is it not so with the infectious diseases, measles, small-pox, &c.? All that renders these diseases characteristic and dangerous to the person himself or to others, is in intimate relation, not with the processes within the bloodvessels, but with those occurring externally to them, and dependent on the escape of the contagium from the vessels. In small-pox the patient receives the poison, and after a time there occur the peculiar symptoms and the rash. On the eliminative theory it is assumed that the poison discharged from the blood produces the cutaneous inflammation as a step towards its final discharge from the body. Obviously this is not the fact. No doubt the poison of small-pox escapes and produces the inflammation of the skin; but it is at least equally certain that the skin thus inoculated becomes a most active manufactory of the virulent poison. It can hardly be argued that all the virus of each pustule is from the blood. Clear evidence on this subject is afforded by the phenomena of inoculation. The primary pustule goes through the same stages as the others, no doubt from the direct multiplication of the poison inoculated, and it follows almost as a matter of course that the other pustules are due to a similar local multiplication of the poison. If this be true of small-pox, surely the same is true also of scarlet fever, measles, and probably also of whooping-cough and mumps.

Are not all these considerations applicable with equal force to the explanation of the phenomena of cholera? A specific poison enters the blood which has a marked tendency to become eliminated by the alimentary mucous surface. There is no proof that this acts injuriously while retained, and, judging mainly by analogy, it may be assumed that it produces its characteristic effects when it escapes from the blood and acts on the solid tissues. That it undergoes development out of the blood is established by experiments on animals, and by the experience that cholera evacuations become more and more infective up to the time at which, after a few days, they are rendered inert by the advance of putrefactive changes.

A further argument that the contagious particles multiply in the blood or organs, and then sow themselves and germinate in the tissues outside the vessels, rather than are merely eliminated thus, is shown by the fact that the foci of contagion never reinfect the individual. The inoculated pustule of small-pox throws out effective contagious particles into the system. Similar pustules formed at a later date have no such ulterior influence. Absorption by lymphatics is a passive and unselective act, and so no doubt absorption takes place from the secondary pustules as from the first; but whatever is thus absorbed remains inoperative. It may be said that this is connected with the protective influence which follows an attack, but this consideration does not explain the phenomenon. The essential feature is that the duration of the disease is limited—a limitation not determined by the tendency of the vastly multiplied poison to pass off by the emunctories, but due to some occult influence exerted by its development which renders it ineffective for further aggravation or maintenance. The cessation of the disease does not depend on elimination, but on the supervention of a condition which renders the poison inoperative.

These conclusions are incompatible with the value of the eliminative treatment of contagious diseases, and the results of experiment are scarcely favorable to it. Where it can be best applied—as in the exanthemata, which develop chiefly on the skin—measures to promote it would tend rather to increase the rash; and every physician, as a matter of course, treats the patient on the opposite principle, and discourages elimination. Does anyone think of eliminating diphtheria by encouraging the development of false membrane? Yet, if the eliminative theory be true, this should be the proper mode of treatment. There are no sufficient grounds for regarding the discharges of cholera as more eliminative than the eruption of small-pox or the membrane of diphtheria: as the latter is the chief source of danger, and its development is to be moderated as much as possible, so the alvine discharges are detrimental, and to be restrained. The occurrence of rapidly fatal cases of cholera without much evacuation neither proves that such discharges are not a source of danger nor that they are eliminative. Such cases bear the same relation to ordinary cases as do those terrible cases of small-pox, measles, and scarlet fever in which the patient is struck down by the intensity of the attack, to the more common forms. In recorded experience there seems to the lecturer to be little evidence in favor of the purgative plan of treatment, which is so opposed to these theoretical considerations. He doubts whether, in the premonitory stage, it is possible to prevent the subsequent full development of the attack, or materially influence its progress, by either purgatives or astringents; and whether, in the later stage, any benefit can be derived from opening medicine to remove poisonous matter from the bowels—a proceeding which may be compared to the removal of the scabs in variola and the squamæ in scarlet fever. In the premonitory stage, the poison is in the system, and is not in the bowel, and a purgative is no more likely to expel it than is a hot bath to remove the poison of variola through the skin. In the second stage absorption is impossible, and it is obviously useless to give medicines which only operate after absorption. It is as reasonable to hope to check or hasten the discharge by remedies acting locally, as to hasten or retard the downward rush of a mountain torrent by throwing astringent or purgative medicine into it.

Though a large number of diseases are incurable, the skilful physician has numerous opportunities of exercising his skill for the patient's benefit by guarding against and averting complications, and treating as they arise those symptoms which distress the patient or endanger life. He may often be able to cure the patient, though he cannot cure the disease. Further, in many diseases, especially in functional ones, it is absolutely within our competence to cure the disease. In all, regulation of the emunctories, strengthening of the nervous system, improvement of digestion by tonic medicines and regulated food, are of the highest importance. By such means patients are often saved from death during the severity of disease, and brought into safe convalescence.

In conclusion, Dr. Bristowe observed that he feared that much that he had said was little likely to command acquiescence. It was easier to pull down than to build up, but he trusted that he had used no expressions which might have seemed disrespectful or contemptuous towards the opinions of others. He certainly entertained none towards those which he had most freely criticised. Those who held them were as entitled to do so as he was to hold his, and he had no claim to be the judge between himself and them.

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Clinical Lecture
ON
FLEXIONS OF THE UTERUS.

Delivered at the University College Hospital,

By Graily Hewitt, M.D., F.R.C.P.,

Professor of Midwifery and Diseases of Women at University College, Examiner in Obstetric Medicine at the University of London.

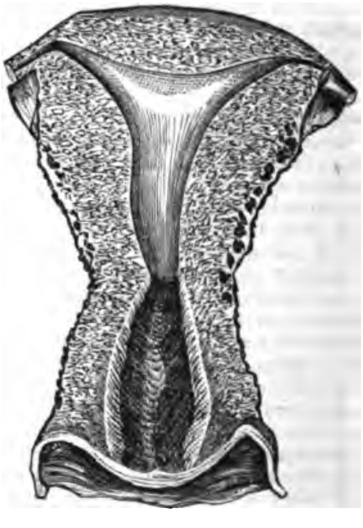
LECTURE I.

Gentlemen,—The subject of uterine flexions is a comparatively new one in relation to uterine pathology. The existence of flexions has been recognised, but the doctrine of the preponderating importance of flexions in relation to uterine pathology, which it is now my object to inculcate, this doctrine is new. That flexions constitute the most frequent as well as the most important of the diseases of the uterus, is demonstrable by statistics of the practice in the department for diseases of women in this hospital, of which I have accurate records for a period of over four years, and shall lay before you later on.

Another definition may be given of flexion—*disease of the uterus the essence of which is a change of shape.* That is, on the whole, perhaps, the best method of formulating the opinion which I wish to express, and it conveys the whole idea. You will observe that in this definition it is not implied that flexion is the only thing which is important, or that it is the only condition which is or may be wrong in the uterus; but it is implied that flexion is the most important element in the matter.

As an introduction to what has to be stated later on, we may consider for a moment the anatomical relations of the uterus, predisposing, as it can be shown they do, to a change of its shape. Consider for a moment the manner in which the uterus is suspended in the pelvis. The upper part of the uterus, which is the main part of it, has no attachments, posteriorly or anteriorly, sufficient to absolutely maintain it in one position. The uterus is suspended in the middle of the pelvis, but it is suspended in such a manner that a degree of motion is allowed anteriorly and posteriorly, and there is much more motion allowed at its upper than at its lower part. It rarely moves from side to side, because laterally there are attachments which bind the uterus pretty effectually to the sides of the pelvis. And with reference to the cervix of the uterus, this is kept in its place pretty effectually by means of its connexion with the bladder and the vagina at the upper part. The axis of suspension of the uterus, as it has been very properly designated, is represented by a horizontal line passing through the uterus from side to side, and about the situation of the internal os uteri. This leaves about half of the uterus above this part and about one-half below it. The lower half is more effectually fixed, and the upper part is very ineffectually fixed, except in reference to lateral motion, which is not allowed except to a slight degree. Now what are the hindrances, it may be inquired, in reference to change of position of the upper part of the uterus? That is the part with which we are mainly concerned. It is quite obvious that the broad ligaments, which are the lateral attachments of the uterus, impede lateral motion. The round ligament is an important structure. It is attached to the upper part of the uterus on its anterior aspect; and when the round ligaments are intact, the motion of the upper part of the uterus backwards is, to a certain extent, and to a certain extent only, controlled. They seem to be so placed as to be intended to prevent this occurrence. With reference to the motion of the fundus uteri forward, there appears to be very little hindrance. The bladder is the chief obstacle to anterior motion of the uterus. The bladder, when distended, prevents the fundus uteri from falling forwards; but of course the bladder is not always full, and when it is empty there is nothing to preserve the uterus in its position—nothing of a special character, that is to say. Another element in the case next to be mentioned constitutes an important hindrance to anterior or posterior motion of the fundus uteri; it is the resistance of the uterus itself. The healthy uterus is an organ having very thick walls. The cavity is exceedingly small in proportion to the thickness of those walls, as will be seen on reference to the diagrams (see Figs. 1 and 2). The thickness of the walls of the uterus, as is very evi

Fig. 1.



dent, forms a very important element as a resistance to alterations of the position of the fundus uteri. It is apparent that if the cervical part of the uterus be held pretty firmly in its place, the position of the upper part will be well maintained by the thickness of the walls. And thus this rigidity of the uterus itself—as it may be termed—unquestionably constitutes a very powerful hindrance to displacements of the fundus uteri. Subsidiary to these hindrances already mentioned must be added the general connexion of the uterus with the adjacent viscera by means of bloodvessels and cellular tissue surrounding the bloodvessels, also the peritoneum. But it cannot fail to be observed that these subsidiary means of attachment of the uterus apply, for the most part, to the lower part of the organ, and they do not, excepting indirectly, help to fix the upper part of the uterus. The Fallopian tubes aid very little in preventing anterior or posterior motion. This very delicate adjustment of the uterus in its position in the pelvis—for it is a very delicate adjustment—is a physiological necessity. It would be impossible otherwise for the uterus to expand, and it would be impossible for it to undergo those changes of position which are involved in the existence and course of pregnancy, were it not for the fact that the attachments of the uterus are such as now described. It is the upper part which undergoes this expansion in pregnancy in order to fit it to become the residence of the fœtus, and this part is left comparatively free. But for the physiological necessities involved in the propagation of the species the uterus would doubtless have been fixed much more firmly and with less liability to the alterations of shape and position.

We now come to consider the *causes* of flexion of the uterus. If we wish to trace the pathology of any disease, we search out its beginning; we endeavor to ascertain the point at which the healthy action passed into one unhealthy or diseased. When the disease is far advanced, it is often very difficult to determine its real etiology and pathology. Various complications have been by that time added, and various secondary effects, which mask the primary one. For this reason I just now directed your attention to the shape and position of the uterus, and the methods by which it is sustained therein. A slight failure in any one of these conditions of equilibrium is really the beginning of a flexion.

We may usefully divide the causes of flexion into two—the predisposing and the exciting causes.

One of the most important of the predisposing causes is *an unhealthy state of the body generally;* another is a previous pregnancy. These are the most important predisposing causes of flexion of the uterus; and I will say a word or two respecting each of them. The tissues of the body, imperfectly or badly nourished, are relaxed and wanting in tonicity. The circulation in the bloodvessels is retarded under these circumstances—it is sluggish and imperfect; and the tissue-changes take place with greater slowness than under ordinary circumstances and in a state of health. The effect of this state of things upon the uterus is most marked: it increases in size; its circulation becomes slow; and, as a necessary mechanical result of this, flowing from the considerations which I have just laid before you, there occurs a diminution in the rigidity of the uterus itself—one of the most important agents, as I have endeavored to show, in preserving the uterus intact. In other words, the uterus becomes pliable to an unusual degree. This is a state of things which constitutes a very strong predisposition to a change of shape in the uterus. In what class of individuals do you observe such a condition as this? In young women who are growing very fast, in whom the vital processes ought to be exceedingly active, at the age of twelve to fifteen or eighteen; who are badly fed, surrounded by hygienic circumstances of a deteriorating character, especially so when confined in close apartments and workrooms. These are the conditions of life which predispose to this want of rigidity of the uterus. The other predisposing cause which I have to allude to is *pregnancy.* If the uterus has undergone the expansion and enlargement inseparable from pregnancy, there is thus constituted a predisposition to flexion, and the predisposition is created in the following manner:—After pregnancy is over and the uterus vacated, it is some time before the organ returns to its normal dimensions. In fact, it does not return to the dimensions which existed before. In round numbers, the increased size would be represented by one-fourth in the healthy uterus after normal gestation and its effects have properly come to an end. In the next place, it must be stated that the quickness and rapidity with which the uterus becomes reduced in size after pregnancy is very various. In some cases the uterus is exceedingly slow in returning to its normal dimensions. If the process goes on healthily, the uterus speedily returns to its proper size—probably within a month; but in other cases it is a considerable number of months before the uterus is found of its normal size. This involution of the uterus, as it is termed, goes on in this inactive manner in a very considerable number of instances; but even when it goes on to the full extent and with the usual quickness, the uterus is still left larger than it was before. And it must not be forgotten that the increased size affects not so much the cervix as the body of the uterus, which, as already stated, is, from its want of connexion, more predisposed to a change of position. These constitute the predisposing causes of flexion. There are several others, but these are the two main ones.

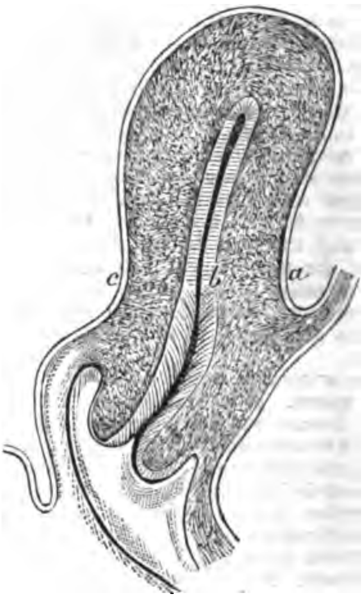
Next we come to exciting causes. The exciting cause is sometimes simply an exaggeration of the predisposing ones. For instance, an ill-nourished woman becomes pregnant. There we have the two classes of predisposing causes in operation together. Here the uterus is a long time in returning to its proper size after the labour is over; the patient very soon falls pregnant again, and perhaps a third or fourth time. Thus the uterus has very little rest: it has scarcely time to recover the effect of one pregnancy before another pregnancy follows. The result is that, in following her ordinary avocations, the uterus in such a patient gradually gives way. The upper part of it, that is to say, comes to assume a position which is partly one of flexion; and when the flexion has once arrived at that point, the patient has generally no more children. She is liable to miscarriage; and if she conceives, that conception is followed by miscarriage. Most frequently she does not conceive, and the uterus is left in this flexed condition; and if it attracts no attention, it remains so and never gets cured. The case which I have just laid before you is one which shows that constant action of predisposing causes leads to the disease by itself, the predisposing causes acting by themselves; and when we get the two together we have what amounts to an exciting cause. The other exciting causes to be mentioned are of a more positive and direct character. One important exciting cause is an accidental strain or fall, such as in the following case:—A lady standing at her door is about to step into a carriage; the servant is sent to fetch something; she stands by the horse's head; the horse suddenly takes fright and starts forward; the lady holds on by the rein, and is dragged some yards. She undergoes great exertion there and then, and the result is that the uterus is forced downwards and backwards, and retroflexion of the organ is instantly originated. I need hardly say I am now giving you an actual case. Take another instance:—A young lady, previously in a state of good health, but unused to much exertion, goes to a ball, and, not accustomed to dancing, dances for a long time together—for five or six hours. She feels rather ill; and, to add to this, the following morning, on coming down stairs, she slips and falls on her back, passing over four or five steps, and receiving a violent jerk. She experiences intense pain; and it is found on examination a few days afterwards that the uterus is acutely anteflexed. Another instance:—a lady, four days after parturition, in the absence of her nurse, rises from her bed, and walks across the room to get something. She experiences a sudden severe pain, and returns to her bed. She makes light of it at first; but a few months afterwards, after undergoing a course of continuous discomfort, it is found that she is suffering from retroflexion of the uterus, which originated doubtless as I have mentioned.

I might multiply instances *ad infinitum* in which it was demonstrable that the starting-point of the flexion was an accident. Another exciting cause of flexion of the uterus is the position of the body. It is quite certain that remaining in a constrained position daily for a great number of hours has a very important influence in the production of one of the forms of flexion of the uterus. I mean anteflexion. Young women, for instance, employed in a dressmaking establishment, and following their occupations for many hours during the day, without any opportunity of taking exercise, combined not seldom with bad living, and thus bringing into play the predisposing causes also. It is not at all rare to find, under such circumstances, that the uterus becomes anteflexed. The use of sewing machines I have observed to be followed by the same result. This is due to the position of the body. A word more in reference to pregnancy as a cause. We frequently find in practice that patients attach extreme importance as regards the commencement of their maladies to their confinements, suspecting the illness to be due to some peculiarity in the confinement itself. Undoubtedly when the perineum has been much injured that is a source of mischief, and may lead to this and other disorders; but flexion may originate without injury of the perineum.

How, it may be asked, does the fundus of the uterus descend forwards or backwards, as the case may be? It descends in consequence of the pressure from above when a sudden or forcible exertion involves putting into a state of rigidity the abdominal muscles. The viscera of the abdomen receive the pressure, and that which offers the least resistance, of course, will give way. It is a matter of accident whether the fundus moves forwards or backwards. That is determined by other conditions which I have to put before you presently.

We pass now to the study of the *varieties* of flexion of the uterus. Supported as the uterus is at each side, lateral flexion is very rare. The uterus is generally either bent forwards or backwards, constituting in one case anteflexion, in the other retroflexion. With reference to the comparative frequency of these two events, the following is, I believe, the truth on the subject. Anteflexion of the uterus is more common than retroflexion, and in the proportion of about three to two; but severe anteflexion is more rare than severe retroflexion. A consideration of the attachments of the uterus gives the clue to this. The drawing exhibited (see Fig. 2) represents the normal shape of the uterus, and the position of the bladder. It is seen that the peritoneal pouch behind the uterus (*c*) is much deeper than that in front of it (*a*). There is, in fact, less support for the uterus behind than there is in front. When the fundus gets bent backwards, it will be seen there is more room for it, and that it can descend further posteriorly than anteriorly. The pouch behind is much the deeper. In front there can hardly be said to be a pouch. That is why the flexion in retroflexion is often more acute than in cases of anteflexion. You may, perhaps, ask at this stage of the inquiry, What is the difference between flexion and version? In point of fact, version very rarely occurs without some degree of flexion, nor does flexion usually occur without some degree of version. Simple retroversion, with no actual curving of the uterine canal, I do not know that I have ever witnessed, but I have seen one or two cases of very complete simple anteversion in the cases of forward inclination of the uterus.

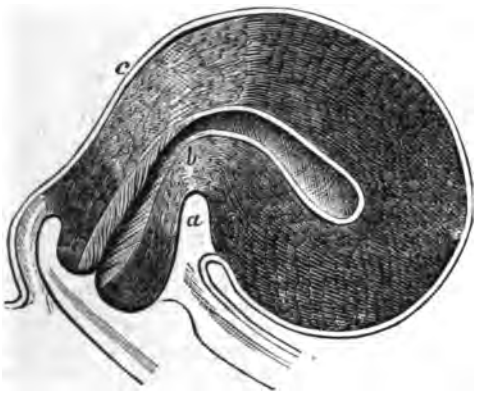
Fig. 2.



Let me, in the the next place, call your attention to some of the *pathological effects* of flexions of the uterus. The drawing on the board (see Figs.

1 and 2) represents the comparative thickness of the walls of the uterus, as shown by a section through it transversely and from before backwards. What would be the effect upon the uterus of a bending of the organ? It would obviously be to produce a compression of the tissues of the organ at the seat of the bend. Such compression is in the nature of things inevitable. The distance between the external and the internal wall will be diminished. The diminution of the thickness of the walls of the uterus will take place to a greater extent on the concave side of the bend. There will be a diminution of the diameter at the position of the flexion (*a*, *b*, *c*), and the general result will be that there is a compressing force exercised at the middle of the uterus upon the tissues of the organ (see Fig. 3). What will be the effect of this compression? The arteries of the uterus pass into it from its sides. They come from the uterine artery, one on each side passing upwards from below. There is another supply of arterial blood from above by means of the small Fallopian branch of the spermatic artery. Were it not for this small branch the effect of forcibly compressing the uterine arterial branches would be that the upper part of the uterus would get very little blood at all. It is certain that compression, such as occurs in flexion, and is exercised on the middle part of the uterus, has the effect of materially retarding the circulation in the vessels, veins as well as arteries traversing the body of the uterus. The result is, in effect, *a congestion of the body of the uterus*, and, in a less degree, also a congestion of the *cervical* part of the organ. In order to give prominence to this mechanical effect, and to signify its clinical importance, I have ventured to describe it under the title of "Strangulation of the Uterus." It is, I believe, an inevitable result that the circulation in the upper part of the uterus should be in a considerable degree interfered with when compression is thus exercised upon the uterus and its vessels, the result being that the upper part of the uterus comes in the end to contain a larger portion of blood than usual. It becomes unduly heavy and larger. It becomes not only congested, but likewise unduly sensitive, to an extraordinary degree in some cases; and the congestion and undue sensitiveness constitute the most important of the phenomena, to a less degree in anteflexion than in retroflexion, but even in the former cases to a marked degree in many instances. This compression in the middle of the uterus has various effects in different cases. After a time, if the flexion is not very acute in degree, the uterus may become habituated to it, and flexion becomes after a certain interval less embarrassing to the uterus. The uterus acquires toleration to a certain extent, and then we do not see the patient. But when it does not acquire that toleration, or when, as frequently happens, the malady increases, we have an opportunity of witnessing these effects; the fundus uteri is found sensitive, swollen, and tender to a degree; the patient is in a state of discomfort which hardly any physical condition of other organs of the body can exceed. The compression of the uterus is a phenomenon to which I attach great importance as a feature in the natural history of these cases. Various writers on this subject take views on the matter which differ considerably from those which I have just laid before you. It is held, for instance, that this congestion, or so-called inflammation, of the upper part of the uterus is not the only primary evil, but the cause of the pain in these cases of flexion; while the flexion itself is of secondary consequence. I would wish to speak with all respect of the opinions of others; but I must say that my experience has led me to take a very opposite view. The inflammation or congestion of the upper part of the uterus is generally considerable. It is only so far primary in the degree and in the manner I have already pointed out in speaking of predisposing causes of flexion. Thus a congestion of the upper part of the uterus, which we may suppose to exist to begin with, may, in the first instance, produce flexion. Having done that, the flexion will react upon the congestion, and will increase it; and, unless cured, it will prevent the cure of that congestion. I do not at all deny the importance of this element of fulness of the blood-vessels of the upper part of the uterus—very far indeed from that; but the relation which exists between the two things is, I believe, the one which I have endeavored to lay before you. This view influences the treatment in these cases very decidedly indeed; for whereas others are disposed to treat the affections, at all events in the first instance, by blood-letting and leeches—a treatment which has for its effect removal of congestion,—I submit that such cases should be treated by going more to the root of the matter—viz., by an alteration of the shape of the uterus, which is the means of keeping up the congestion. And this method of treatment I have had the greatest reason to be satisfied with in practice.

Fig. 3.



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ABSTRACT
OF THE
Lumleian Lectures
ON
DISEASES OF THE MUSCULAR WALLS OF THE HEART.

Delivered at the Royal College of Physicians, March, 1872.

By Richard Quain, M.D., F.R.S.,
Physician to the Hospital for Diseases of the Chest, Brompton.

Lecture I.

In the attention which has been given to diseases of the valves of the heart, the affections of its walls have too much been lost sight of. True, their importance has not escaped the attention of the principal writers on the subject. Laennec, Stokes, Gairdner, Chambers, Peacock, have all referred to the importance of studying the condition of the walls of the heart; but such prominence is still given to the subject of valvular disease that many persons have come to regard the term as synonymous with all heart disease, while there are not a few who regard dilatation and hypertrophy as mere complications of diseases of the valves. No doubt disease of the valves is a very serious element in the production of heart disease; it more or less inevitably leads to dilatation, or dilated hypertrophy. But many other causes are equally efficient in producing those conditions. The real value of clinical study of the valvular diseases of the heart is that a knowledge of their existence leads us to a recognition of the immediate cause of the disease, which may be present in the heart's walls, and also that we may be able to measure the progress of the one by the nature and progress of the other.

Clinical study of the diseases of the walls of the heart teaches us that (1) the really serious effects of heart disease result from hypertrophy, or from dilatation, or from a combination of the two; (2) valvular diseases may exist up to the close of a long life without rendering the subject of them conscious of their presence; (3) it is possible to refer to cases in which, valve disease having existed without causing any inconvenience, something occurs which damages the condition of the muscular walls, and serious disturbance is the result; (4) there are cases in which some additional mischief occurs to valve disease already existing, and this mischief is remedied by the supervention of further compensatory hypertrophy; (5) the converse of the last class is seen in cases of valve disease in which the heart walls fail and the disease progresses; (6) cases occur in which valvular disease—i.e., incompetency—is caused by dilatation of the heart. These propositions show the primary importance which belongs to the study of diseases of the walls of the heart. As these walls are strong and efficient, as they are weak, dilated, and inefficient, so will be the phenomena of disease with which we have to deal.

Enlargement of the heart is the most important of its pathological states, both from the frequency of its occurrence, and from the marked influence which it has over many other morbid conditions of the body. It is rarely the result of a single process, being usually due to a combination of hypertrophy of the principal elements which compose the cardiac walls, with dilatation of its cavities, the result of internal pressure.

The subject of hypertrophy of the heart seems to have been first noticed by Nicholas Massa, in 1534, who speaks of a heart "greatly enlarged." Our own Harvey, in 1601, described a heart very much enlarged occurring in a man "as the result of choler." Mayow, in 1681, describes a case in which, the left auriculo-ventricular orifice being obstructed, hypertrophy of both ventricles had occurred; and it would be difficult to give a truer or clearer description of our knowledge on the subject at the present day than that contained in his account. Vesalius, Senac, Corvisart, and Bertin, all described hypertrophy more or less accurately, but they gave it a subordinate place, regarding dilatation as the more important disease. Bertin first described hypertrophy fully, separating it from dilatation, and he formed a classification which is almost that which we use at the present day.

Enlargement of the walls of the heart may depend, first, on an increase in the muscular fibres; secondly, on an increase in the connective tissue; and, thirdly, on an increase of fat.

The first form, *simple muscular hypertrophy*, is familiar enough in its character and appearances. The determination of the exact nature of the muscular change, whether by increased size or increased number of the fibres, or by both, is attended with so many difficulties that the question is still unsettled.

The second form, *connective tissue hypertrophy*, is one which has not yet received distinct and definite recognition. The disease first came under Dr. Quain's notice nearly twenty years ago, in a case recorded in the Pathological Transactions, vol. i. A similar change has been briefly alluded to by Corvisart, Laennec, Dr. Williams, Rokitansky, Sir W. Jenner, and Dr. Ormerod. Under the microscope a heart affected with this disease presents not only the usually limited amount of intermuscular fibrillar tissue and connective tissue cells, but a decided hyperplasia of this in the shape of connective tissue in all stages of development, from the globular to the spindle-shaped cell, and from this again to the bundle of fibrillæ. The muscular fibres are seen to be surrounded by this connective tissue, and more or less compressed by it. In many of the fibres granular and slightly fatty degeneration may be observed. In other parts of the heart the muscular fibres may be normal in appearance. The thickness of the walls is increased, as in simple hypertrophy, but their density and consistence is strikingly increased to a firm, tough, leathery character. When divided the cut edges do not collapse, but remain stiffly prominent. In color, such a heart may vary from pale buff to deep purple, according to the amount of connective tissue and of blood present. But the change often occurs with less marked characters, and the hearts thus affected may not differ greatly from those hypertrophied by simple addition to the muscular fibres. This is doubtless the reason why the disease has been so often overlooked. The origin of this change is unquestionably to be sought for in a chronic interstitial inflammation or hyperplasia.

The third form *hypertrophy of fat*, has usually been confounded with fatty degeneration. Where this change is moderate in amount, simple striæ of yellow tissue may be observed to lie amongst the fibres, and small masses of fat may appear beneath the endocardium. In the more extreme cases the muscular fibres may be almost concealed, and the columnæ carneæ may seem to arise from a mass of fat, as they are described to do by Laennec and Bizot. Under the microscope the fat is contained in cells as in other parts of the body. It is most abundant towards the outer surface. There, few muscular fibres can be seen, and the wide intervals between them are occupied by the fat-cells. Proceeding inwards, the fibres are more evident and the fat-cells fewer; and, finally, beneath the endocardium we have only a few cells lying here and there between the fibres. Even though overwhelmed with fat the fibres may still retain their organisation; but, in all cases, their course and direction is more or less modified and distorted.

Dilatation of the heart. Its character and ordinary mode of origin are well known. It occurs in especial relation to degenerative processes in the cardiac wall, and particularly when occurring in the recently developed muscular fibre of commencing hypertrophy. Both processes continuing, we get those enormous examples of dilated hypertrophy sometimes seen, in some of which the heart reached the weight of 46 oz.

The *causes* of enlargement of the heart may be classified thus:—(1) Agencies acting through the nervous system; (2) agencies acting mechanically; (3) agencies originating in disordered conditions of its nutritive functions.

Concerning the first of these causes Corvisart called attention to the increased frequency with which organic affections of the heart were met with during the French Revolution. In the overstrained excitement of our own more peaceful age we may recognise a cause equally efficient. Statistics furnished by Dr. Farr clearly show two things; first, as an absolute fact, that during the last twenty years the total of deaths of males at all ages from heart disease have been more than doubled. Corrected for increase of population, the tables show that per 1000 living between 1851 and 1855, ·725 died from this cause, as compared with 1·085 from 1866 to 1870. Secondly, that during the years 1851–1870 the percentage of deaths from heart disease has remained quite unchanged in males under twenty, showing that the difference cannot be due to improved diagnosis of the disease, whereas the percentage in each succeeding period of twenty-five years has risen half as much again. Nevertheless, the same statistics show there is almost no rise in the percentage of deaths of females from cardiac disease during the twenty-five years of life from twenty to forty-five.

The mechanical causes of hypertrophy are such as interfere with the free circulation of the blood. Severe muscular exertion, often repeated, is a frequent cause. The influence on the heart is, for the time, very distressing, and it may lead to permanent injury. Athletic sports in excess often do harm in the same way. The cardiac hypertrophy in Master Magrath was more likely the result than the cause of his sustained muscular efforts. No doubt to the influence of physical labor is due the far greater liability of men than women to cardiac hypertrophy. According to Dr. Vanderbyl, out of 40 cases 34 were in males, with an average weight 22¼ oz., whilst only 6 were in females, with an average weight of 17¼ oz. A more frequently recognised class of mechanical causes consists of those occurring in the direct course of the circulation, in the heart itself, or in the vessels—e.g., contraction of the orifices of the heart, atheroma in the great vessels, constriction of the aorta, the distal obstruction in Bright's disease, and pregnancy. The influence of obstruction in the minute vessels in causing hypertrophy was pointed out by Mr. James, of Exeter, so far back as 1817, and afterwards more fully enunciated by Dr. Bright in his paper on albuminuria, published in 1836. Both he, and subsequently Dr. Snow, referred the increased action to a difficulty in sending the altered blood through the minute parts of the circulation; and Dr. Johnson has shown how this difficulty is produced by the increased contraction of the muscular walls of the small arteries, and subsequent hypertrophy. Traube imagined that the obstruction in the kidneys would explain the phenomenon.

The influence of pregnancy in causing hypertrophy has attracted less attention than it deserves. In 1826 Lacher announced, as the result of an examination of 130 healthy women who died in childbirth, that the heart was enlarged in pregnancy, the hypertrophy being confined to the left ventricle, whose walls were increased from a quarter to a third. The occurrence of the hypertrophy has since been amply confirmed. It is due, no doubt, to the necessity for an increased supply of blood to the womb, as well as to the mechanical resistance offered by the pressure of the gravid uterus upon the great vessels of the abdomen. Some considered that the albuminuria which occurs in pregnancy contributes in some measure to the cardiac enlargement, while most think with Traube that the latter exercises a causative influence upon the albuminuria.

Enlargement of the heart from the third group of causes—disordered conditions of the nutritive functions—is more frequently of a passive and degenerative kind. In chlorosis and anæmia the heart labors rapidly but ineffectually to send its diluted contents for the nourishment, imperfect though it be, of the system. In such cases we see dilatation of the degenerated walls. In those again who live well and take little exercise, the heart becomes large, flabby, and infiltrated with fat. In Bright's disease some of the hypertrophy may be due to excited action caused immediately by the blood-state.

Of nutritive influences acting locally, one of the most interesting is the influence of pericarditis and adhesion. There has been much discussion concerning the state of the heart under these circumstances, some asserting that it is enlarged, others that it is diminished in size. As a general rule it is enlarged, but the enlargement is usually due to hypertrophy of the connective tissue. Such was the case in a heart described by Dr. Wilks in the "Pathological Transactions," vol. viii., in which one-half of the true muscular substance was replaced by fibrous tissue. Where, in these cases, the size is diminished, it is probably due to infiltration by a denser fibroid tissue—a cirrhotic process analogous to cirrhosis elsewhere.

Passing over the well-known causes of dilatation, there occur, lastly, cases of cardiac enlargement which cannot be referred to any one cause. Cases have been recorded by several observers. In some the dimensions of the heart have been enormous. It seems probable that such enlargement, without evident cause, may be due to fibroid infiltration. This view has been confirmed by an examination which has been made of one of these large hearts, weighing 40¼ oz., which has been for the last thirty years in the museum of St. George's Hospital. Under the microscope it was found to depend partly on hypertrophy of the muscular fibre, but also in great measure on a large increase in the amount of connective tissue.

LECTURE II.

The *local* effects of enlargement of the heart are sufficiently familiar. In considering its *systemic* effects it is well to bear in mind the three varieties of hypertrophy described in the previous lecture. In *simple muscular* hypertrophy, developed to overcome an obstacle, so long as the power is merely spent in overcoming the resistance, and is sufficient for that purpose, all goes well. But in some cases it is expended in other and in injurious directions. In distal obstruction, for instance, the intervening vessels are exposed to all the increased pressure from the powerfully acting ventricle, and various congestions result, controllable to some but only a limited extent by the vasomotor nerves: if the vessels be sound, transudation may occur, as in dropsy; if rotten, they may give way, as seen in many cases of cerebral hæmorrhage. Moreover, in the heart itself the increased power may be misdirected. In mitral regurgitation, the ventricular hypertrophy, unless counteracted by that in the auricle, may tend to increase the mischief in the lungs. In *connective-tissue* hypertrophy the freedom of contraction of the fibres is restrained by the surrounding new tissue, and such a heart is dynamically weak. It would seem so far as present observation goes, less likely to dilate than the simply degenerated heart. In the *fatty* hypertrophy there is a languid and feeble circulation, a sense of uneasiness and oppression at the chest, and embarrassed breathing, especially on an effort.

Dilatation of the heart, if confined to the left ventricle, causes passive congestion with all its results. When the right side is effected these are conspicuously seen in the systemic circulation. Interference with the functions of nearly all the organs of the body and general dropsy result. Usually these conditions exist in conjunction—dilatation with some degree or form of hypertrophy; and the clinical physician has to unravel as best he can the tangled web before him.

Certain special effects of enlargement of the heart deserve attention. Its connexion with cerebral hæmorrhage has been a source of much difference of opinion among pathologists; some asserting, others denying, a causative relation. Dr. Burrows has shown a very close connexion between apoplexy and disease of the heart, having ascertained their coexistence in three-fifths of the cases he investigated. Sir Thomas Watson doubts the influence of hypertrophy of the left ventricle in causing cerebral hæmorrhage; while Eulenberg believes that it has such an influence only when it is dependent on peripheral disturbance of the circulation. In 65 autopsies in death by apoplexy, the accounts of which the lecturer has collected, the heart was increased in size without disease in 31, increased in size with valve disease in 12, and in 22 it was normal in size. Of the 31 cases without valve disease, the cerebral vessels were diseased in 17, and only in 4 of the 12 cases with valve disease. In the 22 in which the heart was normal, the vessels were diseased in 14. Moreover, examining the particular chambers affected, we find that in the 31 cases without valve disease the left ventricle was hypertrophied 29 times; in the 12 cases with valve disease it was hypertrophied 10 times. Out of the 65 cases the left ventricle was hypertrophied in all 39 times. In the whole number of cases the kidneys were granular and contracted in 26, in 21 of which the left ventricle was hypertrophied. Hence we may conclude that in a given number of cases of apoplexy the enlarged heart is more frequently present than diseased cerebral vessels, nearly in the proportion of 4 to 3. Further, that in a given number of cases of apoplexy in which the heart is enlarged the cerebral vessels are found as frequently healthy as diseased. Thirdly, that apoplexy is more frequently found with hypertrophy of the heart without than with valve disease in the proportion of 5 to 3. Lastly, that when the heart is enlarged, and the vessels of the brain diseased, cerebral apoplexy is found more frequently without than with valve disease in the proportion of 3 to 2.

The relation of heart disease to phthisis.—First, as regards the size of the heart. In examining the records of 171 cases, it was enlarged in 25·66 per cent. of the male cases, in only 7·0 per cent. of the female. In the males it was small in 53 per cent.; in females it was small in 67 per cent. In males it was normal in 21 per cent.; in females in 26 per cent. Some very small hearts were met with; in a girl, aged eight, the heart weighed only 2 oz.; in another, aged fourteen, only 1 oz. 14 drs. A second point was the duration of life in cases where the heart was hypertrophied. In 215 cases of phthisis used as a standard of comparison, in which no heart disease existed, 41·4 per cent. died under one year, 26 per cent. under two years, 19·5 per cent. under four years; 13 per cent. lived over four years. In 77 cases in which the heart was hypertrophied and dilated, 26 per cent. died under one year, 50·6 per cent. under two years, 6·5 under four years; 16·8 lived over four years. Thus we see that hypertrophy of the heart tends to prolong life in phthisis, and the result is most marked in the first and third years. A third point was the relation of heart disease to hæmoptysis. In 80 cases in which the heart was hypertrophied (in 30 of which valve disease also existed), hæmoptysis occurred in 47 cases, or in 57·3 per cent. In 1381 cases of phthisis without heart disease, hæmoptysis occurred in 1870, or in 63 per cent. Thus hypertrophy appears to have no influence in promoting hæmoptysis.

The relation of heart disease to renal disease.—In 785 cases of heart disease, collected by Dr. Chambers, there was renal disease in 34 per cent. Hence a person with heart disease would be half as likely to have Bright's disease as not. Traube asserts that the renal congestion rarely ends in true Bright's disease, but the lecturer adheres to the opinion which he expressed twenty years ago—that the organic alterations very commonly result, both tubular and intertubular.

Diagnosis of the nature of the cardiac enlargement.—The characters of the simple muscular hypertrophy are well known. Those of the second variety, the connective-tissue hypertrophy, are a strong heaving impulse, with a dull and obscure first sound. The signs are those of increased strength, but the increased strength is spent in great measure in overcoming the restricting action of the fibrous tissue which surrounds the muscular fibres. Hence there is evidence of circulatory weakness in the system generally. Such symptoms were present in the case of Dr. Hyde Salter's, before alluded to. The fatty hypertrophy may be distinguished from that with which it is most likely to be confounded, as Dr. Shapter has pointed out, by the sharpness of the sounds, and by the general state of the patient.

Dilatation, the other great factor in producing cardiac enlargement, is evidenced by a peculiar square shape of the cardiac dulness, and short, clear, and sharp cardiac sounds, accompanied, when the auriculo-ventricular openings have become dilated, with a systolic murmur, in the absence of any valve disease. By the presence, therefore, of concomitant dilatation, healthy muscular hypertrophy becomes considerably modified. But it is especially with degenerated hypertrophy that dilatation is combined. Hence the signs of the two conditions, dilatation and weakness have come to be confounded, and the local phenomena of each, as well as their general effects, really due to the presence of an increased amount of connective tissue, or of fat, or of degeneration in the walls of the heart, have been attributed to the existence of dilatation alone. Hence the paramount importance of determining the textural state of the cardiac walls. In doing this the sphygmograph and cardiograph both render assistance; the former, as Dr. Sanderson has shown, by indicating the arterial tension and the efficiency of the heart to do its work, while the latter indicates with great accuracy the duration of the systolic effort in the manner described by Mr. Garrod.

The treatment of enlargement of the heart has been much ruled—as so many other things in life, and it may even be said in death, have been—by fashion. In the time of Valsalva and Albertini enlarged hearts were treated by systems so depleting and lowering that it was a question whether atrophy was not a frequent result of the treatment for hypertrophy. Now stimulants and tonics are the favorite remedies. It is under such circumstances that we hear of the "uncertainty of remedies," of want of faith in physic, or of the still more hopeless doctrine that there are few or no diseases that can be cured. The word "cure" is a relative term. If it is intended to mean that an organ or texture destroyed by disease can be restored to its former condition, the list of incurable diseases may, it is true, be made a long one. But if, on the other hand, it is meant that the progress of an organic disease can in many instances be arrested, and the patient's health so far restored that the disease ceases to inconvenience him, then the list of curable diseases is each day becoming lengthened. Are phthisis and heart disease the incurable diseases they were once? Formerly they were not recognised till they had assumed an almost certainly fatal character. Now, detected and treated in an early stage, scores of cases do well, whether we owe the result to nature or not. If our knowledge were greater, both of the nature of pathological processes and of the action of medicines, our faith in physic would be stronger, because better founded. For example, if either the lowering or tonic treatment be applied indiscriminately to each of the three forms of cardiac hypertrophy, the ambiguous results need not surprise us. We may anticipate, as we understand them better, and learn the action of medicines upon them, that our results will be far more satisfactory.

In simple muscular hypertrophy, the physician has to discover its causes, and remove them as far as he can, and secure to the laboring organ all possible conditions of repose. We must try to subdue abnormally excited action, and, on the other hand, prevent the decay and degeneration which so often follow in the course of excessive development. In the action of drugs which have an influence in reducing the force of the heart—as aconite—lies a very promising field of investigation. In connective-tissue hypertrophy, if we could diagnose it in its early stage, remedies likely to subdue the inflammatory state in which it takes its origin might have a beneficial influence. In the third form—fatty hypertrophy—our treatment must be such as is calculated generally to prevent the formation of fat. In dilatation and dilated hypertrophy, the different conditions that are present demand corresponding measures for their relief. The dilating pressure would evidently be most immediately relieved by withdrawal of some of the blood, and this sometimes in urgent cases is very beneficial; but more frequently we attain the end by indirect measures—by purgatives and stimulation of the excretory organs, which, at the same time, diminish its volume and purify the blood. The next object must be to secure, if possible, compensatory hypertrophy by improving the nutrition generally, and especially by influencing the nutrition of the contractile substance of the heart. Two drugs especially have great power for this purpose, iron and digitalis. To prescribe the former before local congestions have been cleared away is worse than useless; but after the blood stasis has been relieved, steel is well borne, and its effects are most valuable. The influence of digitalis has been most ably studied by many investigators, and especially of late by Drs. Brunton and Fothergill. There seems to be no doubt that it is a stimulant to the nervous ganglia of the heart, and neither a sedative to those nor a paralyser of the vagus, as was once believed. Its administration is at once followed by better and more complete ventricular contraction. Dilatation is lessened, and with it all the local disturbances. The heart is enabled to supply all the organs with their due amount of blood, and there is a general restoration of healthy nutrition. The treatment, however, must be always secondary to the application of the eliminating agencies.

Clinical Lecture

ON

TRACHEOTOMY.

Delivered at King's College Hospital, Jan. 12, 1872,

By John Wood, F.R.S.,

Surgeon to the Hospital.

Gentlemen,—Since we last met, before the Christmas vacation, there have been one or two cases requiring tracheotomy in the wards of the hospital. As this operation is usually called for on an emergency, it seldom happens that the whole class have an opportunity of seeing the conditions which demand the operation, or of estimating the manner in which it can be done most expeditiously and safely. The spectators of the operation are mostly those who are employed in continuous offices in the hospital, with some whose constant and steady zeal for practical knowledge overcomes the tendency to absent themselves from the hospital except when their presence is expected on stated occasions. Yet it is not the less desirable on that account to lay before you, as a class, the reason why, and the manner how, an operation so important and so frequently called for should be performed. It is one which all of you who practise your profession will at some time or other be called upon to perform. And it must usually be performed urgently, immediately, and with a decision which can only be properly obtained by a premeditated consideration of all the possible contingencies that may arise, of all the instruments that may be required, and with a fair knowledge of the anatomy of the parts concerned.

In the first place, I must premise that there are no less than three operations for opening the windpipe in cases of obstruction to the respiration in this part of the respiratory passage, each of which possesses advantages that render it the most properly applicable in certain circumstances. These I propose briefly to lay before you in this lecture. To enable you the better to understand why they are severally more fitted we must consider the operations themselves and the site in which they are performed.

The operation of *laryngotomy*, as being placed the highest in the tube, and as the most simple, I will take first. It is performed in the crico-thyroid space by dividing the thin but tough and elastic membrane which connects the inner surface and lower border of the thyroid with the outer surface of the cricoid cartilage. This space is placed immediately below the true vocal cords, and of course below, or to the lung side, of that important strait or movable gateway through which we inspire the breath of life. Now the true vocal cords have a character in reference to the mucous membrane that covers them which has an important bearing upon the limitation of morbid affections of the larynx and trachea. The membrane which covers their highly elastic fibrous structure is very closely adherent to its substance. Above them the mucous membrane of the larynx is much more loosely connected by a submucous areolar tissue, which is apt, when inflamed, to swell up so as to overlap and overhang the chink of the glottis and obstruct respiration in this manner. It is also more liable to the ulcerative processes common in syphilis and phthisis, as well as to the spread of traumatic, erysipelatous or diphtheritic inflammation from the mouth, fauces, and pharynx. Foreign bodies passing from the mouth, if irregular or pointed in shape, or somewhat soft in structure, are liable to become impacted in the same situation; and, lastly, tumours more frequently grow from the walls of the larynx or its marginal structures at a level above the vocal cords.

The close proximity of the crico-thyroid space to the rima glottidis enables the surgeon readily to open up the interior of the larynx by dividing the thyroid cartilage vertically, should the nature of the obstruction require it, or to pass instruments through the chink of the glottis to dislodge impacted bodies. Many of these are advantages which can often not be appreciated before the operation is commenced, and in the proper choice of a position is embraced the capability of meeting an enemy in various and unexpected combinations. Perhaps an advantage which would be reckoned by many as greater than any of these is the important one that this operation is much more simple, less dangerous, and more quickly and readily performed, in cases of impending suffocation, than any other upon the windpipe; and it is one which may be performed by almost any bystander with ready nerve, decision, and a tolerably sharp penknife. The crico-thyroid space is very superficial, placed at the most forward part of the curve of the trachea, immediately below the projection of the thyroid cartilage called the "pomum Adami." Below this point the windpipe pursues a backward and downward course, receding rapidly from the surface, while the outline of the neck is sloped considerably forwards down to the sternum by the sterno-mastoid and other muscles, and by the interposition of the thyroid gland. In the median line the membrane is covered only by the skin and fascia, but on each side the crico-thyroid muscles diverge from their origin to their insertion into the thyroid cartilage, so as to leave a triangular interval, with its base upwards, uncovered by muscle. Further out are placed the sterno-hyoid, thyroid, and thyro-hyoid muscles, which need scarcely be touched in the operation. A very small branch of the superior thyroid artery crosses the space to anastomose with its fellow, giving off on each side a very small twig through the membrane, called the anterior laryngeal. You see, therefore, that there is only skin and fascia, with a few of the innermost fibres of the crico-thyroid muscle, and perhaps this very small artery, which need to be cut in the operation. The wound of this artery has been said, however, to cause death in a very young child, already more than half suffocated, by bleeding into the trachea; but any hæmorrhage from this minute source can always be stopped by the pressure of a tube passed through the opening, or by a cold sponge. When the vessel is, as sometimes happens, irregularly large, its very superficial position will indicate its presence beforehand, or enable it to be readily secured by twisting or ligature.

In the text-books you are directed to make first a vertical cut an inch long from the pomum Adami downwards through the skin and fascia, and then to cut horizontally the crico-thyroid membrane close to the upper border of the cricoid cartilage. Now, under a sense of impending suffocation, patients usually throw the head backwards, and this movement stretches the skin of the neck and tends to close this vertical incision, and thus to interfere with free inspiration through the wound. But this frequent movement tends, on the contrary, to open a transverse cut in the tissues. I therefore prefer, with a sharp, small-bladed knife (double-edged, if at hand), to make a single transverse incision across the lower part of the hollow depression felt by the finger, just above the cricoid ring, through the skin and membrane at once, right into the windpipe, and to extend it sufficiently laterally to introduce a tube. Such a wound will remain open even without a tube; sometimes, indeed, the patient will breathe easier without one. If a tube be used, it should be broader in the transverse than in the vertical diameter, and shorter in the length between the shield and the curve than the one adapted for tracheotomy. Upon the latter point I shall have more to say subsequently.

Sometimes the incision is found, in a narrow female or infantile larynx, to be too limited to permit free respiration or manipulation. In such a case I should extend it by a median vertical incision downwards through the cricoid cartilage, or, if the case demanded it, upwards through the thyroid. Experience has found that such openings in these cartilages heal well, and without producing the contractions or mechanical disarrangement of the laryngeal apparatus which were formerly dreaded.

The circumstances under which laryngotomy is advisable are those which are found in the fauces, throat, larynx, and rima glottidis; with the addition of those in which foreign bodies in the trachea and bronchi are freely movable, hard, and smooth, and which would consequently pass out either through the wound or through the glottis after the trachea is provided artificially with an alternative channel of respiration. Most cases of impaction in the rima glottidis by foreign bodies, by tumours growing from the ventricles of the larynx, epiglottis, or vocal cords, or by inflammatory swelling which is traumatic or non-spreading, would be included in this category; and it might embrace many cases also of syphilitic or phthisical ulceration of the larynx in which the disease does not extend below the true vocal cords.

Above all operations, that of *tracheotomy*, being one of haste and emergency, demands the most simplicity in the construction, use, and number of the instruments employed. Numerous contrivances for opening the trachea have been invented, some with more or less theoretical excellences; but all these may be stultified by not being at hand, not being properly used, or necessitating an embarrassing change of instruments in the operator's hand at a critical moment of the operation.

I have been accustomed ordinarily to use, and to recommend the use of, three instruments only—namely, a single-edged pointed scalpel; a pair of slender and somewhat elongated forceps, provided with a single hook at the ends, which project very obliquely towards and fit each other in the manner of the teeth of a rodent animal; and, lastly, a tracheotomy tube of appropriate size, constructed with an outer pair of spring blades held firmly together by a shield, and an inner tube which slides between them, and which can be removed for the purpose of cleanliness.

I usually prefer to stand on the left side of the patient, so as to cut upwards from, and not down towards, the sources of danger in the lower part of the neck; but it is not in all cases possible to have this choice.

The two operations of treacheotomy are performed respectively above and below the isthmus of the thyroid body. The former is by far the more readily performed, and involves the fewest dangers during and after the operation. The site of the primary incision is, in both, pretty nearly the same—namely, the same vertical incision in the median line of the neck, about two inches long, and extended a little higher or lower as the case may require. In a short neck, whether infantile or adult, the incision extends downwards nearly to the top of the sternum when the operation is performed below the thyroid. If the upper operation is at first intended, and afterwards it is seen that the lower one is preferable, the change can readily be effected by extending the incision downwards. After cutting through the skin and fasciæ, a small anastomosing tributary of the anterior jugular is often divided, but gives little trouble beyond slight pressure with the finger. Sometimes, however, this communicating branch is enlarged so as to assume the proportions and functions of the entire trunk, and passes into the vein on the opposite side of the neck, and may require ligature. The sterno-hyoid and thyroid muscles are now exposed, the latter meeting each other in the median line, which is indicated by a mere areolar interval. Through this, as nearly as possible, the knife should next pass, and the muscle be held aside with the hooked forceps. A fascia covering the isthmus of the thyroid and the communicating branches of the thyroid arteries and plexus of inferior thyroid veins is now exposed. The surgeon can at this stage open the trachea either above or below the thyroid gland. To accomplish the former is, as I have said, more easy, on account of its comparatively superficial position. The windpipe is here covered pretty nearly by the same kind of structures as it is below; but the effect of the direction and action of the sterno-mastoid muscles, as soon as the skin and fasciæ are divided in the median line of the suprasternal fossa, is to raise the edges of the wound, and to increase its depth very much at the lower part of the neck. The inferior thyroid plexus of veins is also met with below the thyroid isthmus, and in most cases requiring tracheotomy these are gorged and distended to the utmost on account of the arrest of respiration, and, when cut, bleed very profusely. Again, to the sides of the larynx are attached the strong muscles, the actions of which balance each other and hold it laterally tolerably steady. But since no muscles are attached to the trachea at the lower part of the neck, nor any fascial or other structures which would steady it laterally, it is moved about and sometimes even contorted by the plunging spasmodic respiration and heaving chest, and may be very difficult to fix, especially in young children. The nearer you get to the larynx, therefore, the steadier laterally does the trachea become, and the easier to fix and penetrate. The tracheal cylinder also diminishes somewhat in width downwards. And with the depth of the wound, the amount of the bleeding and the motion and smallness of the trachea increase the difficulties and dangers of tracheotomy.

If it is determined to open the trachea above the isthmus of the thyroid, the upper part of the fascia which invests the gland and connects it with the trachea must be seized with the forceps, and the point of the knife applied so as to cut or scratch through it horizontally. The handle end may then be placed under the isthmus and made to push it downwards, and at the same time to separate it to a sufficient distance from the trachea. The isthmus varies much in bulk, being larger in females, and, proportionately, in children, than in the adult male. It seldom covers the first ring of the trachea, being usually placed opposite the second and third. Usually it may easily be displaced downwards so as to permit division of the three upper rings. To accomplish this, seize the fascia covering the fibro-cartilage or upper cartilaginous spaces as low down as possible, and a little on one side of the median line, with the hooked forceps, the teeth of which, projecting well downwards, will bite easily into its substance. Then, guided by the interval between the double straight blades of the forceps, pass down the scalpel and puncture the windpipe vertically, afterwards extending the incision upwards as far as the cricoid cartilage. A rush of air mixed with sputum and blood usually follows the puncture; and sometimes, in the confusion and nervousness caused by this coming in his face, the young operator loses his self-possession, and lets go the trachea before a most essential part of the operation is accomplished—namely, the introduction of the silver tube. Now, for a speedy and satisfactory conclusion of the operation, it is essential that the hold upon the trachea, being once obtained, should not be let go; but, the knife in the right hand being changed for the outer part or spring sheath of the tube, this should be firmly compressed between the finger and thumb and carried down along the forceps through the tracheal wound, and then pushed in a curvilinear manner home to the shield. The holes in the sides of the shield should be first furnished with narrow tapes, conveniently rolled up, to be tied round the neck, to hold-in the instrument when the operation is completed. The inner tube should not usually be passed in until some of the spasm consequent upon the operation has passed away. The manipulation of the trachea must usually be done (after the first glimpse has been obtained) by the feel rather than by the sight. When the forceps has obtained a proper hold, it acts very efficiently in the double capacity of a holder and a guide to the knife as well as to the trachea-tube. If sufficient room cannot be obtained by pushing down the thyroid isthmus, the incision may be extended upwards through the cricoid cartilage. Although it is perhaps better to avoid this if possible, yet its performance is not usually attended with any after-inconveniences.

When it is intended to open the trachea below the thyroid isthmus, it may be necessary to carry down the external incision a little further. The fascia enclosing the inferior thyroid veins in the middle line should be carefully cut or scratched through by the combined use of the knife and forceps; the communicating veins should be rather torn than cut, the tension necessary to tear them being beneficial in forcing back the blood contained in them, and rendering them less liable to bleed for a few minutes, until the tube has been introduced and the blood kept out of the trachea. In this case the inner tube should be put in immediately to stop out the blood as much as possible. In performing this lower operation increased dangers beset the operator from the close contiguity of the left innominate vein and the innominate artery. In very young children, also, the size and high position of the thymus gland may be a source of embarrassment. It may project out of the cavity of the thorax and overlap the left innominate vein, which, usually in these cases turgid with blood and provided with thin coats, may easily be injured either with knife or forceps in pushing downwards the protruding thymus. The innominate artery here lies to the right side of, and somewhat upon or in front of, the trachea, and in many adults even may be seen and felt to pulsate inside the sterno-mastoid in the supra-sternal fossa. The difficulties in opening the trachea are increased under these circumstances by the much greater depth of the windpipe, by the bleeding from the inferior thyroid plexus of veins, and by the more mobile nature of the trachea at the point operated on. In young children these difficulties are increased by the small size and yielding nature of the walls of the windpipe itself, the wound in which can best be made, and, when made, held open for the introduction of the tube, by steady traction with the hooked forceps lifting forward one lip of the slit in the windpipe, so as to elevate it above the other. The closed valves of the outer tube can then usually be easily introduced by a slight sideling motion. A fault often committed by young operators consists in letting go the trachea through the movements of the patients or nervousness, or in changing instruments before the opening in the tube is made sufficiently large. Then the air rushing out of the incomplete wound, mixed with blood, froths and churns up out of the wound, and obscures all the landmarks of safety. An attempt is then made to enlarge the opening in the windpipe, with the effect probably of making a second and independent wound. The remedy for this is to get a firm hold of the trachea, and not to let go until the tube is fairly lodged. It is advisable in using the knife in this situation, and under these circumstances especially, always to keep the back of the knife towards the sternum, so as to avoid touching the distended innominate vein or innominate artery. In persons past the middle age the difficulties assume a different shape. They usually arise from the ossification of the tube, or some morbid change in or near the thyroid gland. In case of the patient being beyond middle age, and of the male sex especially, in whom calcification of the trachea is more common, it is advisable to be provided beforehand with a pair of strong scissors, or cutting forceps bent edgewise at the joint, with short, stout blades, of which the under one is pointed; these are provided with long handles of convenient thickness. If the scalpel fails in making impression upon the trachea, this can be substituted, the sharp blade being thrust through and turned upwards in the tube. With this it will be easy to divide an ossified trachea, or even to remove a piece, if desirable. The lower operation may be required in cases of tumour of the thyroid isthmus, of foreign bodies impacted near the bronchi, and in removing the false membranes in croup.

Among the dangers of the lower operation of tracheotomy must be reckoned, in addition to those of the operation itself, such as bleeding from the inferior thyroid plexus of veins, wounding the inferior thyroid artery by a slip of the knife on one side of the trachea, or of the innominate vein or artery, those consequences which are secondary—viz., infiltration of air into the anterior mediastinum and general subpleural tissue, or of blood or pus into these structures, and the embarrassment of breathing and other bad symptoms which may ensue thereupon. One source of danger, which has proved fatal in many instances, some of which have been recorded in the Transactions of the Pathological Society during the last few years, arises during the subsequent wearing of the tracheotomy tube under the constant friction of the chest-walls in breathing. A progressive ulceration, either downwards from the shaft, or forwards from the point of the tube rubbing against the anterior wall of the trachea, has at length opened into the innominate artery, and proved suddenly fatal by hæmorrhage into the bronchi. Another danger is the separation of the shaft of the trachea-tube from the shield at the joint which unites them, permitting the shaft to slip entirely into the trachea. Two cases of this kind have occurred in the practice of Mr. Henry Lee and of Mr. Holthouse, and were brought forward at the meeting of the Royal Medical and Chirurgical Society on Jan. 9th, 1872, necessitating another subsequent operation for the extraction of the tube. A valuable suggestion was made on this occasion by Mr. Brudenell Carter, that the shield and tube should be made in one piece of metal, without the intervention of solder, which corrodes sooner and more easily, or that the joint should be gilded over to guard it from the combined action of air and liquid secretion. It is not, however, this action alone which contributes to this unfortunate result, but also, and more powerfully I believe, a defect in the shape of the tube usually employed. For the lower operation of tracheotomy the shaft of the tube between the shield and the curved part is, for male adults especially, much too short. The consequence is, not only a difficulty in making the tube lie evenly and comfortably in the trachea and with the lower opening fairly in a line with the axis of the trachea, instead of being inclined, as it usually is, so as to look towards, and press with its sharp edge upon and rub against, the anterior wall just under the innominate artery,—but there is also a considerable traction inwards exerted by the concavity of the curve lying in the trachea, against the shield resting obliquely upon the surface of the skin. It is this traction, combined with the suction-power and constant motion exercised by the chest in respiration, which contributes most to separate the parts of the tube from each other, and to suck in the shaft of the instrument. For the upper operation of tracheotomy the tubes in common use do very well under ordinary circumstances; but for the lower operation in adults, where the trachea lies an inch and a half deep, and may lie deeper, as in cases of tumefaction from emphysema, inflammation, solid growths, or spinal and chest deformity, it is essential that the length of the tube from the shield to the culm of the curve, should be *an inch and a half*, instead of barely half or three-quarters of an inch, as we find in the tubes at present in use.

I must here impress upon you the great importance of proper care and treatment subsequent to this operation. After clearing the tube from blood and thick, viscid mucus, the air which is inspired through the tube must have communicated to it the moist and warm characters which it normally acquires in its passage through the nose, mouth, fauces, and larynx. This can be done immediately by dipping a large coarse sponge, well cleansed, in water of a temperature a little above that of the body, and tying it loosely on each side to the tape which encircles the neck. As soon as possible, also, the air of the sick chamber should be made warm and moist by a steam-kettle with a long tube placed on a good fire. In large rooms or hospital-wards it is better to erect a sort of tent of blankets over the bed, close beside the fire, and put the mouth of the steam tube under it. A great many tracheotomy cases, both in children and adults, die afterwards from bronchitis and pneumonia, which, sometimes brought on, are always aggravated by the admission of dry cold air to the lungs. The above precautions are of course even more necessary in cold and frosty weather.

In conclusion, there is one point which it may happen to be important to keep in mind; and that is, the duty of completing and finishing these operations upon the windpipe, when once begun, in the face of every possible eventuality which may arise in the course of it, even up to and beyond the apparent death of the patient. In no case is the truth of the saying, "while there's life there's hope," more paramount than in operations to relieve suffocation, to which, as in cases of drowning, the higher truth may be almost applied, that "beyond life there's hope." One of the most striking instances of resuscitation after apparent death during tracheotomy I have been lately witness to. A few weeks ago I was called in the middle of the night to the relief of a medical man, considerably beyond middle age, suffering from laryngitis combined with bronchitis. On hastening to his house, which was a considerable distance from my own, I found Dr. Goddard, of Edgware-road, in attendance, who had judged very rightly that an operation afforded the only remaining chance of relief from the very threatening symptoms presented by the poor patient, and had accordingly requested my assistance. A glance at the distressed countenance and the dusky lips and finger-nails—a hurried pressure of the failing pulse —a brief listening to the raucous laryngeal noise and heaving chest, deprived of vital air—led me, after a hasty consultation, fully to coincide with the imminent necessity for tracheotomy, with a view of at least prolonging life. While the rapid incisions were being made to expose the trachea, the patient suddenly ceased to breathe altogether, the pulse stopped, the eyes rolled over and seemed to become glazed, and after a brief spasm of the facial muscles, usually observable just before dissolution, all seemed to be stilled in death. A considerable obstacle was now met with in extensive and very hard ossification of the trachea, upon which the scalpel made little impression. With the words upon my lips, "I fear we are just too late," but remembering the maxim I have endeavored to impress upon you, I persevered notwithstanding, and by the exercise of considerable force, and by first converting the edge of the scalpel into and then using it in the fashion of a saw, I got through the hardened trachea and passed in the tube. After employing the usual methods of artificial respiration, we were in a few minutes encouraged by a gentle sigh of inspiration; and in less than a quarter of an hour the patient was sitting up in bed, breathing freely through the tube, expressing himself as comfortable, and writing upon a slate a professional criticism as to the urgent necessity of the operation and the dexterity of its performance.

Original Papers.

CONIUM IN THE TREATMENT OF ACUTE MANIA.

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It has been lately intimated that the sedative treatment of mental diseases characterised by excitement is neither necessary nor desirable. Dr. Maudsley has hinted that recovery is retarded rather than hastened when sedatives are had recourse to; and has questioned whether "the putting the nerve-cells of the patient's brain into chemical restraint, so to speak," by the use of such remedies, "does really benefit them." The arguments which have been thought to justifiy these speculations have not yet been fully set forth; and, as they must doubtless be of a weighty nature, it may seem rash, when only partially acquainted with them, to dispute the propriety of the practical suggestion which has been founded upon them. That practical suggestion is, however, so thoroughly opposed to the teachings of experience and of modern pathology, and derives so little feasibility from the insinuations by which it is accompanied, that it challenges the immediate examination of those who are engaged in the treatment of insanity, and who are anxious to be able to give some reason for the therapeutical faith that is in them.

To prove that sedatives have been highly valued in the treatment of mental excitement from the very earliest times when medical science was brought to bear upon insanity, would not, perhaps, count for much in these days in which the wisdom of ancestors is disallowed in anything less obvious than the invention of the wheelbarrow, were it not that, in the present instance, the wisdom of our ancestors is in happy accord with that of ourselves. Sedatives are still all but universally employed in lunatic asylum practice, and have not as yet forfeited in any degree the confidence of those who are most familiar with them. And that confidence is by no means so blindly empirical as is supposed by Dr. Maudsley, who is certainly in error in asserting that "no one has ever yet tried the experiment of treating one case of acute insanity without giving any sedative whatever, and of treating another case as nearly like it as possible with sedatives, and of observing the result." I have no hesitation in saying that that experiment has been repeatedly instituted, and has led to a conviction, in those who have tried it, in all respects favorable to sedatives. In two cases in which I myself applied that very test, I found that a rapid recovery took place in that in which a sedative was used, and that protracted excitement and a tardy convalescence had to be encountered when everything was trusted to the course of nature. It does certainly excite surprise to notice a case of puerperal mania, in which recovery occurred in *four months*, referred to by Dr. Maudsley as an instance of the success of a non-sedative treatment.

That sedatives have the power of subduing mental excitement is not denied, so that the question is simply whether it is judicious to employ that power. Now mental excitement is a sure indication of disordered activity in the delicate nerve-cells and tissues of the cerebrum; and not only so, but it is a rough gauge of the severity of those encephalic changes of which it is the outward expression. When the excitement is mild in character, interrupted in progress, or of brief duration, we infer that it coincides with mere functional derangement. When it is intense in type, of firm persistence, and of long continuance, we conclude that it betokens some organic alteration. Whether, however, it is intense or mild, of organic or functional origin, we know as a certainty that mental excitement corresponds with morbid processes or changes which have an enfeebling influence upon the structures affected by them, and which may eventuate in permanent weakness or decay of those structures, or in systemic death. We know further that the evil consequences of that destructive metamorphosis upon which excitement depends are serious and far-reaching just in proportion as that metamorphosis was extensive and protracted. Where the waste is rapid and the excitement fierce, exhaustion and death are apt to ensue; where they are protracted and uncontrolled, dementia or relapses may be anticipated. Only where they are moderate and short-lived can complete recovery be looked for. The testimony of those who have had large opportunities of observing insanity will establish that in acute mania, life and the restoration of reason are jeopardised in proportion to the intensity and duration of the mental excitement, which constitutes its chief symptom, and of the histolitic processes the presence of which that excitement betrays. My own investigations have satisfied me that there is a constant relation in general paralysis between the amount of maniacal excitement by which its progress is marked and the period at which death supervenes, as well as the extent of the pathological changes which are essential to the disease. In recurrent mania it is acknowledged that every attack of mental excitement leaves an increased weakness of the brain behind it, and that thus it is that the intervals between the attacks are gradually shortened.

The above considerations, without any multiplication of examples, make it evident that the cerebral conditions upon which mental excitement depends are most pernicious in their immediate and remote effects. It would seem to follow, as a necessary corollary, that it is of the first importance, as far as possible, to reduce the severity and curtail the duration of these cerebral conditions, so that their effects may be mitigated. This corollary, however, does not secure assent. Dr. Maudsley asks if it is a proper thing to stifle excitement, which suggests the inquiry if it is a proper thing to stifle fire. Both, when they have overleapt their ordinary boundaries, hurry to destruction. The nerve-cells of the brain have no innate tendency to energise in a beneficent and healthy manner. In education they have to be stimulated in some directions and repressed in others. In disease they do not develop any new principle of spontaneous rectification. Insanity is not a self-limited disorder, running a determinate and inevitable course. On the contrary, it may be arrested or indefinitely prolonged. And even if it were self-limited, that would be no reason for abandoning all attempts to modify its progress. Pyrexia may be fitly compared with mental excitement, which is, as it were, the fever of the mind. In many bodily diseases the prognosis may be founded on the height of the pyrexia, as in mental diseases we have seen it may be drawn from the height of the excitement. Well, what is the modern treatment of pyrexia? Why, to reduce it by all possible means. Those who have made acquaintance with the writings of Jürgensen, Liebermeister, and Ziemssen in Germany, and of Dr. Wilson Fox in our country, will allow that certain fevers have had a new hopefulness conferred upon them by the practice of applying a most powerful sedative—viz., cold water—to the burning skin. All observers agree as to the inestimable value of this method of treatment in diminishing mortality, and in altogether averting certain symptoms of a distressing kind. Well, sedatives are to the excited mind what cold water is to the fevered body. They cool the heated faculties; they soothe and comfort the agitated brain; they chemically restrain the exuberant nerve-cells. Chicago would not have been desolated had adequate restraint been exercised on the conflagration at its outset; and many an inflamed mind would be saved alive could the excitement which consumes it be got under at once. The sedative treatment of excitement is not contra-indicated by any special pathological condition. Whether that excitement originates in excessive functional activity of the vesicular neurine, in defective nutrition of that tissue, in peripheral irritation, or in alterations in the quantity or quality of the blood-supply, well-chosen sedatives may always with other remedies be suitably employed. Dr. Maudsley, of course, repudiates all this. "Let a person," he says, "be made artificially lunatic by belladonna. Who would, if called to treat him, prescribe a dose of chloral or a dose of opium?" Dr. Maudsley must have forgotten the researches of Anderson and Bell, and the fact that opium has actually been successfully used as an antidote to belladonna, or he would not have asked such a question. When mental excitement is indubitably due to blood-poisoning, we should no more on that account refrain from attempting its subdual by sedatives than we should on the same ground hesitate to staunch a hæmorrhage or to relieve pain. The *materies morbi* is not eliminated in incoherence, delusions, and muscular agitations, but is probably fed and reproduced by the disintegration of tissue which these promote.

The objection that sedatives cut short excitement by inducing dementia, and not only restrain but disable the nerve-cells, will be refuted by a reference to experience. The facts that, notwithstanding the calumnies which have been uttered against them, they are still held in high esteem by sagacious practitioners, and that an addition to their number is hailed as a boon to humanity, ought to indicate that the tranquility which they create is not so deadly and deceptive as has been represented. In this paper will be found recorded a number of cases in which, under the use of an arch sedative, unmistakable improvement, not only in weight, appetite, and temperature, but in mental condition, took place, in which primary excitement was calmed and secondary dementia curtailed. Another opportunity will be taken of combating some statements derogatory to opium which have been made, and of reporting results differing from those which Dr. Clouston has obtained in his experiments with that drug. The analogy drawn between the prejudicial effects of mechanical restraint applied to the limbs and the action of sedatives can hardly be intended as a serious argument. To say that because strait-waistcoats did harm sedatives can do no good would be as if to contend that shutting off the steam must be futile because holding on by the fly-wheel had been found ineffectual.

From what has been now said it seems to result that mental excitement is assuredly the special indication for the use of sedative remedies. To this, however, it must be added that there are several varieties of mental excitement and several varieties of sedatives, and that it is in the nice adaptation of the latter to the former that successful treatment consists. If a particular sedative has been tried in a particular case of mental excitement and found inefficacious, that is a reason, not for casting discredit upon the whole class to which it belongs, but for doubting the wisdom of its selection. If a particular sedative of much potency has been resorted to in all cases of excitement without distinction, and has signally failed to do good in a majority of them, then the proper conclusion would seem to be, not that sedatives are useless, but that their indiscriminate application is foolish.

In a number of investigations having for their object the discovery of some principles by which the choice of sedatives in cases of excitement might be guided, I have been led to the conclusion that conium—a very old remedy with a good modern reputation—is the sedative, alone or in combination, which above all others is best suited for the treatment of that variety of excitement which we call acute mania. A remark of Dr. Harley's, in his admirable work "On the Old Vegetable Neurotics," first suggested to me that mania might be favorably influenced by that drug. As, however, my earliest experiments with it were, from causes which shall be hereafter explained, almost barren of results, I yielded to the conviction that Dr. Harley was mistaken, and that conium could not exert any beneficial influence upon the higher ideational nervous centres. Remembering that the mind of Socrates remained unclouded to the last—he actually recollected his debt to the doctor when *in articulo mortis*,—and that Dr. Harley's observation pointed only to the influence of conium on the motor centres, I conceived that the cerebral hemispheres were beyond its reach; that it might act very well in chorea, paralysis agitans, and convulsions, but was impotent in mental aberration. Circumstances, however, having led me to review that conclusion, and to investigate the properties of conium with a more thorough and searching procedure than that formerly employed, I soon came to perceive that it is most beneficial in mania, and that it is so chiefly by virtue of that action on the motor centres which Dr. Harley, Dr. Bennett, and others have demonstrated. It will be at once recognised that motor excitement is an important and constant element in mania. Exuberant locomotor and vocal spontaneity are often amongst its earliest harbingers. The novice in that disease hurries from place to place, trembles or chatters incessantly long before the movements of the limbs or tongue can be connected with any morbid sensation or emotion. A vague impulse to activity with an overflow of vigor, dependent upon irritation of the motor centres, is all that can be recognised at this stage. But the rapid movements in which this impulse and overflow are displayed tend, by their nature and physiological concomitants, to excite the cerebral hemispheres and involve the mental functions. The mild delirium described by Coventry Patmore—the most domesticated although certainly not the tamest of poets—as the result of the nineteenth century waltz, is an example of the facility with which quick movements may pass into quickened thoughts and feelings—a process of which innumerable illustrations are contained in the history of the ecstatic worships of antiquity, of tarantulism, and of the convulsionnaires in more recent times. Thus the restlessness and loquacity of incipient mania favor emotional agitation, which again stimulates to muscular exercise, and so on through various stages of mutual exasperation until furious madness is reached. After this climax is attained, and indeed throughout the whole progress of acute mania, the muscular element plays an important part. If we turn to the best descriptions of that disease, we shall remark that they all dwell on the extraordinary mobility which characterises it; that they enumerate commotions of the muscular apparatus, such as tremors or jactitations of the limbs, tossings in bed, wild gestures, distortions of the countenance, violent movements, cries, and vociferations, as among its most invariable symptoms. Some of them, indeed, point to even more special affections of the motor organs. Prichard says:—"Notwithstanding this constant exertion of mind and body, the muscular strength of the patient seems daily to increase; he is able to break the strongest bonds, and even chains; his limbs seem to acquire a remarkable nimbleness and pliability, and a singular aptitude of performing movements and actions which appear almost supernatural." Griesinger, while denying the development of any extraordinary strength in mania, adds that "it is correct and very remarkable that the patients can continue, often for a very long time, to make use of their muscular power, and to an extent which far surpasses their capability during health." In several cases in which death has ensued from exhaustion after, or rather during, mania, I have observed that the corpora striata have participated in that appearance of vinous staining, or purple blotchy discoloration, which is occasionally observed in the cortical layers of the hemispheres and adjoining medullary substance under such circumstances.

Seeing, then, that the motor factor forms so large a proportion of the integer of mania, it is obvious that if it can be subtracted an important step is made towards the complete resolution of the disorder. If muscular tranquility can be ensured in mania; there is good ground for hoping that mental calmness will speedily follow. If the limbs and tongue can be laid at rest, a predisposition to sleep is created. In health we all know that closed eyelids, the recumbent posture, general stillness, and a sense of weariness are most conducive to the approach of the "sweet restorer"; and in disease the same holds good to a great extent. In mania, if bodily quiescence is once secured, the chances are that sleep and improvement are not far distant. Now conium gives us the power of ensuring this bodily quiescence that is so eminently desirable. It soothes and refreshes the irritated or exhausted centres of motor activity. It exerts a powerful control over the corpora striata and whole motor tract to the periphery of the centrifugal nerves, allays restlessness and agitation of the muscular system, and thus clears the way for sleep, which is only waiting an opportunity to assert itself. Dr. Harley's investigations establish beyond cavil that the earliest physiological effects of conium are—sensations of weight and weariness in the legs, dimness of vision from impairment of power in the muscles of the eyeball, giddiness, general languor and drooping of the eyelids; and that its later effects, when given in full doses, include strabismus, double vision, dilatation of pupils, prostration of strength, and complete muscular repose merging into paralysis, without any failure of memory, confusion of mind, or emotional disturbance. They also prove satisfactorily that, in convulsions and spasmodic diseases, in chorea, paralysis agitans, cramps, and indeed in all morbid conditions in which there is much excitement of the motor centres, much benefit may be derived from a judicious use of conium. Now these physiological actions of conium, and these therapeutical uses, alike indicate that it is upon the motor tract that its influence is first exerted, and that it is from this tract that it is propagated in certain directions. The rapidity with which the effects of conium are diffused—one might almost say flashed—throughout the system,—together with the fact that they are often accompanied by a deep-seated aching pain in the forehead, suggests that of the motor centres the corpora striata are those principally affected. But, wherever the affection of the motor centres may be inaugurated, there can be no question that, from first to last, that affection is of a tranquillising character. Conium soothes and mollifies the motor centres especially when they are irritated or excited, and does not, as has been alleged, disastrously depress muscular activity. The recovery from its effects is as the waking from invigorating slumber. It is as if a tonic had braced the active powers. No weariness, weakness, nor oppression remains. And hence its great value in mania. It produces, if we may so say, a salubrious paralysis, which slackens mental speed, invokes restorative sleep, and leaves behind it an enduring impression upon any tendencies which may exist to motorial extravagance, without entailing exhaustion or debility.

The virtues ascribed to conium in the treatment of mania have been abundantly shown forth in the trials of it which I have made in the West Riding Asylum. As I before announced, it was employed here in the first instances without affording any encouraging results. That was in 1869; and, as I now know, the experiments then made with it were absolutely nugatory because the preparations used were probably inert and the doses administered certainly insufficient. In the beginning of last year, however, experiments with it were again commenced, and the results of these only require to be stated to secure the recognition of their importance. Since the 23rd of February, when these experiments were begun, up to November 1st (beyond which I have thought that it would not be fair, in the meantime, to carry my examination of the observation made), twenty-eight cases of mania in all—twenty-five of acute mania and three of acute delirious mania—have been treated from the first with succus conii, the only preparation which can be employed with confidence. Of the twenty-five cases of acute mania, twenty-three have, up to the present date (Jan. 1st, 1872), been discharged recovered: one (a woman, Elizabeth N——, admitted July 31st) has suffered a relapse; and one (a man, Thos. R——, admitted July 17th —second attack) is still maniacal, but exhibits decided signs of improvement. Of the three cases of acute delirious mania, one has been discharged recovered; one (Hannah E——, admitted July 18th) is still under treatment, but is improving steadily; and one (Mary Emma A——, admitted on April 24th) terminated in death. In the case, however, in which there was a fatal issue, it ought to be mentioned that at the time of admission there were large bed-sores over the sacrum, ulceration of the lips and mouth, a crop of boils over the body, and a state of profound exhaustion from refusal of food. These figures must, I think, be regarded as eminently satisfactory, and as affording strong ground for believing that the dangers which ordinarily beset the treatment of mania—namely, death from exhaustion, consecutive dementia, indefinite protraction, and relapses—are in great measure obviated by the conium treatment. To give these figures just weight, it is neccessary to add that the cases of acute mania in which the conium treatment was tried were not specially selected, but that all instances of that disease admitted after the date mentioned were without exception submitted to it.

With the view of determining the relative duration of cases of acute mania treated with conium and with other remedies, I have contrasted twelve cases from each category. As the result of that contrast, I find that twelve consecutive cases treated from the first with conium, admitted subsequent to Feb. 25th, 1871, and since discharged recovered, had an average duration, as measured by residence in the asylum, of 102 days: whereas twelve consecutive cases treated with other remedies, such as bromide of potassium, canabis indica, chloral, and digitalis, admitted subsequent to October 1st, 1871, and since discharged recovered, had an average duration, as measured by residence in the asylum, of 150 days. This shows a balance of 48 days in favor of conium. It will be seen, from the cases reported below, that when conium was used, that period of convalescence which it is thought prudent to interpose between the date when recovery might be called complete and the date of discharge from the asylum was in no instance unusually shortened. In some cases it might be thought that it was unnecessarily prolonged. So rapid and decisive were the beneficial effects of the remedy that it was feared at first they might prove transitory, and that, as is not seldom the case when recovery is sudden, a relapse might be looked for. Experience having now taught that this fear is groundless, a considerable curtailment of the term of medical supervision may be henceforth safely conceded. Indeed, so prompt is recovery, as a rule, under the conium treatment, that it appears that the necessity of removal to an asylum may be obviated in some cases of acute mania if it is had recourse to, and judiciously conducted, in their initial stage.

The curative effects of conium in acute mania are not limited to those cases in which that remedy is used from the outset of the disorder. Eight patients in this asylum in whom other methods of treatment had been first employed have derived marked advantage from the substitution of conium for other drugs.

To indicate the value of conium in acute mania more conclusively than can be done by general statements or numerical results, I have drawn up epitomes of some of the cases in which its therapeutical worth has been clearly exemplified in this asylum. Before adding these epitomes, I have only to express my conviction that the conium treatment of acute mania will speedily recommend itself, to those who use it rightly, as the most efficacious mode of dealing with that form of mental disease. In order, however to secure its benefits, two conditions must be observed: firstly, the preparation used must be good and active; secondly, the doses administered must be adequate in amount. The succus conii is certainly the most trustworthy preparation of the drug, and is that which has of late been used exclusively here. Even the succus, however, varies in activity in an extraordinary degree. Some of it is absolutely inert. That which has been found most reliable by me has been obtained from two of the London houses named by Dr. Harley, from an Edinburgh and from a German firm. As to the doses required, I can strongly corroborate Dr. Harley's assertion, that they must be sufficient to produce the physiological action of the drug in order to prove beneficial in disease. I can also support his conclusion, that the effect of conium is inversely as the motor activity of the individual to whom it is given. This being so, it must be evident that in acute mania, in which motor activity is at the maximum, very large doses will be essential. I have given a woman laboring under acute mania as much as two ounces of succus conii at one dose, and have repeated this every four hours for two days. This was, however, an extreme case, and the patient had been gradually habituated to the use of the drug. As a rule I have commenced with two drachms of the succus for a woman, and three drachms for a man; and have rapidly increased the dose until I have noticed some cessation of restlessness, or signs of lassitude or weakness of the limbs. It is rarely that a dose of one ounce or ten drachms requires to be exceeded; and sometimes improvement begins with the very first administration, in which case no increase of quantity is necessary.

CASE 1.—Lydia F——, aged thirty-five, housewife, from Barnsley, admitted March 31st, 1871, having been insane for one month. Is a member of a family of drunkards; her sister hanged herself. When admitted she was in a state of violent maniacal excitement and great exhaustion, groaning, shouting, and struggling; pupils dilated and unequal; face flushed; refuses food, and is sleepless. Ordered succus conii, two drachms three times a day.—April 2nd: Much quieter; takes her food and sleeps well.—4th: Perfectly calm, and employs herself in sewing; pupils contracted and sensitive.—May 16th: Discontinue medicine.—June 20th: Is passing through a protracted stage of slight dementia characterised by loss of memory and energy.—Aug. 22nd: Discharged recovered.—Weight: March, 100 lb.; April, 115 lb.; May, 124 lb.; June, 139 lb.; July, 138 lb.; August, 141 lb.

CASE 2.—Sarah S——, aged thirty-seven, married, housewife, from Doncaster, admitted April 24th, 1871. Has been intemperate; became insane a week ago. Is in a state of extreme maniacal excitement; talkative, restless, and violent; pulse 120; pupils of average size, but sluggish; tongue and lips dry and covered with sordes; muscles hard and rigid. Ordered two drachms of the succus conii three times a day.—May 8th: Quiet and rational; pupils widely dilated; pulse 80.—11th: Convalescent. Discontinue conium.—July 7th: Discharged recovered.—Weight: April, 94 lb.; May, 94 lb.; June, 109 lb.; July, 112 lb.

CASE 3.—Lavinia B——, aged thirty-five, widow, domestic servant, from Huddersfield, admitted April 25th, 1871. Had a previous attack nine years ago. No cause known for present attack, which commenced a week ago. Is highly excited, shrieking, whistling, and fighting; sleepless and requires to be fed. Ordered half a drachm of tincture of cannabis indica and a drachm of bromide of potassium thrice daily.—April 29th: No better; is very noisy, and has not slept since admission; kicks and bites all who approach her; face and ears much flushed; pupils contracted; eyes injected; lips and tongue dry; pulse 126. To omit previous mixture, and take two drachms of succus conii every four hours.—30th: Quieter; face less flushed; pulse 92.—May 1st: Has slept well all night, but is again greatly excited; takes her food well. Ordered four drachms of the succus.—2nd: Has again slept well, and is now much quieter. To take the conium only twice a day.—4th: Quiet, but confused in her ideas. Complains of pain, weakness, and numbness in her limbs.—6th: Convalescent.—July 5th: Discharged recovered.—Weight: May, 126 lb.; June, 128 lb.

CASE 4.—George H——, aged twenty-four, married, laborer, from Hunslet, admitted June 3rd, 1871. Was never before insane. Was attacked three days ago, and has since been violently excited. Is fiercely maniacal; very restless and combative; assaults those around him; shouts out oaths and blasphemies; fancies that he is to be buried alive and is fighting with the devil; strips himself naked and destroys his clothes. Is a stout robust man, and enormously strong, florid complexion, brown hair, which stands out from the head as if bristly; muscles start and tremble as if from eagerness; conjunctivæ injected; mouth dry; pulse 108.—June 4th: Has passed a restless night and is most unmanageable. To take three drachms of succus conii every four hours.—5th: Still maniacal. Five drachms of conium every four hours.—6th: Much quieter, though still unable to converse; restless, and given to strip himself.—8th: Quiet and rational. —14th: Convalescent; complains of a strange feeling of lightness in the back of his head, and of general weakness. To omit the medicine.—August 24th: Discharged recovered. — Weight: June, 167 lb.; July, 165.

CASE 5.—Elizabeth R——, aged forty-seven, house-wife, from Wakefield, admitted June 3rd, 1871. No previous attack. Has been insane three weeks. Was first depressed for a fortnight, then excited. A maternal aunt died in this asylum. Is violently excited, raving incoherently. Has not slept for some time, and has refused food. Face flushed; tongue red and raw; fed with the stomach-pump; two drachms of succus conii being administered by that means also.—June 4th: Has had several hours' sleep, and is more tranquil; answers intelligently, but cannot keep her attention fixed; takes her food. To have three drachms of succus conii thrice daily.—5th: Is more excited, very garrulous and mischievous; bowels confined.—6th: Has slept well during the night, but is still excited and restless. To take four drachms of the succus conii every four hours.—7th: Much more composed; talks rationally, but voluminously.—23rd: Convalescent.—July 23rd: Discharged recovered.—Weight: June, 120 lb.; July, 130 lb.

CASE 6.—Joseph H——, aged thirty-five, married, blacksmith, from North Brierley, admitted March 7th, 1871. His father died of paralysis; a paternal uncle and aunt were insane. Has suffered from domestic anxieties. Became excited eight days ago, after ten days of sullenness and depression. Is very restless and talkative; cannot remain still for a second; says that he is St. Paul. Is in fair bodily health; neck bears marks of recent blister.—March 18th: Extremely violent. To take six grains of ergotine thrice daily.—23rd: The excitement has passed off, but the patient is silent and stupid. 30th: Still quiet. To omit medicine.—May 22nd: Has again become dangerously excited. To repeat ergotine.—June 11th: No better. This morning he sprang out of bed naked, and jumped through a glass door without so much as scratching himself. Ordered to leave off ergotine, and to take four drachms of succus conii thrice daily.—13th: Declined food and medicine, and remained maniacal until last evening, when he was fed by means of the stomach-pump, the succus being given at the same time. Passed a better night, and is now more tranquil; takes food and physic voluntarily. —15th: Much improved.—16th: Comparatively rational: quiet in his demeanor.—Aug. 18th: Is slowly recovering from a state of partial dementia. —October 3rd: Discharged recovered.—Weight: April, 134 lb.; May, 130 lb.; June, 133 lb.; July, 133 lb.; August, 137 lb.

CASE 7.—Maria P——, aged thirty-seven, married, housewife, from Saddleworth, was admitted on June 5th, 1871. Has had two children, the youngest of whom is now seventeen years old. No heredity; of temperate habits. Was under treatment in this asylum for an attack of acute mania from February 17th to October 9th, 1867. Grew excited eight days ago, after a short period of depression. Is now wild and ungovernable, dancing, singing, and raving. Seems to delight in movements and gesticulations, and puts forth much strength if interfered with. Ordered two drachms of succus conii every four hours.—June 6th: Half an hour after taking the medicine she becomes lethargic, then drowsy, and remains so for an hour. If roused, however, she is as noisy and exuberent as ever. Pupils of average size and equal; pulse 110; face pale.—7th: Quieter than when admitted, but when disturbed breaks out into maniacal excitement; any noise increases her vehemence.—16th: Quiet, but mischievous in a good-humored jocular way; becomes excited whenever there is the least noise in the ward.—July 2nd: Has gradually calmed down. To omit the medicine.—August 8th: Quite convalescent.—30th: Discharged recovered.—Weight: June, 106 lb.; July, 126 lb.; August, 145 lb.

CASE 8.—Fanny D——, aged thirty-five, married, housewife, from North Brierley, was admitted May 8th, 1871. Her youngest child is fifteen months old. The violent death of a brother has preyed on her mind. A week ago she became suddenly excited, and has since been uninterruptedly noisy, violent, and destructive.—9th: Very noisy; talks incessantly and incoherently, and attitudinises. There is a diastolic murmur at the base of the heart. She is greatly emaciated, but active and vigorous. Ordered three drachms of succus conii thrice a day. —12th: Now composed and rational.—23rd: Convalescent.—June 2nd: Slightly despondent.—July 25th: Again quite convalescent.—Aug. 1st: Discharged recovered.—Weight: May, 104 lb.; June, 107 lb.; July, 114 lb.; August, 122 lb.

CASE 9.—Ann S——, aged forty-five, single, housewife, from Bradford, was admitted May 12th, 1871. After slight mental depression of a year and a half's duration, she became excited ten days ago. Is now talking perpetually in a loud, harsh voice, and will not remain in one place. She apparently understands what is said to her, but replies in unintelligible jargon. Face and head much flushed; pupils contracted; pulse 90; skin cool; tongue very foul. Ordered two drachms of succus conii every fourth hour.—16th: Still very maniacal; requires to be fed with the stomach-pump. The dose of succus conii to be increased to four and then to six drachms.—18th: Now takes her food voluntarily; is much quieter, but talks incoherently.—19th: Quite rational; medicine only twice a day.—23rd: Improving. To omit the medicine.—June 20th: Convalescent.—Sept. 4th: Discharged recovered.—Weight: May, 112 lb.; June, 126 lb.; July, 130 lb.; August, 137 lb.

CASE 10.—Elizabeth H——, aged forty-three, married, housewife, from Huddersfield, admitted May 30th, 1871. Was insane for a short time three years ago. Has had six children; the youngest is now five years old. A brother and one of her own children have died of epilepsy, and one sister has had puerperal mania. The present attack commenced eight weeks ago, and has been characterised by gradually gravescent excitement.—31st: Excited and out of bed all night. Will not answer questions, and is very restless and fidgety. Fairly nourished. Skin of the face and forearms bronzed. Ordered two drachms of succus conii three times a day.—June 1st: Has been quiet during the night, and continues so this morning.—9th: Convalescent.—Sept. 12th: Would have been discharged some time ago, but has been detained with her own sanction because of an attack of hepatic disease.—Nov. 7th: Discharged recovered.—Weight: June, 124 lb.; July, 132 lb.; Aug., 131 lb.; Sept., 134 lb.; Oct., 131 lb.; Nov., 135 lb.

On a future occasion I hope to have an opportunity of recording the results obtained in the treatment of certain mental diseases by conium in combination with opium and hyoscyamus.

January, 1872.

CONTRIBUTIONS
ON THE
PHYSIOLOGY & THERAPEUTICS OF FOOD.

By F. W. Pavy, M.D., F.R.S.,

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No. VII.

ELIMINATION OF UREA AND MUSCULAR WORK.

In the *New York Medical Journal* for October, 1870, Dr. Austin Flint, jun., records the result of the examination of the urine secreted during the performance of an extraordinary amount of muscular work, under the form of walking, within a limited time. A Mr. Weston, aged thirty-two, of medium height, and weighing ordinarily 122 lb. without his clothes, celebrated as a pedestrian of the United States, undertook to perform the astonishing feat of walking one hundred miles in twenty-two consecutive hours. The feat, it appears, was accomplished within the time—namely, in twenty-one hours and thirty-nine minutes. The food consumed during the period was taken in small quantities at short intervals, and consisted of between one and two bottles of beef essence, two bottles of oatmeal gruel, and from sixteen to twenty raw eggs with water. He drank a little lemonade, and took water very frequently, but only in quantity sufficient to rinse his mouth. While walking the last ten miles, he took, it is further stated, two or three mouthfuls of champagne, amounting to about three fluid ounces; and about two and a half fluid ounces of brandy, in ten-drop doses. The head and face were sponged freely at short intervals, and the food and drink were taken mainly on the walk, which was conducted within a covered enclosure. The urine passed during and at the completion of the walk measured seventy-three and a half fluid ounces, and presented the specific gravity of 1011. According to Dr. Flint's analysis, it contained 424¾ grains of urea.

Now, 500 grains form about the average daily quantity of urea discharged under an ordinary mixed diet; and, as the diet during the performance of the pedestrian feat was rich, as the account shows it to have been, in nitrogenous matters, the quantity of urea, apart from any other consideration, was even less than might have been expected. And yet, on the strength of a comparison with another examination of the urine conducted three months later, in the absence of subjection to muscular exertion, when only 191 grains of urea were given as discharged, Dr. Flint brings forward the observation as affording evidence that muscular exercise notably increases the elimination of urea.

To take a solitary result of so exceptional a kind as this latter as a criterion or ground of comparison, and reason from it as Dr. Flint has done, is surely to violate all rules of sound induction; and it is to be hoped that we shall not find the observation quoted by writers as bearing out what Dr. Flint has contended for.

During November, 1870, Mr. Weston undertook another pedestrian feat, and this time a very elaborate examination was made of the ingesta and egesta and of various conditions of the body by Dr. Flint and a staff of associates. The results are recorded in detail in the *New York Medical Journal* for June, 1871. The feat proposed was to walk 400 miles in 5 consecutive days, and upon one of the days 112 miles were to be walked in 24 consecutive hours. Mr. Weston commenced the undertaking on the 21st of November. The examination of the ingesta, egesta, &c., had been conducted for 5 days before; it was also carried on during the 5 days of the walk, and continued for 5 days afterwards. Thus the results for three periods—before, during, and after the walk—were obtained. The subjoined tabular representation will give a summary view of the leading results noted. The walk was undertaken over a measured track marked out in the form of a parallelogram, within a large cov-

Dr. Flint's observations on the effects of the five-day pedestrian feat performed by Mr. Weston.

	Weight of body (nude).	Temperature.	Pulse.	Miles walked.	Nitrogen in ingesta.	Nitrogen in egesta.	Excess or deficiency in nitrogen egested.
Before the walk.	lb.	Fahr.			Grains.	Grains.	Grains.
First day	120·5	99·7°	75	15	361·22	323·26	— 37·96
Second	121·25	98·4	73	5	288·35	301·18	+ 12·83
Third	120·	98·0	71	5	272·27	330·36	+ 58·09
Fourth	118·5	99·1	78	15	335·01	300·57	— 34·44
Fifth	119·2	99·5	93	1	440·43	320·06	— 120·37
During the walk.							
First day	116·5	95·3	98	80	151·55	357·10	+ 205·53
Second	116·25	94·8	93	48	265·92	370·64	+ 104·72
Third	115·	96·6	109	92	228·61	397·58	+ 168·97
Fourth	114·	96·6	68	57	144·70	348·53	+ 203·83
Fifth	115·75	97·9	80	40·5	383·04	332·77	— 50·27
After the walk.							
First day	118·	98·6	76	2	385·65	295·70	— 89·95
Second	120·25	98·4	73	2	499·10	358·81	— 140·29
Third	120·25	99·3	70	2	394·83	409·87	+ 15·04
Fourth	123·5	98·8	78	2	641·71	382·89	— 258·82
Fifth	120·75	97·5	76	3	283·35	418·49	+ 135·14

ered space—namely, the Empire Skating Rink in New York. It appears that Mr. Weston failed this time to accomplish the feat he had attempted; the distance walked during the 5 days amounting to 317½ miles, and the greatest distance on any one day to 92 miles.

Dr. Flint still holds to his former opinion, and looks upon the above results as showing, to use his own words, that "excessive and prolonged muscular exertion increases enormously the excretion of nitrogen; and that the excess of nitrogen discharged is due to an increased dissimilation of the muscular substance."

Let us accept Dr. Flint's estimates of the ingoing and outgoing nitrogen. It is true, during the first four days of the walking period the exit of nitrogen was in considerable excess of the entrance; but why should this be referred specially and exclusively to muscular disintegration? There was during these four days a progressive decline in the weight of the body, the loss reaching a little over 5 lb. From the account given considerably less solid food was taken then than before and after. There existed a state of marked disturbance of the bodily functions, as shown by the depression of temperature and elevation of pulse; but little sleep was obtained; and on the third day, when an attempt was made to walk the 112 miles in twenty-four consecutive hours, drowsiness, it is stated, prevailed to such an extent that it was found impossible to make the necessary time to accomplish what had been intended. On the fourth day Mr. Weston actually broke down for a time altogether, becoming dizzy, staggering, and at last failing to be able to see sufficiently to turn the corners of the track.

Now, apart from the fact that a marked deviation from the physiological state existed when the results upon which the conclusions are based were drawn, is there anything in the results to show that in reality we have more to deal with than simply a consumption of nitrogenous material within the system beyond the supply for the time from without? Taking the figures throughout, there is not much more to be seen than a difference occasioned by a falling off in the amount of nitrogen ingested during the first four days of the walk; and it is well known that when the ingesta do not furnish what is wanted for meeting the expenditure going on (as during inanition), the resources of the body are drawn upon, and the nitrogenous matter existing in the various parts—both solids and fluids—wastes or yields itself as well as the rest. On the fifth day, after a prolonged sleep, which appears to have restored the flagging powers, the previous relation was reversed. The food ingested afforded more than enough to meet the requirements. There was a gain of 1¾ lb. in body-weight, and, according to the figures, the nitrogen discharged fell short by 50·27 grains of that which entered, notwithstanding a walk of forty miles and a half was performed.

The distance walked during the five days amounted to 317½ miles; and the excess of nitrogen eliminated during the time over that ingested appears to have been 633 grains. Presuming, for sake of argument, this to have represented the nitrogen of muscle disintegrated in the accomplishment of the work performed, we have before us the data for ascertaining how far the force producible in this way would correspond with the expenditure that must have occurred.

According to Mulder's analysis, albuminous matter contains 15·5 per cent. of nitrogen. Reckoning from this proportion, 633 grains of nitrogen will represent 4083 grains of dry albumen. Now, the force producible from the oxidation of albuminous matter has been experimentally ascertained by Frankland; and, as it occurs within the body, the oxidation of 4083 grains of dry albumen would give rise to the evolution of an amount of power equal to lifting 1540 tons one foot high.

Here we have one side of the question—the amount of work obtainable from the nitrogenous matter presumed to have undergone disintegration as muscular tissue; and so far the information in our possession may be regarded as sufficiently authentic to enable us to frame a reliable conclusion. As regards the work accomplished, we may assume, with Professor Haughton, that the force expended in walking or progressing on level ground is equal to that required to lift one-twentieth of the weight of the body through the distance traversed. The distance walked amounted to 317½ miles, and if we take the weight of the body and clothing at, say, 120 lb., this will give the performance of an amount of work equal to lifting 4490 tons one foot high, or about two-thirds more work than the oxidation of the nitrogenous matter representing the 633 grs. of nitrogen could accomplish. And, in this calculation, only the external work has been taken into consideration. There is, in reality, also a considerable amount of internal work constantly being performed—viz., that employed in keeping up the circulation, in respiration, and various other essential actions of life.

I have entered thus minutely into the question of the elimination of nitrogen in relation to muscular work because it bears in so forcible and direct a manner upon the question immediately before us—viz., the uses to which the nitrogenous alimentary principles are applied in the system. Briefly represented, the position of the matter may be said to be this:—

Many years ago it was asserted by Liebig that muscular action involved the destruction of muscular tissue. The plausibility of the doctrine, and the readiness with which the views of its author were then received, must be considered as having led to its being at once generally accepted as though it formed a scientific truth, although in reality only constituting a speculative proposition unsupported by anything of the nature of proof. It was further argued that, if muscular action involved the destruction of muscular tissue, the excretion of the nitrogenous product of destruction—urea—ought to be in proportion to the amount of muscular work performed. This seemed to follow as a necessary sequence, and the one being accepted, the other was taken for granted also. Thus, notwithstanding the absence of anything in the shape of proof, we find physiologists writing and reasoning as if the doctrine had been actually proven.

If the theory of Liebig were true, we should have to look upon nitrogenous alimentary matter as forming, through the medium of muscular tissue, the source, and the only source, of muscular power. The renewal of muscular tissue for subsequent oxidation in its turn, and evolution of muscular force, would thus constitute one of the functions of nitrogenous alimentary matter, and on its supply would, accordingly, depend our capacity for the performance of muscular work.

It is only lately that the doctrine has been submitted to the test of experiment; with what result the foregoing account of the researches of various observers has shown. Even Liebig* has now come to assert that muscular action is not attended by the production of urea. He admits that the question as to the source of muscular power has been complicated by an inference which has proved erroneous, and for which he acknowledges himself as responsible—the inference, namely, that muscular work is represented by the metamorphosis of muscular tissue and the formation of urea as a final product. While admitting this much, however, Liebig still looks to changes in the nitrogenous constituents of muscle as the source of muscular power. He assumes the presence in muscle of nitrogenous substances in a much higher state of tension than syntonine and albumen; and to these he refers the performance of muscular work taking shelter under the proposition that it is due to the liberation of the tension thus presumed to have been accumulated in them during their formation.

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ON GONORRHŒAL OR URETHRAL RHEUMATISM.†

By Thomas Bond, B.S. Lond., F.R.C.S.,

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The pathology and treatment of some so-called rheumatic diseases of the joints have been much misunderstood, and particularly so gonorrhœal rheumatism, or, as I think it might more correctly be called, urethral rheumatism.

The disease is not, in my opinion, rheumatism at all in the proper application of the word, nor is it the effect of any specific poison or constitutional diathesis. It often occurs quite independently of the acute urethral inflammation called gonorrhœa, as well as of any gouty or rheumatic predisposition; but it is dependent on a local condition of the urethra which I shall presently describe. Nevertheless, I shall continue to use the name urethral rheumatism as being most convenient.

This affection is more frequent than is generally supposed, and occurs commonly in men of an anæmic and weakly condition, whose habits and occupations are sedentary and unhealthy, and particularly does it occur in those cases in which a gonorrhœa has been treated by a long course of antiphlogistic medicine, or by an excessive and indiscriminate use of copaiba.

In my experience, it very rarely occurs in florid and healthy men. Out of upwards of fifty cases of gonorrhœa among the well-to-do classes which I have treated within the last year, I have had one case only of urethral rheumatism; while in the Lock Infirmary of St. George's Union at Westminster, where I treat over 300 cases of venereal disease yearly as in-patients, I find that a considerable number of the gonorrhœal cases among men, I may safely say ten per cent., are complicated with this disease. Those men are always the most anæmic, ill-fed, and wretched specimens of humanity possible to be seen; but it is a curious fact that the women of the same class are rarely affected by it. Again, the men are very rarely admitted for the gonorrhœa; they complain of rheumatism in the knees from cold, or say they have tender feet from over-walking. Lately I have made it a rule to examine the penis on such statements being made, and thus I have discovered many urethral discharges which would not otherwise have came under my notice; for often, when the discharge is perfectly evident and even profuse, they deny all knowledge of it, and in some cases I have no doubt they are ignorant of the local affection. The following is a typical case:—

John H——, aged thirty-five, married, a pale anæmic man, admitted to the infirmary October 6th for chronic rheumatism, said he had been subject to rheumatism for some years, and that he had never had any venereal disease; had been married many years and had children. Has much effusion in right knee-joint, complains of great pain in the heels and in balls of great toes when he walks, pain in elbows and shoulders on movement, and dull aching pains in all his limbs at night, and especially an acute pain in the back each morning. Skin dry and harsh, but complains of an exacerbation of sweating following the pain in the back. Has some congestion of the sclerotic vessels of the eye; denies that he had any discharge from the urethra. I examined, and found the lips stuck, and, on squeezing, a drop of perfectly transparent gleety fluid exuded. He then confessed that he had a sort of discharge fifteen years before, and that ever since he found the lips of the urethra glued when he made water. He said that he had been treated by many doctors and at several hospitals for rheumatism, but that his penis had never been examined before, and that nothing did him any permanent good. As an experiment I treated him with iodide of potassium, ten-grain doses, with lemon-juice and a full meat diet.—October 23rd: No improvement either in the pain or effusion; general health worse, if anything. Commenced to use a tannin-and-opium injection to-day; to leave off all medicine, to continue full diet with a pint of porter daily.—November 6th: Pain and effusion nearly gone, the appetite good, and the rest unbroken. He has increased in weight, but the discharge is not entirely cured. I shall continue the treatment another month as he has had so many relapses.

In this affection the first symptoms are the anæmic condition of the patient and the dull pain in the heels and ankles, with pain and effusion in the knee-joints, evidently a subacute inflammaof the synovial membranes and of the fibrous tissues, especially about the heel and ball of the great toe. I think I may say the effusion into the joints is invariable. It generally commences in the knees or ankles, but gradually affects the elbows, shoulders, and hands. There is also a most common and characteristic complication affecting the eyes, and this is a congestion of the sclerotic. The conjunctiva is sometimes slightly congested, and the caruncula are red and injected, but the great peculiarity is the congestion of the sclerotic vessels which are seen radiating around the cornea. This affection is quite distinct from gonorrhœal ophthalmia, and never leads to such destructive consequences. In the course of time the general health suffers very severely. The exacerbations of pain in the loins increase, and are followed by profuse perspirations every morning. The urine is scanty, the bowels are constipated, the skin is dry, the tongue coated, the face hectic, pulse feeble; great effusion occurs into the joints, with constant dull, aching pain, causing complete loss of appetite and sleep, the patient frequently remaining bedridden for months, so that the limbs often become permanently contracted unless great skill and care be used in attending to the position of the limbs. But suppuration does not occur. Lastly, and most important to be remembered in the treatment, comes the local condition of the urethra. With the above symptoms well marked I have rarely found an acute inflammation of the urethra, but I have never found the urethra in a healthy state. The condition varies considerably, from a profuse muco-purulent discharge to the slightest gleety fluid, unaccompanied by any inflammatory symptoms whatever. As a rule, pain, swelling, and redness are absent, and the discharge in the majority of instances amounts to nothing more than a slight gleet—sometimes a glairy mucus, sometimes a concreted pus. This state is of course liable to be increased and attended by irritability of the urethra or bladder, by local or constitutional causes, such as strong injections, violent exercise, coitus, &c., or by constipation, catarrh, or alcoholic drinks.

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* Proceedings of the Royal Bavarian Academy of Sciences, 1869; Pharmaceutical Journal, 1870.

† Abstract of a paper read before the Medical Society of London, Nov. 6th, 1871.

Pathology.—This disease is distinct from the specific syphilitic rheumatism, as well as from the ordinary constitutional or diathetic rheumatism; it is septicæmic, in fact a chronic pyæmia, the blood being constantly vitiated by a morbid material which is absorbed from the urethra. The exact process of the absorption I am not prepared to explain, and it is not easy to determine; for the disease is not often fatal, so that we have no opportunity of examining the urethra after death. But I am prepared to advance the theory that this absorption, whatever may be the process, goes on continually during the course of the disease, and that the abnormal condition of the blood is not maintained by the assimilation of the new materials of the blood to its altered state, as in the case when a specific poison is absorbed, such as the vaccine or syphilitic virus, the insertion of which into the blood, in however small a quantity, effects and alters the whole of the blood; and the alteration thus produced, inconceivably slight as it must be, is long maintained, the blood exactly assimilating to its altered self the materials derived from the lymph and chyle. Thus, when once inoculated, it retains by the exactness of its assimilation the taint which it has received, though after a time it may not have in it one of the particles which the taint first effected. This is the case in syphilitic rheumatism; while in urethral rheumatism the diseased state of the blood is maintained by the daily absorption of the morbid material from the urethra, the new blood being still assimilated to its normal state. Thus, as soon as the supply of the morbid material for absorption is stopped, the blood gradually eliminates the poison and returns to its healthy state. With regard to the peculiar immunity of women from this disease, I believe it to be owing to the greater thickness and coarseness of the vaginal epithelium to that of the urethra, the urethra in women being very rarely affected. It is possible also that they are less liable to it from not being pulled down by antiphlogistic treatment and the administration of copaiba. I have seen two cases of this disease in women: One, a prostitute, in whom, after I had cured the disease in the vagina, I found the urethra very inflamed and irritable. I used nitrate-of-silver injection, and both the discharge and the rheumatism soon left her. In the other case, a married lady, the symptoms were very characteristic. Pain in the heels, effusion in the knees and elbows, inflamed sclerotic, and the usual constitutional symptoms. On examination I found the vagina perfectly healthy, but a glairy fluid issuing from the os uteri, the lips of which were red and inflamed. I immediately formed the opinion that the continuance of the rheumatism depended entirely on the diseased state of the uterine mucous membrane, the epithelium of which is very similar in character to that of the urethra. I set myself immediately to cure the local condition by applications of nitrate of silver and tannin pessaries, at the same time administering steel and quinine. The recovery was very tedious, and the discharge was continually returning, always accompanied by increased effusion and rheumatic pains. It is now eighteen months since I first had the case in hand; for the last six months there has been no relapse. The lady is now in robust health; no discharge, and she has gained over a stone in weight. In this case I persevered with local remedies only, except that I gave sulphur and cream of tartar to regulate the bowels and kidneys.

I am sorry to have been unable to uphold my opinion with regard to the pathology of this disease by anatomical or microscopic examination, the reason of which I have before mentioned, so that I am obliged to rely on the experience I have had in its treatment and my observation of its many points of analogy to pyæmic disease to maintain my proposition. The disease is considered very difficult to cure, and certainly is so when treated by the usual antiphlogistic and constitutional remedies recommended in ordinary synovitis. Acute inflammatory symptoms being very rarely present, antiphlogistic measures are not often resorted to, and I have never known any good result from their employment, except the temporary removal of scalding and urethral inflammation. Blistering, Scot's bandage, perfect rest, friction, and mineral baths, I have used without the slightest benefit. Iodide of potassium and colchicum I have found worse than useless, and I never give it except, as in the case I have related, in order to compare its effect with the result of similar doses in ordinary rheumatic affections. The effect of iodide of potassium is to increase the anæmia, weaken the pulse, impair the appetite, and often to cause petechiæ or purpura without relieving the pain or effusion in the slightest degree. The general treatment I have found most efficacious is quinine and hydrochloric acid, or steel and strychnine, together with the administration of port wine, stout, eggs beaten up with brandy, and general high feeding, with soothing local applications, rest, poultices, and wet backings; but even then the disease often cannot be cured, although the appetite and general health return, and the pain and swelling almost disappear, yet the disease is sure to recur on leaving off the treatment should any urethral discharge remain. A very severe case first led me to form the opinions I have expressed. I had treated a gentleman for effusion in the knee-joints for several months without success. Knowing he had a slight urethral discharge which he thought nothing of, I determined to cure it, and told him I should try a course of injections. I commenced with alum and lead, which brought on a good deal of discharge, so I discontinued it. The discharge then diminished, but a good deal of irritation remained, so I prescribed half a drachm of tannin to six ounces of water, and mixed it myself. The tannin dissolved easily in the cold water, I then added two drachms of tincture of opium to soothe the irritation. The clear solution of tannin immediately became densely clouded with a flocculent brown substance, in very minute suspension, but subsiding in about half a minute into a light precipitate filling about half the bottle. The idea immediately struck me that it ought to be a very valuable injection for gonorrhœa and urethral discharges, for an inflamed surface requires a constant application to properly control the disease, the occasional application acts only as an irritant. Here I had an injection which answered all the purposes of the soluble bougie without any of the inconvenience; for by simply shaking up the injection, and using it immediately, the flocculent substance, which I conclude is gallotannate of morphia, is precipitated out to the sides of the urethra; and by keeping the injection in the urethra about a minute, and then letting it gradually escape, the clear fluid comes away, but leaves all the sediment behind: and I have found, from a number of experiments, it takes about four hours for all the solid residue to escape. It can be seen as a dark-brown spot gradually being deposited on a piece of lint placed over the meatus; thus by injecting every four hours, I found that I could keep a powerfully astringent application in constant contact with the urethral mucous membranes. In the case I have been referring to, I ordered this injection to be continued several weeks. It cured the discharge entirely, and the rheumatism soon after began to improve; and in three months, the effusion and pain having entirely disappeared, all treatment was left off. Directly after the patient contracted another discharge, which he neglected, and in less than a month the rheumatism returned worse than ever, the effusion was very great, and the pain very severe. He became extremely emaciated, with profuse sweats, and hectic—in fact, with every symptom of blood-poisoning. His friends took him into the country for three months to try a course of the Bath waters. He came back little improved, with the effusion into the joints no better, and still with the gleety discharge. This I again cured with the tannin injection; and in six months from that date he increased in weight from nine to eleven stone, the rheumatism got quite well, and he has since had no return, nor is he at all liable to rheumatic pains.

Deduction: That urethral rheumatism is a slow form of pyæmic poisoning, due, not to a sudden absorption,* but to a gradual vitiation of the blood by progressive absorption of the urethral discharge; that the vitiated state is not maintained independently of the local disease; that, when the discharge is thoroughly and permanently stopped, the rheumatism may soon be cured, and has no tendency to return, except by the access of a fresh urethral discharge.

ALCOHOLIC PARAPLEGIA.

By SAMUEL WILKS, M.D.,
Physician to Guy's Hospital.

The above name is one I have long used in reference to a class of cases which are not at all uncommon, but have been hitherto left undescribed, in a manner worthy of their importance, in systematic works on medicine. In my published lectures on Diseases of the Nervous System I briefly referred to the affection as one requiring our earnest recognition, and gave some particulars of the cases which I had then seen. Since that time several others have come under my notice, and during the last year no less than four more. These facts had led me to commence a paper for the consideration of one of the medical societies, when I met with an essay by Dr. Handfield Jones, contained in the *Practitioner* of last month, on "Epilepsy and other Nervous Affections resulting from the excessive use of Alcohol." A perusal of this essay shows me that my object is now unnecessary, and that I need do little more than recommend its attentive perusal to the readers of The Lancet, and say for my own part that I can endorse nearly all the statements therein contained. I have seen epilepsy and many other nervous diseases result from alcohol, although my attention has been more especially drawn to the spinal affection. In order, however, to strengthen Dr. H. Jones's statements, I will quote from a lecture given by me in October, 1867, as it contains an epitome of what I had then observed:—

"*Drunkards' or Alcoholic Paraplegia.*—I do not know that this is deserving of a distinct name from its possessing any pathological peculiarities; but as arising in connexion with a very well marked exciting cause, it may deserve your especial attention, and I refer to it the more especially because I believe authors have generally overlooked it. I have already told you how long-continued habits of intemperance in alcoholic drinks tend to the production of a fibrous or fatty degeneration of the various tissues of the body, and that, as a consequence, the membranes of the brain and spinal cord become thickened, and the organs within wasted. This, of course, would give rise to what might be called a general paralysis of body and mind. But besides these general results, we often meet with more direct effects on the spinal cord, and to these I particularly refer. I have now seen so many persons, especially ladies, who have entirely given themselves up to the pleasures of brandy-drinking, and been rendered paraplegic, that I have become pretty familiar with the symptoms. From what we hear of our continental neighbors, it would seem that the diabolical compound styled 'absinthe' is productive of exhaustion of nervous power in even a much more marked degree; it would appear that the volatile oils dissolved in the alcohol give additional force to its poisonous effects. Of course, drunkards of all descriptions suffer from nervous and muscular weakness; but, as I before said, it is more especially in the legs that the effect is most striking. A loss of power is first observed, accompanied by pains in the limbs, which might indicate a chronic meningitis of the spinal cord, and in some cases there is anæsthesia. There is, at the same time, some amount of feebleness of other parts of the body as well as the mind, and thus an approach to general paralysis is produced; but sometimes the symptoms are almost confined to the legs, and resemble in character those of locomotor ataxy."

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* It was mentioned during the discussion that sometimes sudden and acute inflammation of a single joint takes place. I have seen two such cases in which one ankle was affected. The gonorrhœa was cured immediately by injection in one case, and the ankle got well in a fortnight, the health not being affected. In the other the discharge was allowed to run on, and the acute inflammation became chronic, all the joints became affected, and the patient suffered some months from urethral rheumatism.

Since this was written I have seen several cases of a similar kind; and it is worthy of remark that they occurred mostly in women. I would repeat that something more definite is intended by the term "alcoholic paralysis" than that general muscular and nervous debility which is as well known to the public as the profession; for no character is more easily recognised on the stage of the theatre than the victim of chronic drunkenness. That which I wish more particularly to draw attention to, is the case where alcohol is seen exerting its influence more directly on the spinal cord, making paralysis the leading symptom. Although there is a tendency to a particular form of degeneration in alcoholism, it is not very evident why one person should be affected in one way and another in another; or why one person should have cirrhosis of the liver, another Bright's disease, and a third atrophic meningo-cerebro-myelitis. I use this term since the changes which the brain and spinal marrow undergo are probably identical with those which are found in the two other affections just mentioned. As regards the brain, the tissue degenerates and the membranes become thickened, and thus the mental condition of the "brainless sot" is familiar to all. It very often closely resembles that observed in the general paralysis of the insane; which is not remarkable, seeing that the two affections may have in some cases a similar pathology. Now, in alcoholic paraplegia there is every reason to believe that the spinal cord is affected in the same way as these other organs, and the following is the usual condition of the patient: —She lies in bed or on a couch, complaining of severe pains in all the limbs, more especially in the lower ones, which are much wasted, or of a sensation like electric shocks running through them, together with numbness and considerable anæsthesia, and at the same time only slight power of movement, or total inability to stand. With the addition of the akinesia, the symptoms are not unlike those of ataxia, which in its ordinary form appears to be comparatively rare in women. In one case there was hyperæsthesia. In nearly all the cases the liver has been enlarged; there has been sickness, and all the other usual signs of chronic alcoholism. I alluded in my lecture to the existence of pains in the limbs from which drunkards often suffer before any marked signs of paralysis have shown themselves.

As regards treatment, this is hopeful, and should always be attempted; for I think it must be confessed that it is impossible to tell what kind and amount of change has occurred in the centres to produce paralytic symptoms. If the patient be young and the tissues not much degenerated, I should recommend the ordinary treatment, and especially such medicines as the iodide or bromide of potassium, before commencing galvanism and a tonic course. What, however, I would especially insist upon is the removal of the primary cause of the complaint. This seems a common-sense recommendation; but, I am sorry to say, is one not always enforced. Sometimes the reason is that the doctor fears for his position as health-proprietor of the family should his hints be offensive, or he has not moral courage to unfold an unpleasant truth. Sometimes, however, he will most conscientiously refrain from recommending a discontinuance of the alcohol for fear of the results, such as the sudden dissolution of the patient, or an attack of delirium tremens. He need not have the slightest fear on these grounds, for no harm, but only good, will ensue from its withdrawal. I am aware that opinions are at variance in this matter, but nevertheless I enforce my own views strongly, with a large amount of experience to support me. I have never yet seen a person die or suffer from delirium tremens or any other disorder in consequence of the complete withdrawal of alcohol when the system was being poisoned by it. On the other hand, I have seen such remarkable recoveries after its total discontinuance as would certainly surprise those who had never ventured upon the plan. I have seen persons, more especially ladies, lying on their beds surrounded by friends waiting for their last moments, where scarcely a mouthful of food had been taken for months; where the prostration, increased by urgent sickness, was extreme, and where they were constantly plied with brandy to keep them alive a little longer, and yet in such cases, where the diagnosis was clear, from the blood of the patient being overcharged with poisonous elements, and the room stinking with the fetid vapors of the body, I have recommended a withdrawal of every drop of the so-called "stimulus," and with effects more marvellous than anyone who has not adopted the plan can conceive. Unfortunately these cases of alcoholism are so bound up with moral considerations, and in women so often associated with bodily or mental suffering, that it is most difficult to publish them in all their details. Three cases which I have witnessed during the last year have made a great impression upon me. A lady not far from my residence, the unfortunate subject of alcoholism, and having considerable weakness of the legs, was reduced at length to the utmost stage of prostration by want of food and constant retching. A little brandy was put from time to time between her lips, in order, as was hoped, to eke out her existence a little longer. The end apparently approaching, straw was laid down in front of the house, and her children were sent for in order to take a final farewell, when, after repeated urging on my part, all stimulus was suspended. The sickness soon ceased; the blood became gradually depurated; after a few hours a little food was taken; and in a fortnight's time this lady was sitting at the dinner-table with her family. I can say nothing, of couse, about the likelihood of relapse, as this opens up another subject. But lately, also, I have been seeing a tradesman and his wife, with Mr. Joyce of Nottinghill, both of whom were addicted to intemperate habits, and both, most remarkably, had almost complete paraplegia. We prevailed on the husband to relinquish his drink, and he began at once to improve; but we had no influence on the female, who was approaching the state of delirium tremens. In another case, that of a medical man, who, after drinking hard, became so ill that he took to his bed, had epileptiform attacks, ate nothing, and was constantly retching, his wife standing over him administering brandy and champagne from time to time to keep him alive a little longer, I succeeded, after several attempts, in inducing his wife and two medical attendants to stop every drop of alcohol. When this was done the patient soon cried out for drink; but, after imploring in vain for some time, he was violently sick, and then sank into a sound sleep. Upon waking he took a little beef-tea, in a few hours ate some solid food, and within a week was back again in his practice.

I do not wish to discuss in this place the merits of alcohol as a remedy, as it is one I constantly use with the utmost advantage; but simply to state my experience that I have never seen the slightest harm accrue from the sudden deprivation of the accustomed stimulus, but, on the other hand, have had the satisfaction of knowing that I have saved fellow-creatures' lives as certainly as if I had plucked them drowning out of the water. I cannot conceive what the objection is to the withdrawal of the alcohol under the circumstances I mention, any more than I can conceive why, when the system is saturated with poison, and the patient dying from the effects of it, such a gross burlesque on the homœopathic doctrine should be practised as to continue administering it.

The purport of this paper is more especially to draw attention to the fact of paralysis, and more particularly spinal paralysis, occurring as a result of alcoholism; and therefore that when a medical man is called in to see a case of this kind, he should remember intemperance in drinking as a possible cause, just as he would if he found an enlarged liver.

If the affection should turn out to be in any way peculiar in its pathology it will certainly deserve a distinct appellation; but even should the morbid changes in the cord, together with the resulting symptoms, resemble what is seen in other forms of paralysis, I would still recommend the adoption of such a term as alcoholic paralysis as significant of its cause, for we are warranted in so doing by the use of the expression puerperal, syphilitic, or uræmic epilepsy (eclampsia) in reference to the origin of the fits when arising under special circumstances.

Grosvenor-street, Feb. 1872.

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SOME RESULTS FROM THE USE OF CHLORAL HYDRATE.

By J. Wilkie Burman, M.D. Edin.,

Assistant Medical Officer, West Riding Lunatic Asylum.

The following results, in two cases treated with chloral hydrate during my experience as assistant medical officer at the Devon County Lunatic Asylum, may perhaps be interesting to the profession, as forming a sequel to other similar cases already described by Dr. Crichton Browne and others. I am indebted to Dr. Saunders, of the Devon Asylum, for the use of the notes of these two cases, and I shall just transcribe them as I entered them in the case-books of that asylum. In both, a rash, accompanied by fever and sore-throat (simulating scarlatina), occurred during the administration of the drug; and in both there was a *relapse* of all the primary symptoms, in one case on the *fourteenth*, and in the other on the *twenty-first*, day after the first appearance of the rash.

Case 1 (No. in Register 4011).—Female, aged thirty, married. Admitted, on Sept. 13th, 1870, in a state of acute melancholia and in feeble bodily health. Was ordered, on admission, the following mixture: Chloral hydrate, two drachms; tincture of opium, three fluid drachms; water, to twelve fluid ounces: one fluid ounce to be taken three times a day. The medicine seemed to have a good effect, as she improved under its administration. On the morning of the 24th of September, having taken the above-mentioned mixture about ten days, and a double dose having been given on two or three occasions, it was noticed that she was "covered with a scarlet eruption," which had "come out" during the previous night. The redness of the skin was almost universal, and, disappearing for a time under pressure, resembled more than anything else the eruption of scarlatina. It was accompanied by fever and sore-throat, the tonsils and upper parts of the pharynx being visibly red and congested. The pulse was 120; the tongue was furred, but did not present that peculiar "strawberry" appearance so frequently observed in cases of scarlet fever. There was neither nasal nor conjunctival catarrh. It was considered advisable to strictly isolate the patient. The chloralic mixture was stopped, and simple salines were given instead. On the 29th the patient was still feverish, but the throat was not worse, and, the rash having died away, desquamation had commenced on the hands and arms. As she was restless and excited, she was, on Oct. 1st, ordered *scruple* doses of chloral night and morning. The febrile excitement had almost gone, and desquamation had gradually spread all over the body, when, on the morning of Oct. 7th, *fourteen* days after the first appearance of the eruption, she was found to have had a complete relapse, and to be again "feverish and covered with a scarlet rash," which had come out during the preceding night. The chloral was again stopped and salines given. The eruption was this time not so universally spread as before, and on Oct. 15th desquamation, more considerable than hitherto, set in, and on the 27th was nearly completed. On Nov. 6th she had quite recovered from her bodily illness, and was sent back to her ward, having been isolated for a period of six weeks. Her mental condition remained unchanged. There was no albuminuria during the whole progress of the case.

Case 2 (No. in register 8787).—Female, aged twenty, single, an imbecile of decidedly rheumatic diathesis and predisposition, who had already been in the asylum several years. On Dec. 2nd, 1870, being very choreic, and so much excited that it was necessary to place her in a padded room, she was ordered scruple doses of chloral three times a day. This soon caused considerable amelioration in her condition, but in process of time seemed to lose some of its effect, so that the third dose of the day, that to be given at bedtime, was ordered to be doubled. Eighty grains of the drug were thus taken in twenty-four hours. On Dec. 19th, in order that she might take a nervous tonic—consisting of a grain each of the sulphates of iron, zinc, and quinine, to be given three times a day,—the administration of the chloral was confined to a night draught of two-scruple doses. On the morning of Dec. 28th it was observed that she was covered with a scarlet eruption, which had come out during the night previous. The general appearance of the rash was much the same as in Case 1, and it was likewise accompanied by sore-throat and febrile excitement, and characterised by the absence of the "strawberry" tongue. Except that strict isolation was not maintained, the general treatment was the same in this as in the former case—the chloral was stopped and simple salines were substituted. On Jan. 2nd, 1871, the eruption was fading, and desquamation had commenced on the face and neck, and, gradually spreading over the whole body, had, on the 16th, almost finished. On the morning of the 17th—the twenty-first day after the first appearance of the eruption—she was found to have had during the night a complete relapse of the rash and febrile symptoms; the rash being on this occasion neither so intense in color nor so universally spread as before. On the 21st desquamation had again commenced, and, gradually spreading over the body, nothing remained on Feb. 6th except a slight peeling of the cuticle of the hands and feet. On Feb. 11th she had quite recovered from the exanthem. As in Case 1, there was no albuminuria, though the urine was examined daily after desquamation had commenced. She still remained choreic, however, and it was not until the lapse of several weeks, under a course of treatment by nervous tonics and alkalies in alternation, that the chorea left her. In both these cases the skin was oiled, and occasional tepid baths given, during the periods of desquamation. No other such cases have occurred since, and I cannot find that any such cases had been noticed at the Devon Asylum previous to the introduction and use of chloral.

Remarks.—I believe it was at the Devon County Asylum that chloral hydrate was first tried, to any considerable extent, as a therapeutic agent in this country, for its use was commenced there immediately after Dr. Richardson's notice of it at the meeting of the British Association at Exeter in 1869. The good effects from the use of the drug in that asylum, described by Dr. Saunders in its Annual Report for 1870,[*] have since been amply verified and extended, and a steadily increasing consumption of it bears witness to the augmented esteem in which it is now held in that institution, where, when I left, from three-quarters to a pound was being used weekly. I saw about twenty-five pounds of the drug administered, chiefly in cases of excitement, with beneficial result, and in only two (those just described) out of the many cases in which it was given did any untoward symptoms occur, and these, as we have seen, were, happily, neither of a fatal nor of an alarming nature. Without entering into needless details of cases (which are scarcely ever read when given), I may state, as my experience at the Devon Asylum, that chloral has proved itself to be a most valuable hypnotic and sedative, that it has succeeded after extended trial of the more common sedatives was unavailing for good, and that it can, by reason of its not causing headache, sickness, dryness of the tongue or other diminution of the secretions, be used as a sedative in cases of excitement associated with phthisis or bronchitis, the presence of which would contra-indicate the use of morphia and opium. I think it is more especially in this class of cases that chloral will be found to be peculiarly valuable. In other respects it, at any rate, adds one more to the list of drugs on which we may "ring the changes" in chronic and obstinate cases, and in these it is used with best effect by combining it with morphia, opium, or bromide of potassium, according to the nature of the case. Chloral is, comparatively speaking, speedy and evanescent in its action, and its judicious combination with other drugs of a sedative nature seems to bring about the desirable prolongation of its effect.

As to the method of its administration at the Devon Asylum, and the doses, I may say that it is now always given in the form of a syrup (home-made) containing ten grains to the fluid drachm; and that the usual doses of the drug, when uncombined, are from twenty to sixty grains; when in combination, from ten to forty grains will suffice in the majority of cases. The lowering in price of a drug, consequent on its increased consumption, has been well exemplified in the case of chloral, which, though at first 6*s.* per ounce, is now only about 9*s.* a pound—almost as cheap as the bromide of potassium, and certainly at least as useful an addition to our Pharmacopœia. It would be invidious to mention names, but it is very important that the drug should be purchased from respectable houses, and it should be stipulated that it must be fresh, pure, and carefully prepared. Both the solid cake and the solution ought to be kept in carefully stopped bottles, otherwise, I believe, the preparation loses strength. The cake deliquesces a little in process of time, and loses to a certain extent that sharp pungent odor which it has when fresh; and, on this account, it is advisable, when a bottle of the solid preparation is opened, to convert it into syrup at once.

West Riding Lunatic Asylum, Wakefield, Nov. 1871.

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ON THE DIAGNOSIS OF TYPHOID FEVER IN ITS EARLY STAGES.

An Abstract of a Clinical Lecture delivered in March, 1871, at Addenbrooke's Hospital, Cambridge.

By P. W. Latham, M.D., F.R.C.P., &c.,

Physician to the Hospital.

I have now described to you, gentlemen, most of the symptoms which may accompany typhoid fever as far as the twelfth or fourteenth day, when, as before stated, mild and severe cases may generally be discriminated from each other. One symptom, however, and that a most important one, I have still to discuss—namely, the variation in the temperature of the body which takes place from day to day during the progress of the disease, and which can only be accurately determined by means of the thermometer. Within the last few years very great attention has been given to the indications furnished in many diseases by this little instrument, but in no other disease have the investigations been so numerous as in the one under consideration, and the results obtained amply repay the trouble. In typhoid fever the thermometer is indispensable. You see in the wards how much importance I attach to the charts at the bedside of the patients. On these charts are records of the patients' temperature, taken by the house-surgeon twice at least daily, at 8 A.M. and 6 P.M., and marked by a black line on the chart. The frequency of the pulse is also recorded at the same time by a red line. We have likewise tabulated the number of the evacuations in the preceding twenty-four hours, whether loose or formed, and, if containing blood, its quantity. These charts represent very true pictures of the progress of the case, and by looking at them I can most generally tell, before I ask either the patient or the nurses any questions, whether that progress is favorable or otherwise. From the information the thermometer gives me I can fully endorse the following statement:—

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[*] I might perhaps state that in this Report there may be found short account of some of the physiological effects of the drug on myself, observed by Dr. Saunders.

"The physician who judges of fever cases without a knowledge of the facts of thermometry, and without taking note of the temperature, is like a blind man trying to find his way. With much practice and great intelligence, the blind man may succeed; but he will more frequently fail, and always do, only with great difficulty and unsatisfactorily, what to him who sees requires no effort."

Let me now show you how far this is true. During the first four or five days the general symptoms which may then, as I have told you, accompany the disease—viz., the rigor, the languor and feebleness, headache, epistaxis, giddiness, pain in the back and aching of the limbs, the appearance of the tongue, the state of the bowels, the condition of the urine, &c.,—may not be very distinct, or any one of these morbid symptoms may be entirely absent. In a considerable number of cases, in fact, it would be impossible for you to say, without using the thermometer, whether the patient were suffering from typhoid fever or not. But the thermometric course of the disease at this time, unless it supervenes on some other malady, is very regular; and by taking the temperature at 8 A.M. and 6 P.M. for three days the presence of typhoid fever may be decided. On the other hand, one single observation may, with very great probability, negative the existence of the disease.

The following is the formula (from Wunderlich) of this initial stage:—

	Morning.	Evening.
1st day	98·6° F.	100·4° F.
2nd "	99·4°	101·4°
3rd "	100·4°	102·6°
4th "	101·6°	104°

If, then, a person, previously quite well, feels uneasy, perhaps has a rigor, and in the evening we find his temperature about 100·4° or 101° F., falling the next morning about a degree, rising again in the evening, and approximately following the above course, the disease may be diagnosed with tolerable certainty.

On the other hand, the disease is not typhoid fever if (1) on the second, third, or fourth evening the temperature approximates even to the normal (98·6° F.); (2) if during the first two days the temperature rises to 104° F.; (3) if between the fourth and sixth days the evening temperature of a person under middle age does not reach 103°; (4) if the temperature on two of the first three evenings is the same; or (5) if it is the same on the second and third mornings. From the fourth to the tenth day the evening temperatures are tolerably uniform, the highest being most generally on the evenings of the fourth, fifth, or sixth days, and reaching from 104° to 105·5° F., or even higher. The morning temperatures are from 1° to 2·6° F. lower than the evening ones; on the fifth, sixth, and seventh days the variations between the morning and evening temperatures being less than take place from the sixth or seventh to the ninth or tenth days. During this period (from the fourth to the tenth or twelfth day), if the general symptoms are obscure, an absolute diagnosis may not be readily made, and the disease may be confounded with several others, unless thermometric observations extend over several days. Here, again, the thermometer gives us then very definite information. Wunderlich tells us: "That if in a youth or individual of middle age, who was previously well, an illness has continued from five to ten days, and we find evening temperatures of 103·4° to 105° F., or a little over, alternating with morning temperatures of 1·4° to 2·6° lower, and without any other disorder showing itself to explain the elevation, and without the patient previously being subject to gross neglect, then we are justified in positively diagnosing the disease as typhoid fever." There is at the present time in the hospital a girl in whose case the application of this law materially helped us to arrive at a correct diagnosis; from the ninth to the twenty-first days of the disease, as you see from the chart, her evening temperatures mostly reached 103° to 103·8° F., with some morning temperatures 1°, and some 2° lower. This, excepting small successive crops of rose spots, was the only distinct evidence of the existence of the disease; the other symptoms—the condition of the tongue, the character of the evacuations, &c.—not being at all characteristic. The normal temperature in this case was not reached until the thirty-fifth day.......

To Professor Wunderlich's work on temperature I am much indebted for the information it affords. I last year in my clinical lectures gave you copious extracts from it on febrile diseases in general, on pneumonia, ague, &c. It is soon to appear in an English form, translated for the Sydenham Society,* and I recommend it to your careful study from the great importance and extreme interest of the subject.

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ON THE

TREATMENT OF RETENTION OF URINE IN IMPERMEABLE STRICTURES OF THE URETHRA.

By P. A. O'Connell, M.D.,

Late Medical Director of the "Ninth Army Corps," U.S Army, Etc.

It happens not unfrequently that persons suffering from bad stricture of the urethra, but who, under ordinary circumstances, can pass water well enough to satisfy themselves and relieve the bladder, become, from causes unknown, suddenly unable to do so. The fear of treatment, the inability to meet the expense perhaps, or even reckless indifference itself, may have made them neglect proper attention to their condition heretofore. They were satisfied to let "well enough" alone; but now they cannot pass a single drop of water. The bladder becomes distended; their agony and fear combined render them desperate, and prepare them for any means of relief; and, finally, they send for the surgeon.

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* Since published, translated by Dr. Woodman.

When the surgeon sees the case the catheter is resorted to of course, but the attempt to pass it fails. Instruments of different sizes and material, perhaps, are tried, but in vain; and after prolonged, patient, and skillful labor over the case, the surgeon finds himself unable to effect an entrance through the stricture by any means at his disposal. The stricture is an impermeable one; baths and such aids to treatment have failed to break up the embargo; the patient is suffering intensely from the retention of urine; no time is to be lost, and what is to be done?

In two cases of this kind which have come under my care I have been able to relieve the patients upon the principle of suction; and the simplicity of the method, as, also, its advantages if upon further trial, it be found to be of universal application, prompt me to make it public.

In the first case which presented itself the gentleman had suffered from retention of urine on account of stricture before, and, consequently, was not easily alarmed. He was always intending to submit himself to treatment for the cure of the stricture; and this *in*tention became very determined and positive during his attacks of *re*tention; but relief from immediate danger made him forget his resolution, and he would always postpone action. When I saw him, however, the usual means of relief had failed. Baths, opiates, and the catheter had been resorted to by a medical gentleman of skill to whom he had applied for assistance, but all in vain, and his sufferings were very severe. I made careful attempts to pass instruments, but without success. The other means of relief that suggested themselves to my mind had failed already, and the sufferings of the patient required that something should be done immediately.

Having upon my office table an india-rubber hand-syringe, consisting simply of a rubber pouch or ball, with a hard rubber stem to it, that I generally used as a part of Politzer's apparatus for inflating the inner ear, it occurred to me that it might be made use of as an exhauster—a suction instrument,—and that by this means, perhaps, the stream of water could be started. Acting upon the idea, I took a catheter of medium size, made a perforation, in its extreme end, and passed it *down* to the stricture; then squeezing the rubber pouch so as to drive out the air, I connected it by means of a short piece of india-rubber tubing with the catheter already in the urethra, and allowing it to expand gently, instructing the patient at the same time to make a gentle effort, *and only a gentle effort*, to pass his water, I had the satisfaction of learning that the experiment had become a success, and that the man was relieved.

As this was merely a transient patient whom I had not seen before and did not see again, I do not know what became of him afterwards. He stated, while in agony, that he would return on the following day to place himself under treatment for his stricture, but, as in the previous instances he had told of, no doubt relief brought with it carelessness, and I saw him no more.

In the second case in which I tried this method, the man was a reckless, careless liver; also a procrastinator, for whose procrastination, however, fear of anything bearing the name of "operation," perhaps also a chronic scarcity of money, acted, to a great extent, as cause.

The use of the india-rubber bulb, exhausted and then fixed to the catheter after its introduction *as far as the stricture*, succeeded in this case also. Once afterwards, when the passage had become closed again, and his experienced efforts to relieve himself had failed, so that the case presented apparently the same necessity for the use of the suction apparatus as before, percussing the bladder after all other efforts had failed (this was done for the purpose of ascertaining the extent of the distension) alone started the flow, and he became relieved without any further operation. This latter occurrence suggested the probability of the retention having resulted from some slight mechanical obstruction inside the stricture which became dislodged by the percussion, and thus allowed the water to pass. Whether such a condition of things would interfere with the success of the method of treatment by the suction conducing rather to retain the obstruction *in situ*, must be the subject of further experience.

The attention of surgeons is called to this method of relieving retention of urine from impermeable strictures, when other methods have failed, with the hope that further careful experiment will develop its value fully.

No expensive apparatus is required, a simple catheter perforated at its extreme end (although a hollow tube without side openings through which the lining of the urethra can be acted upon would be better), and a hollow india-rubber ball that may be connected with the catheter by means of a short piece of india-rubber tubing, is all the paraphernalia required; and I doubt not that its use will relieve much suffering, and give time for subsequent calm and deliberate treatment for the permanent cure of the stricture itself.

Introduce the tube, or catheter, as far as the stricture; let the suction be gentle and continuous, and let the patient assist, by a very easy effort, to void urine during the operation.

It is possible that a common syringe of any kind might be used for this same purpose in place of the india-rubber bulb, but as more is expected from the persistence of the force working continually in the right direction than from its violence, there seems to be good reason for adhering to the original method. A common syringe, used with rashness, is capable of doing injury in cases of this kind.

Vienna, Austria, Feb. 1872.

CASE OF ABDOMINAL ENCYSTED DROPSY CO-EXISTING WITH PREGNANCY; OBSCURE DIAGNOSIS.

By Angus Mackintosh, M.D.

In October last year the mother of B. J——, a carpenter's wife, engaged me to attend her daughter in her third confinement (having been with her in her previous accouchements), which was expected towards the end of November. She was thirty-five years of age, healthy and strong.

On November 20th I was called in great haste to see the patient, the messenger stating that she was in labour. I arrived at the house in a short time, being only a distance of three miles from my own residence. On being questioned, she said it was her full time, and complained of considerable pains in the lower part of the abdomen. From her enormous size she expected twins, as did her neighbors. The abdomen was greatly distended, and appeared almost uniform—i.e., not protruding more to one side than another. On examination per vaginam, the os uteri, which was very high up, could only be reached by a little difficulty—indicated no symptoms of labour; in fact, it was actually closed. I told her she must have a little more patience, and not to send for me again till she should experience real labour pains.

In course of a week thereafter I was summoned during the night to attend her. The message was that "there was no mistake now of her being in labour". To my surprise and disappointment, on examination I found the os in the same state and position that I left it the week previous. I said she had again fooled me, to which she replied that something must be done, for she could live no longer that way. I placed her on her back and examined the abdomen for the first time, and, to my great astonishment, found it enormously distended with fluid. I told her that she was dropsical and not with child, of which statement she appeared rather sceptical. I proposed an operation, as she was suffering from great dyspnœa, from the upward pressure, no doubt, of the diaphragm, and also from much flatulent disturbance by compression on the bowels and stomach. Next day I called to my assistance an aged and experienced medical gentleman, and, after a thorough examination, we failed to detect any indication of a fœtal existence, either through the parietes of the abdomen or the vagina. Consequently we considered it necessary to operate immediately, the symptoms being urgent. We placed her at once on the edge of the bed, and relieved her—by tapping in the median line, three inches below the umbilicus—of six gallons of water, at which she expressed great relief. In a month from the first operation I again tapped her of four gallons of a gelatinous and fetid liquid. The third operation was a fortnight after the second, when three gallons were taken; the fourth eight days after the third, when two gallons of a grumous heavy fluid were taken. The patient daily got weaker in spite of tonics and stimulants. I suggested a consultation, as the features of the case were becoming more serious in consequence of constant urging and sickness, with great oppression in breathing. Assisted by my esteemed friend, Dr. William Hender, of Callington, I tapped her for the fifth time—three weeks after the fourth operation,—and removed four gallons of brown, fetid, and partly putrid liquid.

After the operation was completed we could detect on the left side a circumscribed and well-defined tumefaction, measuring about five inches in diameter, which we concluded after a careful examination to be a serous cyst. On reintroducing the trocar and canula through the same opening, and turning it at an acute angle in the direction of the swelling, my assistant at the same time pressing the swelling towards the trocar, I felt it most distinctly penetrating the cyst. On withdrawing the trocar there was a gush of transparent and almost colorless serum, to the amount of at least one pint, quite different in specific gravity and color from any seen before. In two months' time she was, altogether, tapped of twenty gallons.

In a week from the last operation I was asked to see her, as she was said to be very ill. On entering the house I was bewildered to find that she had given birth a few minutes before to a living, well-developed female child. The child and herself are doing well, though she has suffered at times from abdominal tenderness. It is now a month since her confinement, and there are no signs as yet of any evil consequences.

Remarks.—This case appears interesting owing to the rarity and infrequency of its complications. Menstruation ceased about nine months before the first operation. The mammæ were flabby and very little enlarged; the abdomen enormously distended with fluid, so much so that the pressure on the venous trunks internally caused great enlargement of the veins externally. Palpation and the stethoscope failed to detect any reliable signs of a fœtus, and examination per vaginam did not indicate a state of pregnancy. These symptoms, accompanied by dry skin, great thirst, scanty urine, and general emaciation, convinced us that we had to do with a case of ascites; and the result shows the extreme difficulty of arriving at a correct diagnosis in some cases which may appear at first sight easy, and proves the necessity of carefully weighing every symptom and sign to guide us in obscure cases like that narrated. The fœtus must have been forced upwards and backwards into the hollow of the spine by the fluid, so as to completely obscure any palpable indications of its presence, which fact no doubt, saved the uterus from being punctured by the trocar. I believed the pressure on the portal circulation caused congestion of the internal organs; hence the dropsy. The cyst associated with ascites I can faintly understand, though, so far as I know, of very rare occurrence, and of itself worth recording. Two facts, however, connected with the circumstances puzzle me: first, that labour and birth were protracted for two month at least in consequence of dropsy and operations; and, secondly, that it was possible for a fœtus to survive for seven months under such adverse and unfavorable circumstances.

Callington, Feb. 1872.

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CASE OF

CARIES OF THE OSSA PUBIS FOLLOWING DELIVERY; SEPTICÆMIA; DEATH.

By R. Eardley-Wilmot, M.R.C.S., L.R.C.P.,

Resident Accoucheur to King's College Hospital.

The following case, illustrating an extremely rare puerperal complication, with fatal issue, and complete post-mortem examination, is, I think, of sufficient interest to deserve publication. The notes were very carefully taken by Mr. J. H. Philpot, clinical clerk.

Jane C——, aged twenty-three, was admitted into King's College ward on Jan. 29th, 1872, under the care of Dr. Priestley. She had been perfectly healthy up to Dec. 1st, 1871, on which day she was delivered, easily and naturally, of her first child. Convalescence seemed to progress favorably till the third week after delivery, when she began to suffer occasional pain in the hypogastric and left inguinal regions, which much increased on moving the left leg. At the same time an abscess formed within the labia, anteriorly, and, bursting spontaneously into the vagina, discharged much foul, creamy matter. This discharge continued throughout her illness. Marked constitutional disturbance now set in—rigors, loss of appetite, and rapid emaciation. To these symptoms a harassing night cough and extensive bedsore over the sacrum soon became added.

On admission, she was seen to be extremely emaciated and prostrate, suffering from pain and tenderness in the hypogastrium, aggravated by any attempt at motion, and from an extremely offensive discharge, thin, dark, and copious, which poured in gushes from the vagina on the slightest movement. The left leg was abducted and motionless, showing some slight redness and œdema over Scarpa's triangle. On vaginal examination, the uterus appeared healthy, and normally involuted; no orifice for the exit of discharge could be detected in any part of the vaginal tract. The lungs seemed healthy on examination; tongue red, glazed, and aphthous; the expression of the face extremely anxious; the breath had a remarkably sweet odor. Pulse 144, full and non-resistant; temperature not much elevated, but showing evening exacerbations. Appetite fair; slept very badly. She was placed on generous diet, with six ounces of brandy per diem, and the following mixture was ordered to be taken three times a day: chlorate of potash, ten grains; dilute hydrochloric acid, ten minims; syrup of oranges, ten minims; infusion of chiretta, one ounce. Also twenty grains of chloral at bedtime. The bowels were relieved by enema. A vaginal injection, containing Condy's fluid, ordered twice daily.

From the time of her admission the disease made rapid progress. She grew weaker daily, and became more and more emaciated. The discharge increased in quantity, and continued horribly fetid. On the 4th February the temperature rose to 102° F., and she seemed in every way worse. An erythematous patch appeared on the left thigh, over Scarpa's triangle, which soon became the seat of a tense, painful swelling, in which fluctuation could be distinctly detected. On the 8th February this swelling was freely opened by Mr. Cross, house-surgeon, and a large quantity of foul pus discharged. Slight temporary relief followed, but by the 13th she was again worse, and seemed to be sinking fast. Brandy increased first to eight ounces and then to ten ounces per diem. On the 15th she seemed quite collapsed; pulse 150, very weak; temperature 97°. Brandy raised to twelve ounces per diem. Evening temperature 105°. On February 16th she was weaker; temperature still 105°. She sank and died at 1.30 A.M. on February 17th.

Autopsy, thirty-six hours after death.—Body extremely emaciated. Thorax: Adhesions in left pleura; lower lobe of left lung hepatised, the consolidated portion studded with numerous grey, softened points (metastatic infarctions) from which a purulent matter was obtained by scraping the surface. Heart healthy.—Abdomen: Liver large, smooth, and fatty; kidneys pale and fatty; uterus healthy, and well involuted. On making an incision through the skin covering the symphysis pubis, the following conditions were found:—The ossa pubis were separated at the symphysis to the extent of half an inch, the inter-articular cartilage and subpubic ligaments having almost entirely disappeared. The bones themselves had undergone extensive caries; they were black in color, rough on the surface, and soaked in fetid pus. From the symphysis pubis descended a large sac, partly filled with foul, flaky matter. Its cavity extended in two directions, first, downwards under the mons veneris and labia to a point situated upon the anterior vaginal wall, just within the orifice, where it communicated with the vagina by a small aperture about three lines in diameter; secondly, outwards and downwards to the left groin, where a large, irregular cavity occupied Scarpa's triangle, opening externally by the incision made on February 8th. There were numerous burrowing sinuses around and between the muscles in all directions. The left acetabulum and head of the femur showed signs of commencing inflammatory mischief.

It will be seen that there was free exit for the pus from the diseased bone by two apertures—through the vagina, and through the artificial opening in the thigh.

The specimen is preserved in the King's College Museum.

The case seems to me full of interest; the diagnosis of this disease must always, I think, be difficult and doubtful, the prognosis unfavorable, and all treatment unavailing. I should be very glad of any suggestions as to its probable cause and pathology.

King's College Hospital, March 9th, 1872.

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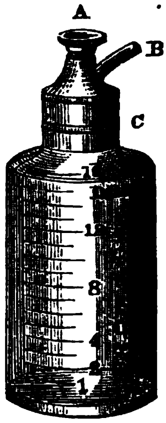
A NEW FORM OF DROP-BOTTLE FOR CHLOROFORM, ETC.

By J. Davies Thomas, M.D. Lond., F.R.C.S.,

Resident Medical Officer at University College Hospital.

One of the inconveniences experienced in the administration of chloroform on lint, &c., is the difficulty of supplying speedily the lint with fresh chloroform from time to time. When an ordinary stoppered bottle is used, a considerable time is occupied in removing and replacing the stopper; and this generally requires the use of two hands. Moreover, the quantity poured out is extremely uncertain. In order to lessen these inconveniences, Messrs. Mayer and Meltzer, of Great Portland-street, have constructed for me the bottle represented in the accompanying woodcut. It is a two-ounce graduated bottle, surmounted by a metal cap (A B C), the upper portion of which can be unscrewed from the lower, in order to fill the bottle with the anæsthetic. The upper part contains in its interior the stopper, which is ordinarily kept tight by a spring, but by pressing with the finger on the disc A, and inclining the bottle with the spout (B) downwards, the chloroform will escape in a uniform stream. By withdrawing the finger from the disc, the flow is at once arrested. As the stream is uniform in size, a short experience will enable the administrator to regulate tolerably exactly the amount of chloroform poured out; and as there is no risk of spilling the chloroform over



the patient's face, it is practicable to continue the supply on the lint without removing it from the mouth.

February, 1872.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

> Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

HOSPITAL FOR WOMEN.

CASES OF OVARIOTOMY.

THE following is a brief record of all the cases of ovariotomy (ten in number) performed at this hospital during the year 1871, for the notes of which we are indebted to Mr. Walter Rigden, the house-physician.

To prevent needless repetition, we may as well state here that in all the cases the pedicle was tied and dropped in. A double ligature was passed through the centre of the pedicle, and tied on each side; and then one of the ligatures encircled the whole pedicle, and was tied again. No special care is taken to prevent access of air to the abdominal cavity, which is always thoroughly cleansed out with sponges on holders wrung out in warm water, before the sutures are inserted. When the wound has been brought together a broad piece of strapping about three inches wide is placed across the abdomen tightly from hip to hip, and a large linseed-meal poultice with a drachm of laudanum is applied, and is changed every four or six hours, and continued for six or seven days.

All the cases for the first week, or longer if the ward is not required, are kept in a separate room, after which they are removed on an easy running sofa to a general ward. Each case is treated essentially on its own merits; stimulants being given according to the pulse. Case 10 had not more than one tablespoonful of brandy during the first week; particular care was taken to give what food each patient was able to take in small quantity, and to allay pain and vomiting as soon as they arose. It may be as well to remark that all cases which presented themselves, which were going on from bad to worse, had the option offered them of a chance of cure by operation.

Of the ten cases, seven recovered (one dying two months after at home), and three died in the hospital.

CASE 1. *Multilocular cyst; history of three months' duration; cyst slightly adherent; perfect recovery.*—S. D——, admitted under Dr. Sanderson, January 7th, 1871, aged twenty-six; married three years and a half, one child two years and a half ago; catamenia regular, profuse, and painful. She stated she had never been strong since her confinement, which was attended by flooding; she kept her bed for two months, with tenderness in abdomen and bad breast. Four months afterwards she fell ill again, with sickness. The abdomen enlarged. She got better in a week or two, and went to her work for five months, when she fell ill again for two months, then got better, and continued moderately well till the present illness. Three months ago she noticed she was getting larger, and fancied she felt a lump in the left iliac region; she had constant pricking pains all up that side. The tumour occupies the whole abdomen; it is dull on percussion, and resonant in the flanks; deep fluctuation through; girth at umbilicus, 41¼ in.; uterus high up in the pelvis, and central; the measurement is 8¼ in. Urine healthy.

Operation by Dr. Edis, Jan. 28th.—Slight adhesions to abdominal wall, easily broken down. The cyst was removed, the pedicle tied and dropped in, and the operation completed in the usual way. The cyst which came from the left side weighed 4¼ lb. It consisted of one large cyst and several small ones growing from its interior, one of which fell out on to the floor during the operation. The fluid measured fifteen pints.

Albumen appeared in the urine for the first twenty-four hours after the operation. The sutures were removed on the sixth day. On the seventh there was some tenderness and resistance in the left iliac fossa. The uterus, apparently fixed by deposit in left cul-de-sac, was very tender to the touch.

She slowly improved, and was discharged March 11th. Uterus mobile and central. Nothing detected in either cul-de-sac.

October, 1871 (seen and examined by Dr. Edis). —Uterus central and mobile. No thickening in ligaments detected. General health good. Is in service.

CASE 2. *One large cyst; two or three smaller ones; history of eight months; perfect recovery.*—S. M——, aged thirty-two, married six years, three children, last nine months ago, admitted under the care of Dr. Meadows, March 4th, 1871, the catamenia being present at the time. Menstruation was regular till last pregnancy, but has been irregular since. Four weeks after her confinement the patient woke up in the middle of the night with great pain across the lower part of the abdomen, not more on one side than the other, and thought it was the spasms. She got better in a fortnight, and in another fortnight noticed the right side of the abdomen larger than the left; no pain or uneasiness. For three or four weeks she suffered nothing from it, but was then taken with severe pain in the swelling, like knives cutting. These pains lasted off and on for two months, and weakened her so much that she could scarcely stand. She has been feeling better since. Her side has been getting much larger.

She came as an out-patient under Dr. Squarey in August, 1870, with a tumour in right iliac fossa, extending an inch beyond the umbilicus; it seems elastic and rather tender; indistinct fluctuation and dulness on percussion; above the pubes, on the left side, a softer and more elastic mass is felt, not distinctly connected with that before mentioned. On vaginal examination a tumour is felt in front, and on each side of the uterus, but giving the impression of not being connected with the tumour felt in abdomen.

On admission the girth at the umbilicus was nearly 32 inches; the tumour reached 1½ inch above umbilicus, freely movable. Uterus high up in pelvis; no tumour felt per vaginum; urine normal.

Operation, March 18th.—The tumour, which sprang from left ovary, was found free from adhesions; four pints of viscid, slightly milky fluid were removed through the trocar; the pedicle was tied as in other cases, and the tumour removed. A closely compacted cystic tumour, about the size of a large pear, found in connexion with each ovary, was removed, and some little cysts in the left broad ligament were opened, and the contents evacuated. The wound was closed, and the abdomen covered with linseed and laudanum poultices, which were changed every four hours.

The patient recovered without a bad symptom. A trace of albumen appeared on the third day. Sanguineous discharge came on the fourth day, and lasted in small quantities four days. The sutures were removed on the sixth day. The wound was perfectly united, but there was some little suppuration afterwards from one of the upper sutures.

A fortnight after the operation the uterus was found central, fixed by deposit around, but especially in the posterior half of the pelvis, quite elastic, and somewhat bulging. Six days afterwards the lump was only felt as a hard ridge behind the uterus.

The patient was discharged thirty-two days after operation. She came as out-patient August 1st, when she stated that she continued to menstruate quite regularly, and has sexual desire as much as ever.

Case 3. *Multilocular cyst; history of three months; recovery.*—C. T——, aged thirty, single, admitted under Mr. Heath March 22nd, 1871. The catamenia began at seventeen years of age; very profuse; lasting a week; the last interval being only a fortnight; no pain. Eight weeks ago her attention was attracted to the abdomen by feeling pain in the left side, when she found it very hard and resistant and larger than usual. Dr. Robson, of Iver, who has attended her, said that it had increased two inches in three weeks. She thinks she has noticed her abdomen large for three months. Appetite good. Nearly the whole of her abdomen is occupied by a tumour rather more to the left than the right side; dull on percussion; felt rising out of the pelvis on the right side; moderately smooth on the surface; harder and nodulated in the left hypochondrium; the whole right side of the tumour is less hard and more elastic than the right. No distinct fluctuation; no enlargement of superficial veins. Girth at umbilicus, 32 in.

Vaginal Examination.—The cervix felt of normal length, rather soft; directed downwards, backwards, and to the left. Sound three and a half inches upwards and slightly to the right, quite in front of the tumour, which is felt posteriorly and very close to the uterus. The tumour in the pelvis is the same as that in the abdomen. Urine normal.

A consultation was held, and the almost universal opinion was that the case was more likely to be a uterine than an ovarian tumour; but an exploratory incision was recommended, as patient was getting unfit to do any work.

Operation, March 25th.—The tumour was found to be an extensively multilocular cyst of the right ovary, with a very broad pedicle and no adhesions. The uterus felt healthy, not particularly enlarged. The fluid removed, which was small in quantity, was very viscid, and dark-brown in color. The cyst, which was very solid, weighed 7 lb.

The urine for the first twenty-four hours contained a trace of albumen. The sutures were removed on the fifth day. The wound was quite united, but gaped a little five days afterwards, and did not perfectly heal for another month, when she was discharged (May 11th).

Aug. 23rd.—Well and strong; cicatrix sound; pain in right iliac region after exertion.

Case 4. *Multilocular cyst of five years' duration; slight adhesions; death.*—H. E——, admitted under Dr. Protheroe Smith, March 24th, 1871, aged fifty-nine; single. Catamenia ceased ten years before. About five years ago she had retention of urine, and had her water drawn off for four days, when she got quite well. In another three months she was troubled in the same way again, and soon after this noticed a hardness at the bottom of the stomach, not more on one side than the other. She consulted a doctor in Hastings four years and a half ago, who said it was an ovarian tumour about the size of a cocoa-nut. It has been gradually growing since, but has not given rise to much inconvenience till lately, when she has had a good deal of aching pain in the right side, at times vere severe. The girth at the umbilicus is now 31½ inches. The tumour occupies the whole abdomen, fluctuates freely, solid matter being felt along the right side. The general health is good. Urine healthy.

Operation, April 1st, 1871.—Nothing particular about the operation. The pedicle was tied in the usual way, as was also a band of adhesions connecting the cyst with the intestines on the left side. The fluid removed measured eight pints, thick, dirty yellowish in color, with masses of white doughy material floating in it. The tumour weighed 2½ lb.; it was a multilocular cyst of right ovary, with three hard nodules, varying in diameter from 1 to 2½ inches, not umbilicated, very hard on section. Under the microscope, scrapings composed of cells of various sizes and shapes, with a distinct nucleus, and full of granular matter. Some of the cells were caudate. The sloughy material free in the fluid contained similar though larger cells, and a quantity of cholesterine crystals on the surface.

The patient progressed favorably, sickness diminishing, until the evening of the third day, when the abdomen began to get distended, but not tender. This continued and increased considerably in the evening of the fourth day, when the pulse began to rise, and continued to rise to 140. She died at 9.30 A.M. on the morning of the sixth day.

At the post-mortem examination signs of general peritonitis were found.

Case 5. *Multilocular cyst; slightly adherent; recovery; history of five years' duration, with two intercurrent pregnancies.*—S. A. C——, admitted under the care of Mr. Scott, March 18th, 1871; aged thirty-five years; married ten years; has had three children and four abortions; the last pregnancy, which went to full time, two years and a half ago. Five years ago she felt pain in the right lumbar region; and on putting her hand there noticed a lump of the size of a marble, hard and not tender to the touch. It increased to the size of an egg in a week. The same week she became pregnant; and during the whole nine months she had very severe pains in both groins, but not in the right flank. The labour was long and severe. The patient did well for ten days, when she was seized with "inflammation," and kept her bed for seven weeks. Never noticed any pus with her motions or per vaginam. Directly after labour she noticed that she was quite as large as she was before, and never got any smaller. She continued to feel ill, but without feeling anything definitely the matter, and in a year's time became pregnant again. When she had gone seven weeks, "inflammation" came on again; pain and tenderness over the lower part of the abdomen. This lasted for seven weeks; but she was never able to walk about during the whole of her pregnancy. The child was born alive; labour very quick and severe. During this pregnancy the patient noticed that the uterus with the fœtus lay all on the right side, and the tumour was on the left, and there was a deep depression between the two at their upper parts. She did well for ten days after labour, when she got another attack of "inflammation" from wearing damp clothes, and kept her bed for sixteen weeks. She then got to feel fairly well till last Christmas, when violent pains in the side and back came on, without fever, and induced her to apply to the hospital.

On admission the abdomen was found to be occupied by a large tumour, reaching to within an inch of the ensiform cartilage; filling up more the right flank than the left; dull on percussion; elastic; non-fluctuating. Some hard masses were felt deep down in the right flank; the tumour free from tenderness; irregular in outline. The skin moved freely over the tumour, which seemed more firmly connected to deeper structures. The girth at umbilicus was 33⅛ inches. The uterus lay in the hollow of the sacrum. The roof of the vagina was occupied by the lower part of a tumour (?) from left ovary. Urine normal.

Operation, May 18th.—The tumour was found to be a multilocular cyst of right ovary, slightly adherent to abdominal walls; free elsewhere, and weighing 4¾ lb. On removing it Mr. Scott found a hard solid tumour connected with the left ovary, weighing 1¼ lb., which he also removed. The pedicle of the multilocular cyst of the right ovary was long and twisted. Both pedicles were tied and dropped in, the after treatment being the same as in the previous cases. The patient made a good recovery. Sanguineous vaginal discharge, which came on two days after the operation, lasted twelve days. She left the hospital 34 days after operation.

Seen August 9th.—Catamenia has not appeared since leaving the hospital. The tumour of the right ovary was multilocular; the tumour of the left was of a solid fibrous nature, with a portion of ovary at one end enclosed in the same capsule. Under the microscope this portion at the end presented the appearance of healthy ovarian stroma; then there was the intermediary tissue with the appearance of true ovarian stroma, and of fibrous tissue, the bulk of the tumour being purely fibrous.

Mr. Scott exhibited this tumour at the Obstetrical Society.

CASE 6. *Multilocular cyst; probable history of nine years; old and firm adhesions; recovery from operation; death three months afterwards from exhaustion.*—E. C——, admitted under the care of Dr. Meadows, May 5th, 1871; aged forty-six; married seventeen years; has had five children and two miscarriages. Last pregnancy nine years ago. The abdomen has been larger than formerly since last confinement, but the patient took no notice of it until four years ago, when it began greatly to enlarge, as much on one side as the other, with dragging pain about the navel.

May 9th.—Abdomen enormously extended; girth 48¼ in. Veins much enlarged; skin shiny. Dulness all over swelling, except in front; resonance in epigastric region and in both flanks, but more especially in the left; fluctuation very distinct; no solid matter discoverable. Uterus procident; vagina inverted; urine normal. The abdomen was tapped, and 14¾ pints of slightly turbid straw-colored fluid withdrawn, highly albuminous, and containing pus-corpuscles. After the fluid was withdrawn, several solid masses were felt in the abdomen, and the uterus returned to its proper place in the pelvis.

Ovariotomy was performed on May 20th. Extensive old and firm adhesions to the parietes were found; the omentum was much thickened and its vessels greatly enlarged; it was adherent all over the anterior aspect of the tumour. These adhesions were broken down with great difficulty, the tumour itself was broken up, and with some difficulty removed; the pedicle, which came from the left side, was transfixed, tied, and returned. Very free bleeding took place from numerous points, both on the omentum and on the parietes; at least a dozen ligatures were employed, the wound brought together, and linseed and laudanum poultices applied. The tumour was an ordinary multilocular cyst, and weighed 5 lb.

The patient recovered well from the operation, and, though requiring much brandy, had no special bad symptom. The sutures were removed on the seventh day; wound united; suppuration from the line of sutures. This suppuration continued. On June 2nd the pus was thin, and had begun to burrow; subsequently the sinus had to be laid open. On July 4th, temperature was normal; pulse 100; tongue clean; appetite fair; patient walks about the ward; abdominal wall granulating up healthily. She was sent to Eastbourne next day, and remained there for a fortnight; came home and was better for a time, but finally sank and died at her own home on August 28th.

Autopsy fifty-two hours after death.—Body extremely emaciated; wound perfectly united quite through to peritoneum, except at upper part externally, where a probe could be passed among the muscles, three-quarters of an inch to right side, but did not perforate the peritoneum. Intestines matter together, very firmly in places, slightly adherent to abdominal parietes. Adhesions also to omentum very firm. Intestines of an ashy grey color. Pelvic viscera matted together, especially in both ovarian regions—inextricably so; right ovary found slightly enlarged, dilated into a cyst containing purulent fluid. To the left of the uterus, Fallopian tube firmly embedded in cellular tissue around, forming a hard mass. Two ligatures were found loose in right ovarian region, and one in omentum. The ligature of the pedicle could not be found, and was probably quite embedded. No fluid in the abdomen; a little purulent fluid in cavity of pelvis.

CASE 7. *Multilocular cyst; adhesions to omentum; recovery.*—E. H——, admitted under the care of Mr. Scott July 3rd, 1871, aged forty-six. Had been married twenty-eight years, and had two children, the last nineteen years ago; no miscarriages. Menstruation was regular till three years ago. Since then it had been irregular, and often absent from three to nine months. She had a slight show for two days a fortnight before admission. First noticed a lump low down in the left side of the abdomen three years ago, which was about the size of a walnut, very hard, tender, and had much pain in it. It gradually increased in size a year ago, since which time it has enlarged very rapidly. Now the whole abdomen is occupied by a large tense tumour, with resonance in both flanks and above; fluctuation free; smooth on the surface, except for an irregularity on the left side at the upper part where the abdomen is most prominent; parietes move freely over it; girth at umbilicus 40½ inches; uterus high up, close to pubes; sound enters three inches, slightly to the left. The patient has been losing much flesh lately. Eleven days before the operation the patient had a catamenia lasting for six days.

Operation, July 22nd.—Mr. Scott found a large piece of omentum in front of the tumour, then an empty portion of the cyst, which was multilocular, overlapping the main tumour, and at first causing difficulty on account of its similarity to intestine; adhesions to omentum and fundus uteri. Three bleeding points in the omentum were tied, and part removed with cautery and clamp. The tumour, which came from the right side, weighed 1 lb. 4 oz. The operation was completed in the same way as the others. The patient made a slow recovery, the temperature and pulse continuing high for an unusually long period. Some of the stitches were removed on the sixth day, and some on the seventh day.

August 4th.—A doughy mass, the size of a goose's egg, was felt to the right of the wound, and tender to the touch. On pressure no pus was squeezed out of the wound; the abdomen resonant all over.

August 10th.—Swelling was tapped with a fine trocar. About half a drachm of pus, with a good deal of very offensive gas, escaped, and the swelling was gone. From this time she gradually improved, and left the hospital Aug. 28th.

Oct. 25th.—Doing well.

Case 8. *Multilocular cyst of four years' duration, adherent to omentum and transverse colon; death after six weeks.*—E. N——, aged thirty-three, came as an out patient to Mr. Scott, Aug. 9th, 1871. Married sixteen years; no children. One doubtful abortion four year before. Catamenia regular, lasting one week; painful and profuse. She complained of swelling of the abdomen, not more on one side than on the other, commencing four year ago. Abdomen enlarged; surface smooth, except for two projections, about the size of the palm of the hand in the right and left hypochondriac regions; dull on percussion, except in the flanks; free fluctuation across, except over the projections. Girth at umbilicus 27½ in. Uterus normal, rather posteriorly situated; cervix looks backwards. General fulness of both cul-de-sacs; but nothing distinctly made out.

Though frequently persuaded, she refused to come in and submit to an operation until Oct. 9th, when the abdomen had increased to the size of 40½ in. She had lost flesh a great deal, and had had frequent attacks of diarrhœa. Appetite good; urine healthy.

Operation, Oct. 21st.—Adhesions were found to the abdominal walls in front and at the sides, which were easily broken down. Extensive adhesions were found also to the transverse colon and omentum, which took considerable time to separate. Three bleeding points were ligatured in the omentum; and a part, being much inclined to bleed, was tied and removed. The right ureter came into view on removing the cyst. The pedicle was short and thick, and came from the left side; it was tied, and the wound brought together by silk sutures. The fluid drawn off during the operation measured seventeen pints. The tumour weighed 4¼ lb., was multilocular, with three solid masses, which on section presented a honeycomb appearance. The patient continued very weak, presenting often an anxious expression. The sutures were removed on the sixth day, the wound being entirely united. The range of pulse had been from 136 to 110; respiration from 32 to 24; temperature from 102° to 98·8°.

The wound began to gape and look sloughy. On the 6th of November there was an aphthous condition of the mouth and tongue; wound cleaning. On the 7th a bed-sore formed over the sacrum. On the 10th the long slough was removed from the bottom of the wound with a ligature round it (portion of ligature in omentum ?). On the 13th there was slight erysipelas of the face. A piece of slough came away on the 15th, a ligature on the 17th, and another piece of slough on the 23rd. By the 28th the abdominal wound was closing, and the bed-sore increasing in size. She died on Dec. 4th, six weeks after the operation.

At the post-mortem examination the abdominal wound was found entirely united, excepting a small hole no larger than a pin's head at the lower angle. There were strong adhesions between the intestines and the contiguous parts. The abdominal cavity contained a little thin purulent fluid. There was no localised abscess. The pedicle was found to have been divided very close to the uterus, and the ligature was quite embedded.

This patient exhibited no rallying power after the first week, then got into a hectic condition, and sank.

Case 9. *Both ovaries diseased; extensive adhesions; death in twenty-four hours from peritonitis.*—E. B——, aged forty-seven, admitted October 4th, 1871, under the care of Dr. Protheroe Smith. Has been married nineteen years, and has two children, the youngest fifteen years of age. Catamenia regular, lasting from three to five days, and occasionally profuse. Was in good health till two years ago, when she was seized with a pain in the right iliac region, and, soon after experiencing the pain, noticed a lump the size of a walnut, hard and tender to the touch, and moving away on pressure. The lump has been increasing upwards since. The pain has been almost continuous, with occasional paroxysms of increased severity. The swelling in the abdomen has become more marked during the last two months, and in the latter three weeks has been getting much worse. Girth at umbilicus 45½ in. Œdema of feet and ankles.

Paracentesis abdominis was performed on Oct. 5th, and 496 oz. of greenish-yellow albuminous fluid removed.

Oct. 10th.—Abdomen partly refilled with fluid. A hard mass can be felt rising out of the pelvis on the left side, and reaching up to the level of the iliac crest; this can be felt reaching across the middle line of the abdomen to the opposite side. A similar mass can also be felt reaching up to within two fingers' breadth of the right false ribs. The masses are freely movable in the cavity of the peritoneum. Uterus central, 8½ in. long. The movement of the tumour in the abdomen communicates movement to the sound in the uterus.

21st.—Abdomen again tapped, and 192 oz. of fluid withdrawn.

28th.—Operation performed by Dr. Edis. The tumour was found adherent to the left side. The pedicle was very extensive, and was transfixed and tied in three separate portions. The tumour was closely adherent to the fundus uteri. The adhesion was easily broken down, the main ligature being within 1½ in. of the fundus uteri. Some bleeding vessels on the left side were secured with silk ligatures. The actual cautery was applied to some oozing vessels, and subsequently a strong solution of perchloride of iron. There was then found a tumour connected with the right ovary, the size of the fœtal head at term, perfectly free in the abdomen. This was also removed, and the operation completed.

The patient rallied well from the operation, but suffered much from pain, which was only partially relieved by hypodermic injections of morphia. At 6 A.M. on the day after operation she was seized with violent pains round the waist. The pulse went rapidly up to 160 and 180 at 10.55 A.M. (about twenty-four hours after the operation), when she died.

Post-mortem.—Evidence of general acute peritonitis. The tumour of the left ovary was composed almost entirely of botryoidal masses, hard, of various sizes and shapes, and very few cysts. That of the right side was one cyst about the size of a fœtal head at term, with hard masses on its summit, as in the other tumour. The two tumours together weighed 7¼ lb.

CASE 10. *Multilocular cyst; no adhesions; recovery.*—E. B——, aged nineteen, single, admitted under the care of Mr. Heath, October 31st, 1871. Catamenia regular and profuse; due in a fortnight. Has noticed the stomach to be larger than it should be for two or three years, not more on one side than the other. She has no pain; general health good enough except that she has felt fatigued more than formerly. Abdomen occupied by a large tumour slightly more prominent to the right of the umbilicus. Right flank resonant; left dull and smooth on the surface, except about one inch below the left false ribs, where solid matter can be detected; fluctuation through. Girth at umbilicus 37¾ inches.

Operation, Nov. 4th.—The tumour found to be a multilocular cyst of left ovary, with a long, broad pedicle, and free from adhesions, was removed. On examining the other ovary it was found to be at least double its size, elongated, deep-blue in color, tense, and glistening at the outer end as if about to burst. It was not interfered with. The operation was completed in the usual way, and the patient put to bed. The tumour consisted of one large cyst and four smaller ones aggregated together at the upper and left part. It weighed 3½ pounds. The fluid measured 11 pints, was clear, yellow, and of a syrupy consistence. Menstruation came on the third morning after the operation, and lasted, in normal quantity, three days and a half. Patient did exceedingly well till the end of the third week, when she caught cold and got an attack of bronchitis, which put her back a little. She left the hospital, cured, December 16th.

UNIVERSITY COLLEGE HOSPITAL.

REMOVAL OF LARGE FIBRO-CYSTIC TUMOUR OF LOWER JAW; RECOVERY.

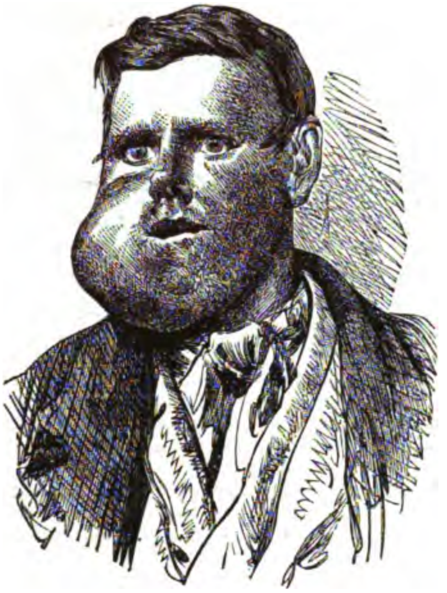
(Under the care of Mr. CHRISTOPHER HEATH.)

FOR the notes of this case we are indebted to Mr. S. Coupland, ward clerk.

The patient is a native of Cumberland, and has been a carter ever since he was eight years old. His parents are living and healthy. When a child he suffered from suppurating glands in the neck and submaxillary region, which were opened; otherwise he has always enjoyed good health. Never had syphilis. Has lived well; drinks beer freely; is often drunk (i. e., on an average once a week). Has followed his occupation up to the time of admission. About six years ago he first noticed a small, hard swelling, about the size of a marble, situated in the right cheek, attached to the gum and lower jaw, but movable under his fingers. From the first it had an aching pain, dull, and constant. The tumour continued to increase in size, and four years ago it was lanced in the gum; it then began to discharge, and has continued to do so slightly ever since. He has noticed that the discharge is more abundant after drinking much. It has been lanced twice since, and within the last six months it has been twice tapped by Mr. Watson, of Lancaster: the first tapping just before Christmas; the second, five weeks before admission. Mr. Watson says that at each tapping about four ounces of fluid were drawn off. From the displacement inwards of the two anterior molars and the second bicuspid tooth by the increased growth of the mass, these teeth were extracted two years ago. Since Christmas the tumour has grown with increased rapidity, and has become more tender. During this time the patient says he has lost a stone and a half in weight, and that his appetite has diminished. The patient cannot assign any cause for the origin of the growth. He has had no bad teeth in that jaw. Remembers having had a blow on the jaw with a pitchfork handle before the tumour appeared.

July 10th.—The patient is fairly built, florid, and sun-burnt. He does not look ill. Tongue clean appetite better than it has been; pulse 76, full and bounding.

The right side of the face presents a large, smooth, globular swelling, which occupies the whole side. It extends in front to the angle of the mouth; behind, to a distance of about an inch behind the lobule of the ear, measuring in this diameter 7½ in. Above, it extends from the tragus of the ear along the lower margin of the orbit to the side of the nose; below, on a level with the hyoid bone. It measures from above, down over the greatest prominence, 8½ in. The circumference of the mass measures 18 in. The right angle of the mouth is drawn slightly upwards and outwards; but the contour of the lower lip is unaffected, and the contours of the chin quite preserved. The upper part of the mass is more vascular in appearance than the lower. On the under surface are some cicatrices from the abscesses which were opened when he was a child. The rest of the surface of the tumour is quite smooth, not ulcerated. Temperature of the cheek, 99·7°. The tumour is more tender posteriorly than elsewhere. Its lower two-thirds feel hard and resisting, the skin being quite movable over the mass. The posterior portion of the tumour is also solid, as well as a portion which extends in front of the ear for about an inch. The rest of the mass is soft and fluctuating, evidently containing fluid, and the upper margin of the solid portion can be distinctly felt across the tumour. Inside the mouth the alveolar border of the right side of the lower jaw is much widened, extending inwards, so as to diminish the cavity of the mouth behind the first bicuspid. The second bicuspid and first and second molar teeth are wanting. The patient says the third molar is present; but it is not visible, nor can it be felt. On the widened alveolar border is an ulcerated surface, covered with a layer of thin purulent fluid, which is continually oozing. The tumour evidently arises from this part of the lower jaw; for, anteriorly, a thin shell of bone can be felt continuous with the jaw and with the surface of the tumour. The upper jaw does not seem to be implicated in the growth. The teeth in it are all present; but the alveolar border has been displaced inwards from the growth of the tumour, so that the roof of the mouth appears contracted.

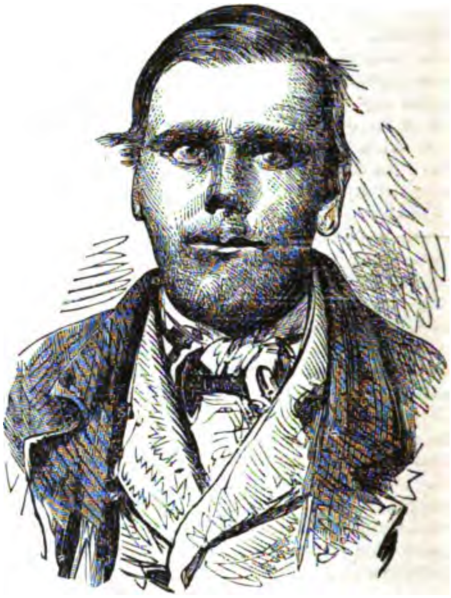


Before the operation. (From a photograph.)

July 11th.—Patient feels rather sick this morning. Temperature 98·4°; pulse regular, 72.

12th.—10 A.M.: Temperature 99°; pulse 80. Urine, 82 oz.; specific gravity 1013; acid; no albumen.

To-day (12th), at 2 P.M., chloroform having been administered, Mr. Heath proceeded to remove the tumour. He first extracted the right canine and second incisor teeth of the lower jaw, a piece of the jaw coming away with the teeth. He then made a vertical incision to the right of the symphysis through the lip down to the base of the jaw; from the lower end of this incision he cut upwards and backwards over the tumour towards the ear as far as one inch above the angle of the jaw; in making this cut he divided the facial artery. Ligatures were applied before the incision was completed. The whole length of the incision was about nine inches. The superficial tissues were then dissected off the tumour, the large upper flap being first raised; and the tumour was carefully shelled out. In dissecting up this flap the facial artery was again cut and ligatured. The cyst at the upper part of the tumour being now fully exposed, it was laid open by a free incision extending right across it, and about ten ounces of fluid escaped. Mr. Heath then continued to separate the mass from the skin at the lower part, and, having cleared it as far as the anterior incision, he sawed through the jaw where the teeth had been extracted. He then cut through the mucous membrane and muscles attached to the jaw, and here again some vessels had to be secured. Having completely separated the mass, he attempted to forcibly depress the jaw so as to disarticulate it; but, the coronoid process becoming caught against the malar bone, he had to detach the process by the bone forceps. On depressing the jaw, he found that a small portion of the condyle was free from the growth. As he was proceeding to disarticulate, the remains of the lower jaw gave way just below the condyle, the tumour shelling out from the expanded bone round it. The posterior part of the jaw was left nearly down to the angle; a small piece of this was afterwards cut off with the bone forceps. About four ligatures were applied to bleeding vessels, and the rest of the hæmorrhage was arrested by the actual cautery. The wound was then thoroughly sponged out and sewn up; for the incision through the lip hare-lip sutures were employed, and a very fine suture for the mucous membrane of the lip, the rest of the incision being closed by silver wire sutures; the whole of the wound was then painted with collodion. There was not very much blood lost during the operation. The patient was not thoroughly under the influence of chloroform the greater part of the time.



After recovery. (From a photograph.)

After removal the tumour was almost globular in form, it measured 3½ in. in diameter at its widest point. It was slightly lobular on the surface. It weighed 18½ oz. Over the whole of the lower part was a thin shell of expanded bone; for the greater part this was over a solid growth, but at the upper and outer aspect was a large cyst capable of containing about 6 oz. of fluid. The lower part of the wall of the cyst was bony, but the whole of the upper part was free from bone. The whole of the inner wall of the cyst was formed of a thin layer of bone. Just anterior to this large cyst was a smaller one containing about ½ oz. of thick fluid, in which was a large quantity of cholesterine. Its walls were bony everywhere. Both cysts were lined by a smooth thin membrane. On the inner side of the tumour were two openings about ¼ in. in diameter, which had opened into the mouth. They communicated with a large cavity in the centre of the tumour, into which the finger could be pushed as far as the second joint. On making a section right through the mass, this central cavity was found to be about 2 in. in length. The inner surface was very irregularly lobulated. The lobules varied in size from a pea to a filbert. They were covered by a smooth membrane. The tumour was moderately firm, of a whitish color, and small points of bone were scattered through it. On scraping, it yielded a whitish fluid mixed with fragments of the substance of the tumour. Under the microscope this was found to consist of a few spindle cells and a vast number of free oval nuclei, containing one, two, or three shining nucleoli. Some of the nuclei were perfectly circular. The average diameter was about 1-1500 inch.

On examining sections made from one of the lobules from the central cavity of the tumour, it was found to consist chiefly of a dense fibrous tissue, amongst which were oval and irregularly shaped spaces, having an appearance much resembling acini and ducts of glands. They were completely filled with oval nuclei, each containing one or more bright shining nucleoli. They were arranged along the walls of the spaces so as to look like epithelium, but they had not the distinct cell and nucleus characteristic of epithelium. The relative proportion of the spaces and fibrous tissue varied greatly. At some parts it was almost firmly fibrous, and at others the spaces formed the greater part of the growth. The patient made a quick and uninterrupted recovery.

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ROYAL NAVAL SICK QUARTERS, HALIFAX, NOVA SCOTIA.

CASES OF SUBACUTE INTERNAL OTITIS.

(Under the care of Staff Surgeon GORDON, R.N.)

THE three following cases of considerable interest have been communicated to us by Assistant-Surgeon Griffiths, R.N.

CASE 1. *Subacute internal otitis of left ear, followed by hemiplegia, including the facial and fourth nerve on the affected side.*—Mr. W——, aged thirty-five, a gunner R.N., was admitted Aug. 10th, 1871, with a purulent discharge from his left ear. He also complained of partial loss of motor power in his right arm and right lower extremity, and acute pain in the left side of his head. He stated that he had had a thin watery discharge from the same ear since childhood, and that the above symptoms had gradually come on during the previous fortnight. On examination of the ear with the speculum, the lining membrane of the internal meatus was found to be thickened, and the membrana tympani presented a dull and opaque appearance, with a reddened circumference. Sensation was perfect in both the right upper and lower extremities; but the motor power was impaired, and when walking he dragged the heel. The grasp of the right hand was feeble. The constitutional symptoms were slight; pulse of average quality; tongue clean; bowels irregular. He was ordered a pill of mercury and colocynth, and a compound senna draught; a blister to the nape of the neck.

Aug. 11th.—Pain in the head relieved; pupils natural; bowels not yet opened. Pill and draught to be repeated; and the ear to be syringed with a weak carbolic-acid lotion. As the bowels had not been opened at 5 P.M., he was ordered five grains of calomel at once.

12th.—Acute pain in the head at the vertex; unable to raise the right arm or to move the fingers; bowels opened by the powder, but not freely. The powder to be repeated.

14th.—Slight improvement to-day; less pain in head; motor power returning to the lower extremity. To take five grains of blue-pill three times a day.

16th.—Pain chiefly in the temples; mouth drawn to the right side. Speech impaired; difficulty in pronouncing labials. A blister to be applied to each temple.

18th.—Feels much better, but no improvement in the arms; passed a good night. Blue pill to be discontinued as the gums are slightly touched by it. Ordered one-thirty-second of a grain of strychnine in a drachm of water three times a day.

From this date to September 1st he continued to improve, and gained some power in the arm, and was able to raise it. The following day he complained of double vision. On examination the pupil of the left eye was found elevated about a line above that of the other eye, and the eyeball rotated, but no external strabismus. The pupil was slightly dilated. Objects seen at close or far distances were seen naturally; but at a medium distance two images were formed, one raised a little above the other. The patient could obviate this defect by fixing the affected eye with one of his fingers. There was perfect vision when each eye separately viewed the object. After this date no improvement took place. He was invalided and discharged for passage to England.

CASE 2. *Subacute internal otitis, followed by transient hemiplegia, including the facial nerve on the affected side.*—John S——, aged thirty (rating, carpenter's crew), was admitted August 21st, 1871, for chronic inflammation of the external meatus of the left ear, with acute pain in the head on the left side, and giddiness. He stated that he had had discharges from both ears for some time. On examination with the speculum the right membrana tympani was found to be destroyed, and round its original attachments a ring of small red granulations was seen. The left membrana tympani was opaque and dull, and the external auditory canal reduced to half its calibre by thickening of the cutis. Poultices were applied to the left ear. On Aug. 23rd a small abscess formed in the left external meatus, which was incised. He was ordered a lotion containing carbolic acid, and a mixture of the citrate of quinine and iron. He went on well till Sept. 2nd, when he was seized with a severe pain in the left ear. Discharge still continued. He was ordered a pill containing two grains of calomel three times a day.

Sept. 8th.—Complains this morning of giddiness. Pupils dilated; pulse small and weak. To leave off pill.—5 P.M.: Giddiness continues; unable to walk steadily; partial loss of power in the right upper and lower extremities; mouth drawn to the right side: pupils still dilated, and not as sensitive to light as natural; ideas confused; watchfulness, and darting pain in the head. The hair was cut short, and a blister applied to the nape of the neck.

9th.—Passed a good night, and feels better. No pain in the head; motor power returning; pulse soft and weak. Ordered five grains of the citrate of quinine and iron three times a day.

From this date the patient gradually improved, and regained, under the use of tonics and stimulants, the use of the affected limbs. The discharge from the ear also daily lessened; and on Sept. 24th he was well enough to be discharged to light duty. He was, however, readmitted on the 27th, with a relapse of former symptoms. He recovered: but, being unfit for service, he was discharged for passage to England.

CASE 3. *Subacute internal otitis; left ear, followed by partial loss of motor power in both lower extremities; recovery.*—F. H——, aged twenty-eight, private Royal Marines, admitted on the 3rd August, 1871, for a discharge from the left ear. The previous evening, while at quarters on board H.M.S. *Royal Alfred*, he was seized suddenly with giddiness, and at once applied for treatment, and, as his bowels were confined, was given a powder containing three grains of calomel and one drachm of the compound jalap powder.

On admission into the sick quarters he complained of acute pain in the left ear, from which was a purulent discharge. He stated that he had had a discharge from both ears at intervals for a long time. On examination with the speculum the membrana tympani of the left ear presented a dull and thickened appearance, and round its margin a ring of small irritable-looking granulations. The right membrana tympani was in a similar condition. There was inability to stand for any length of time, and the patient, on endeavoring to walk, did so in a wild and irregular manner, as if the power of the will was partially lost over the movements of the lower extremities. The ears were syringed with warm water frequently.

Aug. 4th.—Headache and giddiness continue; shooting pains down both thighs; can move the legs better. Ordered a mixture containing two grains of sulphate of quinine three times a day.

10th.—Still a purulent discharge from the left ear. A blister to be applied behind the left ear.

13th.—Discharge increased; complains of giddiness and noises in the head. To leave off quinine mixture.

19th.—Feels better; less discharge.

22nd.—Improving, but still has giddiness. To take the thirty-second part of a grain of strychnine in water three times a day.

He went on well, and was discharged on Oct. 18th.

ST. GEORGE'S HOSPITAL.

CASE OF YELLOW ATROPHY OF THE LIVER.

(Under the care of Dr. WADHAM.)

A. G——, a Swede, twenty-eight years of age, was admitted October 14th with the following history:—He had always been healthy until the previous summer, during the whole of which he had suffered from pain in the epigastrium, with occasional vomiting after food. Three weeks ago his urine became very dark-colored, and the whole of his body gradually jaundiced. Since that time the pain in his epigastrium had increased in severity, and vomiting had occurred after every meal; he had slept much and heavily, and had suffered from great and constant depression of spirits.

On admission he was very jaundiced. He had tenderness, when examined, over the hepatic region, and the liver on percussion appeared to be smaller than usual. He had pain there, occurring in paroxysms; his pulse was 60 and full; the mucous coat of his tongue peeling off; his urine was loaded with the coloring-matter of the bile, and his motions were solid and clay-colored.

In this condition, without any vomiting, but with very constipated bowels and great disinclination for food, he remained until the 20th. He then appeared weaker, very confused in his mind, with a heavy look about the eyes, and vomited three times during the day. From this time until the 29th he was very restless, and frequently vomited a dark-yellow fluid, which stained everything upon which it fell a bright-yellow color. His bowels remained confined; but the motions when passed, and the urine, retained the same characters as on admission. On the 29th he was much lowered, seemed scarcely to understand what was said to him, was with difficulty kept in bed, and constantly vomited yellow fluid. From this time he lay in bed apparently quite insensible, and making a continued moaning noise. He vomited occasionally, and had frequent convulsive twitchings of the left side, which, when not convulsed, appeared as if paralysed. In this condition he remained until his death, which took place at 6 A.M. on the 2nd of November.

The post-mortem appearances, as described by Dr. Whipham, the curator, were as follows:—The dura mater was bile-stained, and there was excessively bile-stained fluid in the ventricles. The septum was softened, but the brain was otherwise healthy. The lungs were natural, with the exception of some vascularity of the bronchial mucous membrane, and a little bloody mucus in the bronchi. The endocardium was bile-stained, but the structure and valves of the heart were natural. The spleen weighed nine ounces, and the Malpighian bodies were large and prominent. The kidneys were flabby, partly decomposed, but their surfaces smooth. The gall-bladder contained a little thin, greenish mucus, and the ductus choledochus was open. The liver weighed thirty-two ounces, was flabby, but not especially soft or friable. On both upper and lower surfaces circumscribed patches, of yellow color and variable size, were observed, which were more numerous and more extensive on the right lobe. On making a section through the liver substance, these appearances were still more marked. Their color in the central parts of the organ was bright ochrey, which contrasted strongly with the unaffected portions of the hepatic tissue; moreover, the somewhat red and congested state of the unaffected parts rendered this contrast still more conspicuous. The internal parts of the left lobe were far less diseased than any other portion of the organ. The vessels and ducts were in all respects normal.

Microscopic examination was made of portions of both right and left lobes, after previous hardening in chromic acid, with the following results. The disease was considerably more advanced in the right lobe than in the left. The cells of the affected portions were in various stages of atrophy—i.e., at the outer margin of the yellow patches the liver cells, though retaining their normal size and shape, were filled with fat-globules, and, after twelve hours' immersion in a solution of carmine, were but slightly tinted. It should here be remarked that the cells and stroma, at a little distance from the margins of the atrophied patches, were to all appearance natural. Nearer to the centres of the yellow area great changes were observed; the hepatic cells had become shrivelled to about half their normal size, their outline was extremely irregular, and the carmine tint was very faint. They contained an abundance of oily matter. The central parts of the affected portions showed still further changes. The fatty cells had in great measure disappeared, and aggregations of brightly-glistening nuclei were found imbedded in a fibrous interlacing stroma, which, though delicate in some places, was for the most part dense and well-marked. In this matter liver-cells could be occasionally seen, either contracted and altered in shape, or of a size nearly natural, but in an advanced stage of fatty degeneration. These nuclei were found to be numerous, or otherwise in exact proportion as the hepatic cells had become atrophied and disappeared. It was further observed that here and there the nuclei were elongated, and exhibited a tendency to assume a linear arrangement; but this condition, though occasionally met with, was rare, and by no means a characteristic feature in the case; they were, for the most part, scattered without any definite arrangement in the fibrous matrix. Again, in other places, the hepatic cells had entirely disappeared; the nuclei were found few and far between, imbedded in an amorphous granular débris. The walls of the bloodvessels did not appear to have suffered in the atrophic process; if anything, the reverse obtained, and the fibrous parietes of the smaller arteries were thickened. The bile-ducts were, to all appearance, in a normal state. In no part of the section examined were crystals of leucine or tyrosine visible.

This case was diagnosed during life to be one of yellow atrophy of the liver, quite as much by a process of elimination as on account of any very marked peculiarities in the symptoms. With the exception of jaundice there were present none of the symptoms of gall-stones, and neither the man's age nor appearance, nor even the most careful examination, made it probable that there was any malignant disease or tumour pressing upon the gall-duct. It was therefore concluded that the jaundice was not caused by retention and reabsorption of bile, but was due to the suppression of that secretion. The man's previous history, the length of time during which the jaundice continued, the rapid and increasing prostration which accompanied it, and its resistance to remedies, clearly proved that, if owing to suppressed secretion, this suppression could not have been the result of mere innervation, or even of disordered hepatic secretion. There was no evidence of tubercle in any part of the body, and the apparent diminution in the size of the liver and the pain caused by examination pointed far more to atrophy than to either fatty or amyloid degeneration of this organ; and upon these grounds the diagnosis was formed. During the progress of the case several attempts were made, by means of the sugar and sulphuric acid test, to ascertain whether biliary acids were present in the urine. In this case, however, as in many others, the test which proves perfectly successful when used upon decolorised bile failed in the presence of the biliary coloring matter to afford any reliable evidence.

SAMARITAN HOSPITAL.

ULCER OF THE STOMACH; RUPTURE OF THE HEART.

(Under the care of Dr. WILTSHIRE.)

C. B——, a widow, aged fifty-seven, came to the hospital on Jan. 16th, 1872, complaining of a "bilious attack." She has pain between the shoulders, distress and tenderness at the epigastrium, and flatus and vomiting after food, which always excites pain; has occasionally vomited "black stuff," and has melæna; bowels open; urine high-colored, but not thick; pulse very feeble. A daughter who accompanied the patient said that a few nights before her mother had had a curious attack, which, from the description given, might have been of an epileptic character.

An ulcer of the stomach was diagnosed, and a mixture of bismuth, soda, hydrocyanic acid, and calumba was prescribed.

Three days afterwards the patient sent to say that she was better, but the day being wet she had not ventured out. The night following the patient felt so much better that she partook of meat for supper, and afterwards ate some figs. An hour or two after this, when about to undress for bed, she was suddenly seized with "faintness" and expired.

At the post-mortem examination (at which, owing to the courtesy of Mr. Dowd, the gentleman who was called to the patient when she died, Dr. Wiltshire was enabled to be present) the pericardium was found to be distended with blood, which had escaped from a rupture, three-quarters of an inch long, in the right ventricle. The muscular tissue of the heart was in a state of marked fatty degeneration. The valves, though the seat of some atheromatous change, were competent. In the posterior wall of the stomach, about the beginning of the pyloric third, were found three small ulcers, their surfaces being covered with discolored blood. Each was about the size of a split-pea, and they were grouped around the cicatrix of an old ulcer, which measured three-quarters of an inch by half an inch. The other organs of the body presented nothing notable.

The sudden death of the patient from ruptured heart permitted a verification of the diagnosis of gastric ulcer made during life; and the case affords another instance of the healing of large ulcers and subsequent invasion of the organ with a like lesion.

Editorial.

MEYNERT ON MELANCHOLIA.

Amongst medico-psychologists Professor Meynert, of Vienna, takes a high position for his researches into paralytic insanity, to the differential diagnosis of which he has recently made important contributions. In a paper read the other day before the Kaiserliche Gesellschaft der Aerzte in Wien, he has laid medico-psychology under still further obligations. His thesis was "The Primary Forms of Insanity"—melancholia and mania; and, in contrasting the two, he placed the former, both as to its genesis and its conditions, in a light which to our readers will be as novel as it is instructive.

Guislain, followed by Griesinger, and thereafter by Spielmann and Leidesdorf, added considerably to our knowledge of the subject when they referred melancholia to a psychical hyperæsthesia causing a psychical malaise. Of this hyperæsthesia, however, the only evidence adducible is that of an exalted susceptibility in the organs of special sense. Meynert, on the other hand, contends that the victim to melancholia is by no means abnormally sensitive to light, sound, and other impressions of the kind. Psychical malaise, moreover, is not, in the first instance, a constant factor in melancholia. Melancholics describe themselves, on the contrary, as sunk in profound listlessness of mind. Just as little is the feeling of anxiety, which is absent in so many melancholics, the root of their malady; while their frequent exemption from the neuralgias is not less notorious.

Professor Meynert finds the most constant symptom of melancholia to be a distressing hindrance of the thought-current; a blunting of the feelings; an unusual retardation in the transit of motory impulses, in connexion with which the psychical malaise is often only secondary and not strictly morbid. Melancholics, moreover, often complain of distressing sensations, difficult to describe, in the head. Interception of the thought-current reaches its acme in *melancholia cum stupore;* while it is present in greater or less degree in all melancholics. In the absence of anatomical detail, this stupor, as the expression of a primary psychical ailment, should, if traced to its efficient causes, conduct us to the exact source of the melancholic state.

Suppose a young man of twenty-three, addicted to hard study and free living, who has lapsed into this *stupor*, to exhibit ossification of the coronal sutures: the result of this contraction of the cranial cavity at an age when the brain is still growing, and requiring therefore a functional hyperæmia, must be the conveyance of an insufficient supply of arterial blood to that organ. A brain so defectively nourished sinks into functional exhaustion, signalised by interception of the thought-current and lowered intensity in the cerebral operations. Quite similarly, Prof. Meynert has seen a student who had lapsed into *stupor* present the symptoms of congenital hydrocephalus; the pressure from within conditioning the disposition to *stupor* in this case, just as the contraction of the cranial cavity did in the former. In other instances atrophy, due to anæmia of the brain, has caused this *stupor*.

The phenomena of milder melancholic ailment are ultimately to be explained by the above-mentioned condition of exhaustion (milder of course in degree) with its sequelæ of lowered intensity in the cerebral operations and interception of the thought-current. From this interception, it is apparent why the melancholic becomes alienated from all that once was dear to him. The stimulus imparted to the retina and the brain by the sight of a strange object on the one hand, and that of a cherished one on the other, differs in this: that the manifold, every-day contact with the latter, under varying aspects and dispositions, sets agoing on the links of association a multitudinous series of memories, which, though not amounting to absolute reproduction, yet renders the recognition of the object a well-outlined impression, and, through the focalising of so many occurring traits, an equally intense one. It is this "intensity" which forms the essence of a thought-affection. Through the interception of the thinking-current, these auxiliary traits in the melancholic patient do not well up in equal quantity or intensity of realisation: he remains unimpressed (*affectlos*). One consequence of impressions weakened in intensity is seen in melancholics who, in spite of perception otherwise sound, maintain that they had no sense of visual or aural impression; that the objects around them no longer exist, or no longer exist in their former guise: statements frequent indeed, and quite incompatible with the hypothesis of hyperæsthesia—just as incompatible as the fact that melancholics in the hyperæmia of incipient eruptive fever sometimes attain to clearness and soundness of perception beyond their wont. The hyperæmia, in other words, which in the sane man causes an excess of irritability, brings up the melancholic only to the normal pitch of cerebral activity.

Besides this non-productive phase of melancholia, the malady develops, in limited compass, a thought-process frequently of intense and continuous character. Illusory ideas are the most frequent embodiment of this psychical operation—ideas led up to by that change in the psychical system which is due to the mechanism of causality in the brain being taken possession of. At the expense of auxiliary or side impressions, these illusory ideas or representations will often attain to an overmastering intensity. From these the collective energy stored up in the brain is subtracted in smaller proportion the greater the obstacles interposed by interception of the thought-current to collateral or modifying associations. According to Fechner, the ideas or presentations confronting the light of attention are the only wakeful ones, the others being shrouded in partial sleep. So it is that in the brain of the melancholic this partial sleep obtains for the one-sided presentation unmodified sway. Within this narrow range of the one-sided intensity of the melancholic's brain the engrossing object of presentation assumes a vividness amounting to hallucination; just as in dreams the hallucination is coincident with the depression of the collateral energy of the brain—is favored, in fact, by the torpor of side impressions during sleep.

On the motory side the interception of energy in the melancholic is obviously not an absolute one. It is often, of course, unable to affect the usual intensity of impressions; but that unusual intensity which resides in the narrowed circle of the thought-process of melancholia it will affect. The strong presentations, the illusions of melancholics, often lead to energetic acts related to the illusory idea.

The psychical malaise of melancholia (not to be confounded with the feeling of anxiety) is to be regarded as the subjective consciousness of the deranged nutrition due to those interceptions in the brain-processes—as another phase of the effects of exhaustion on the vital energy of the brain, and is to be taken in the sense of Lotze, according to whom the melancholic's disinclination to effort depends on the conflict between outward impressions and the conditions of receptivity.

An instructive, purely psychological picture of the genesis of melancholia is afforded by nostalgia. As mountaineers become nostalgic even at home, and all mountaineers by no means become nostalgic abroad, so the victims to that form of melancholia must be regarded as men predisposed to the phenomena of brain-exhaustion and thought-interception. Transported to a new region, the weaving of new impressions with the images already stored up in the brain becomes too irksome to them; the slackened thought-flow, the lowered intensity of the newly-impressed images, lets the picture of their old world define and strengthen itself with morbid vividness.

This conception of melancholia as an expression of an exhausted cerebration makes completely intelligible the agency of the causal forces of the malady. So also become explicable the effects of intellectual strain, of intense emotion, of exhausting excesses; and similarly, on the anatomical side, the connexion of the disease with atrophy of the brain, which, in melancholics whom Professor Meynert has examined post mortem, yields on the one hand an average weight, and on the other a proportion of hemisphere to the composition, of the brain inferior in both cases to those of maniacs. Moreover, we can understand the rare occurrence in melancholics of brain-hyperæmia post mortem, only nine per cent. of melancholics being found in this condition as against forty-seven per cent. of maniacs.

Professor Meynert enters a little into the subject of the arterial nutrition of the melancholic and of the maniacal state; and points out that, while in the former tuberculosis occurs in 55 per cent., in the latter it occurs in only 14 per cent. In conclusion, the Professor's definition of melancholia would be this: the melancholic state is an affection of the central organ, in which the impaired development of vital power and the intercepted continuity of the excitory processes are realised as psychical malaise, most frequently with engendered illusions limited in character, and often with the accompaniment of irritant phenomena, particularly that of the feeling of anxiety.

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SCLEREMA ADULTORUM.

Attention is again directed by Dr. J. Neumann, in the *Wiener Medizinische Presse*, to the remarkable form of cutaneous disease to which the term "scleroderma or sclerema adultorum" has been applied. Though individual cases were recorded by older writers, no special essay upon it appears to have preceded that by M. Thirial in 1845, who published in the *Gazette Médicale* an account of all the cases he had been able to collect, and applied the term "sclerema" to the affection. Since then no less than fifty cases have been reported; and M. Neumann himself adds three more to the list. In most of the cases the skin becomes gradually harder and more tense, though in some instances, and especially when the attack is acute, there is a preliminary stage of œdema. Hebra and Rasmussen distinguish two forms of the disease—"sclerema elevatum" and "S. atrophicum," of which the former is the acute and the latter the chronic type. The disease has been preceded sometimes by rheumatism, sometimes by recurrent attacks of erysipelas, and occasionally it has appeared to originate in injury; but little is really known of its etiology. The symptoms are very variable; but there is always the disagreeable sensation of tension, impeding movement to a greater or less extent. Thus, if the sclerema affects the neck, the rotation and flexion of the head are interfered with. In sclerema of the face, the facial expression is lost, the natural lines and furrows are smoothed out, and the play of the features is lost; the eyelids droop; the alæ nasi are stretched, and the nose is flattened; the oral opening is contracted, and the movements of the lips rendered difficult. With regard to other parts of the body, the abdominal wall is made tense; the scrotum and penis so tense that no erection can occur; the elbows are bent; the fingers half extended and claw-like; the spinal column is bent forward; and the respiration even impeded. In one of Dr. Neumann's cases violent itching was complained of; and in this case the temperature was considerably elevated, though it is usually depressed. The secretion of the sebaceous follicles does not wholly fail, since pustules of acne have been observed to form. In some instances the secretion of sweat seems to be entirely arrested; but not in others. The disease has been observed to be associated with erythema, erysipelas proceeding to vesication, impetigo, ecthyma, eczema, partial teleangiectasis, ulcerations, and acne. It is more common in females than in males; and the majority of cases have occurred between the ages of twenty-five and forty, though one case is recorded in a patient aged six, and in another aged seventy-two. The pathological conditions of the skin that have been discovered under the microscope are hypertrophy and condensation of the subcutaneous cellular tissue, with coincident atrophy of the adipose tissue, deposit of pigment in the rete Malpighii, around the vessels, in the cellular investment of the several ducts, in the outer root-sheaths of the hair, and in the sebaceous follicles. The pigmentation, however, appears to bear no relation to the intensity of the disease, the densest parts being often the lightest colored. In one of Dr. Neumann's patients the epidermis was found to be unaltered, the cells of the rete Malpighii were increased in number, and formed wart-like processes, dipping into the cutis, not pigmented. The fibres of the cutis formed large and strong bands that preserved the characters of connective tissue, and were intermingled with thick trabeculæ of elastic tissue. Collections of cells occurred in the deeper parts of the corium, and in the panniculus adiposus, the fat of which was much atrophied. The sweat-glands were individually much hypertrophied; and the smooth muscular fasciculi were also much increased in size. The vessels running in the cutis were large and filled with blood; but the branches running towards the papillæ were small and empty. The hairs were small and woolly; but the sebaceous follicles were large. No treatment has been hitherto suggested that possesses any value in curing, or even in staying the progress of the disease.

Medical Annotations.

"Ne quid nimis."

THE CHEMISTRY OF THE TESTIS.

This subject, which has received but little attention from former observers, has been recently investigated by Dr. Treskin in the laboratory of Professor Hoppe-Seyler. In his experiments the tunica albuginea was removed from both testes in the steer, roedeer, and goat; and the proper gland-substance, after minute division with glass rods, was macerated for six hours in ten times its volume of water. This, being evaporated, furnished—1. *The watery extract.* The residue of the gland was submitted for the same length of time to the action of a weak solution of common salt, which gave—2. *The saline extract.* The residue of the gland-substance was then placed in warm alcohol, and kept in a warm-water bath for some time, thus furnishing—3. *The alcoholic extract.* Lastly, some water was added to withdraw the alcohol, and the mass was then immersed in a solution of soda for two days, yielding—4. *The alkaline extract.* 1. The watery extract was acid, and cloudy even after filtration, and gave an abundant precipitate with a little acetic acid. The precipitate was insoluble in solution of common salt, and, therefore, was not of the nature of globulin, but agreed in its chemical characters with alkali-albumen or acid-albumen. Further somewhat complicated research showed that the watery extract contained leucin, tyrosin, chloride of potassium, chloride of sodium, and an organic acid the precise nature of which was not examined. Alcohol took up from the watery extract evaporated to dryness leucin, tyrosin, and chloride of potassium, and on one occasion kreatin was found. Phosphoric acid and inosite also formed a constituent of this extract. Dr. Treskin noted the presence of kreatinin, but is unable to say whether it existed preformed, or proceeded from the kreatin already shown to exist in this extract. 2. The saline extract contained globulin and a myosin-like substance. 3. The alcoholic extract contained cholesterine-lecithin. 4. The soda extract appeared only to contain some salts and a little mucin. The percentage of water in the testis was 86·72, and Kölliker had already shown that in cattle, the solid residue being 13·035, the organic substances amounted to 11·427, and the inorganic to 1·308. Special examination for glycogen in the testes of dogs (not at pairing time) failed to detect any trace of its presence.

RAILWAY DUST.

A *résumé* of a paper read by Mr. J. Sidebotham before the Manchester Philosophical Society at a late meeting is contained in a late number of the *Monthly Microscopical Journal*, which includes some interesting observations whilst travelling by rail between Saltley and Camp Hill. Mr. Sidebotham spread a paper on a seat of the carriage, near the open window, and collected the dust that fell upon it. A rough examination of this, with the two-thirds power, showed a large proportion of fragments of iron, and on applying a soft iron needle he found that many of them were highly magnetic. They were mostly long, thin, and straight; the largest being about one fiftieth of an inch, and under the power used had the appearance of a quantity of old nails. He then, with a magnet separated the iron from the other particles. The weight altogether of the dust collected was 5·7 grains, and the proportion of those particles composed wholly or in part of iron was 2·9 grains, or more than one-half. The iron thus separated consisted chiefly of fused particles of dross or burned iron, like clinkers, and many were spherical, but more or less rough, being covered with spikes and excrescences, some having long tails like the old "Prince Rupert's" drops; there were also many small angular particles like cast iron, having crystalline structure. The other portions of the dust consisted largely of cinders, some very bright angular fragments of glass or quartz, a few bits of yellow metal, opaque, white, and spherical bodies, grains of sand, a few bits of coal, &c. After the examination of this dust it became easily intelligible how it may occasion great irritation. Mr. Sidebotham thinks it probable that the magnetic strips of iron are laminæ from the rails and tires of the wheels, and the other iron particles portions of fused metal either from the coal or from the furnace bars. He suggests the employment of magnetic railway spectacles and magnetic respirators in dry weather.

STRUCTURE OF THE AMERICAN SPONGILLA.

An abstract is given in the *Monthly Microscopical Journal* of Professor H. James-Clark's recent investigations into the nature of this species of sponge (Spongilla arachnoidea). Haeckel considers sponges to be essentially compound polypi, the ostioles of the sponge corresponding to the mouths of the polypi, the branching canals of the former to the similar canals in the polypidom of Halcyonarius, and by implication the cilia-bearing cells of the interior lining wall of the zoophyte finding their homologues in the ciliated cell-like bodies of the interior chambers of the Porifera. In opposition to this view Professor H. James-Clark maintains that these last are not mere cell-components of a tissue, but are each severally an independent body; in other words, he regards each ciliated body of the sponge as a cephalic member of a polycephalic individual. He describes these ciliated bodies as composed of nucleated masses of protoplasm from one part of which a single vibrating flagellum protrudes, the base of which is protected by a cylindrical transparent theca or collar. The amœboid portion of the cell is granular, and contains two or three contractile vesicles. These bodies line the interior of centres in the cytoblastematous matrix of the sponge, the flagella or cilia all pointing towards the centre of the cavity. From the somewhat condensed surface of the matrix, which he terms the inner investing membrane, spicula project obliquely and support, as a tent is supported on poles, the outer envelope or investing layer. There are no afferent and efferent canals, at least in the young state. The sponge lives in fresh water, adheres to the stems of water-plants, avoids light, and may attain the size of an orange or more.

Correspondence.

"Audi alteram partem."

THE LOCAL TREATMENT OF SMALL-POX.

To the Editor of The Lancet.

Sir,—I forward for insertion in your journal (if you think it worthy of a place there) the annexed communication, which, after due thought, I firmly believe deserves the careful consideration of the profession. I have patiently waited the results of the treatment in the cases in which I have tried it, and I think they justify me in submitting it to their judgment.

The present epidemic of small-pox, as is usually the case in all epidemics, has, I have no doubt, suggested various modes of treatment to medical practitioners, whose duties call them to treat this very offensive disease. I am not aware that any attempt has been made to treat it otherwise than constitutionally. Various suggestions have at different times been submitted to the profession of applications to prevent "pitting"; but no idea that I am aware of has been proposed of treating the disease itself locally; and yet, if we consider the progress of an ordinary case of small-pox, severe or otherwise (I omit hæmorrhagic variola as being beyond treatment from the incursion of the disease), the treatment by local means appears clearly indicated. After some forty-eight hours' febrile symptoms, more or less severe, a papular eruption makes its appearance, and concomitant with this eruption the primary fever subsides, the improvement being generally so marked that it is evident to the patient's friends. The papules progressing into vesicles, and the vesicles into pustules, are accompanied by the increasing "fever of suppuration," until at length, the whole cutaneous function being destroyed by the universal suppurating surface, the patient succumbs to the disease. It being thus evident that, the "primary fever" having subsided, the subsequent increase of serious symptoms keeps pace with the increasing local disease, it appears only reasonable to endeavor to arrest the local disease by local treatment. I and my partner, Mr. Quicke, have been trying to accomplish this in some nine or ten cases which have lately come under our care; and as cases of small-pox do not exist so much amongst the class of patients of which our practice consists, I strongly urge those of my medical brethren who have large experience in this disease to carry out my suggestion. My partner and myself have employed the liq. calc. sulph.; but I have no doubt other and less offensive applications will obtain the same result. From the known efficacy of preparations of sulphur in cutaneous affections, to which class of diseases I would now relegate variola, I think it desirable to employ those chiefly, and at present should wish them to try the liq. calc. sulph. of full strength. In every case in which we have tried it the patients praise it in glowing terms; and considering that the odor of the preparation is very objectionable, they would not, I think, be so warm in their praise of it unless they had proved its value.

A boy of some ten or eleven years of age, who on Sunday last first showed the papular rash *very* *thickly* spread over the face, and fairly abundant over the body, whose primary fever had been very violent, with wild delirium, had the lotion freely applied over the whole body. I saw him yesterday (Wednesday), and found him quite well; the papules remained papules, and proceeded no further in development, but showed minute crusts on the surface of each papule, and where several were clustered together, and would inevitably have become confluent, they remained *in statu quo*, arrested and destroyed. I am quite confident that had the disease been allowed to have its course, a serious and anxious case would have resulted; but by aid of the liq. calc. sulph. he is now in good health, with good appetite, and anxious to be up and down stairs.

This boy's farther took the disease while nursing a friend, and when I first saw him the papules were becoming vesicles. By the application of the liq. calc. sulph. they *immediately* became pustular, and died away; but he tells me he has had no scabs, and, having been the course of the disease in his friend, is astonished and delighted with the lotion, and, therefore when, upon his recovery, his son and wife were attacked, he immediately applied the lotion in the papular stage, with the remarkable result already stated.

In the whole of the cases treated by us the evidence of the patients is conclusive; and I think, whatever ultimate opinion the profession may form of this "cure for small-pox," they will not be justified without giving it a full and fair trial. Having treated the primary fever by constitutional means, my hope is that we may be enabled to treat with perfect success and satisfaction the cutaneous disease cutaneously, and that we may have arrived at the end of the horrors of small-pox. I must strongly insist on the free, copious, and universal application of the lotion, either laid on thickly and unsparingly with a brush or rubbed in with a glove at least three times a day, unless the skin (especially in the flexures of the joints) become sore. The injury to the mucous surfaces by variola I leave alone, only remarking that the condition of the mucous surfaces after cutaneous burns may lead by analogy to the hope that the speedy restoration of the skin to healthy function in small-pox may be accompanied by a healthy mucous surface.

I am, Sir, yours truly,
HENRY CARNLEY, M.D.

Hull, March 6th, 1872.

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HYPODERMIC INJECTION.

To the Editor of THE LANCET.

SIR,—Some of those who use remedies hypodermically have, no doubt, found that at times the needle will be got to enter quite easily, while at others it requires some manipulation, and even force, to get it through the skin. I will just mention a "wrinkle" by which the needle can always be made to enter easily—viz., to keep the bevelled point of the needle in front, in the way a carpenter ordinarily uses a chisel. The cause of this is too obvious to require explanation. No doubt some of my professional brethren have found this out for themselves; but I am not aware that such was stated in any of the medical journals.

I am, Sir, yours truly,
EDMOND NUGENT.
Fellow and Examiner in Arts, R.C.S.I.

Drogheda, Feb. 21st, 1872.

News Items, Medical Facts, &c.

DR. ARTHUR MITCHELL ON IDIOCY AND CONSANGUINEOUS MARRIAGES.—In the third Morisonian Lecture, delivered at the Edinburgh College of Physicians, Dr. Mitchell discussed the subjects of idiocy and marriages of consanguinity. With regard to the condition of idiocy, he believed that 60 or 70 per cent. of such cases in the British Isles were not congenital, but acquired, and due to one or other of the numerous accidents to which children are liable. In illustration of the aberrant character of the phenomena of heredity, he cited the case of a man whose father and brother and sister had six fingers on each hand, whilst he himself had the normal number. He married, and his first child was six-fingered; showing that, though he escaped the actual abnormality, he still possessed it potentially, being able to transmit it to his offspring. In connexion with the subject of consanguineous marriages, Dr. Mitchell disapproved of unions of near relatives, but said that proof was wanting that any evil resulting from them was dependent on a mysterious influence intrinsic in the consanguinity itself. His objection to marriages of blood relations arose rather from a consideration of the increased risk in such unions of the transmission of morbid peculiarities. Thus, if a deaf-mute is married to a person in possession of the faculties of speech and hearing, the chances of their having a deaf-mute child will be as 1 to 135; but if deaf-mutes intermarry, the chances rise to 1 in 20.

DENGUE.—This singular eruptive fever seems to be still very prevalent in Calcutta, and the *Indian Medical Gazette* contains some interesting papers on the subject. Dr. Charles, according to a clinical lecture delivered by him at the Medical College, considers that the term "dengue" is probably a Spanish corruption of the word "dandy," which was applied to the affection by the negroes in certain parts of the West Indies, owing to the erect or stiff carriage assumed by their companions while the subjects of the aching joints so commonly present in the disease. The most complete account of the disease is to be found in Aitken's article thereon in Reynold's System of Medicine. Judging from the accounts given of the cases at Calcutta, it appears to be very contagious; to be very sudden in its onset; the initial symptom, perhaps, being a sudden and severe pain in one or other of the joints, and to be attended with considerable fever, and a peculiar eruption, sometimes consisting of two kinds—one an initial rash of swollen red patches, and the other a measly kind of eruption that appears later in the case. It does not seem to be fatal, and one of the chief points of interest in connexion with it is the very painful joint affection, which appears to be of the nature of a nerve lesion.

ANTHROPOLOGICAL INSTITUTE.—At the ordinary meeting held on the 18th March, Sir Duncan Gibb, Bart., M.D., read a paper "On the Physical Condition of Centenarians." His remarks were founded upon an examination of six genuine examples, in whom he found the organs of circulation and respiration in a condition more approaching to the prime of life than old age. There was an absence of all those changes usually observed in persons reaching seventy years; and in nearly all the special senses were unimpaired, the intelligence perfect, thus showing at any rate the complete integrity of the nervous system. The author's views were opposed to those held regarding the extreme longevity of centenarians. In the discussion which followed Mr. E. Walford pointed out that the statistics of tontine associations placed the fact of centenarianism, which the late Sir G. C. Lewis had disputed, beyond the possibility of doubt.

A CENSUS recently taken in the Madras Presidency shows a total population of about thirty and a half millions, being an increase in five years of over four millions. Small-pox is again prevalent in Bombay.

PRINTED AND PUBLISHED BY
WM. C. HERALD, Agent, NO. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 7. NEW YORK, JULY, 1872.

Lectures
ON
OPERATIVE OPHTHALMIC SURGERY.

Delivered at St. George's Hospital.

By Robert Brudenell Carter, F.R.C.S.,

Ophthalmic Surgeon to the Hospital, and Surgeon to the Royal South London Ophthalmic Hospital.

LECTURE I.—Part I.

Gentlemen,—Apart altogether from the many questions of pathology and of therapeutics that have to be taken into account, the operative surgery of the eye is an art that demands, from any who would either understand or practice it, a careful and twofold study: first, the study of the mechanical acts that are to be accomplished by the surgeon, and of the various difficulties that may interfere with their accomplishment; next, the training of the hands to do the work to which they are to be put. In the operating theatre of the hospital you will have constant opportunities of seeing all the chief operations upon the eye performed with such skill as I can command; but I have nevertheless wished you to meet me here also, in order that I may explain many details which would there be tedious and out of place, and in order that, by the possession of certain *corpora vilia*, in the shape of the eyes of animals, I may be able to show you, in the first place, what ought to be done, and, in the next, what ought to be avoided. In the operating theatre one strives, of course, to limit teaching by example strictly to the former category; but here it will not only be permissible, but useful and advantageous, to exhibit the consequences that may follow from using instruments in an improper direction, or with an undue degree of force.

Notwithstanding, or possibly even on account of, the delicacy and complexity of its structure, the eye is extremely tolerant of surgical interferance. I may illustrate this tolerance by our experience here during the past summer and autumn, commencing with the 1st of May. In twenty-six working weeks from that date, a period which my vacation carries down to the 15th of November, I performed 212 operations in the theatre upstairs. Six of these were cases of enucleation, which may be set aside; and only two of the remaining 206 were followed by consequences destructive to the eye. In one of them, an iridectomy, undertaken as a forlorn hope, and attended by great loss of vitreous humour, led to wasting of the globe; and, in another, the same operation was followed a few days after its performance, and when the incision had already healed, by destructive purulent ophthalmia, possibly in some unknown way dependent upon what had been done, but possibly the result of accidental inoculation by a vaginal discharge from which the patient was suffering at the time.

I have selected the 1st of May as the starting point for this enumeration, because from that date the records of operations have been kept upon a plan somewhat different from that which had been adopted previously. The number 212 is far too small, and the period of time included is both too short and too near to us for the results of these operations to have any bearing upon questions of general statistics, or any value as evidence either for or against the desirableness of any particular method of procedure. They will serve, however, to show with approximative correctness the relative frequency with which different procedures will be required. The operations represent a smaller number of patients, because in twenty-one persons both eyes were operated upon; and sixteen eyes were operated upon more than once; as in cases where the capsule of an immature hard cataract was lacerated a few days prior to extraction; or in which a soft cataract was broken up with a needle a few days prior to removal by suction; or in which residual shreds of capsule or false membrane required division or removal subsequently to any form of cataract operation. The operations which involved penetration of the eyeball itself, or its removal, were 127; those on the conjunctiva, muscles, eyelids, or lacrymal apparatus were 85: each class composed as set forth in the following table:—

Extraction of cataract	22
Discission of cataract	2
Suction of cataract	9
Laceration of capsule to mature cataract	3
Division of capsule	10
Extraction of capsule	1
Iridectomy	68
Corelysis	4
Abscission of staphyloma	2
Enucleation of eyeball	6
Cauterisation of nævus	1
Cauterisation of bulbs of cilia	1
Division of stricture of nasal duct	12
Division of external canthi	8
Division of external recti muscles	5
Syndectomy	1
Operation for strabismus	83
,, ,, trichiasis	18
,, ,, ptosis	1
,, ,, entropium	1
,, ,, ectropium	3
,, ,, symblepharon	1

To these figures we shall by-and-by return; but for the present they may be laid aside.

I have said that the time included in the return is both too short and too near for the results gained to be of value as evidence; and the reason of this is that the success of operations upon the eyeball requires to be measured by a twofold standard—first from a surgical, next from an optical point of view. We say that an operation is surgically successful when it is properly accomplished, when the operator does what he wishes to do and no more, when it is followed by no destructive or injurious inflammation, and when the incisions that are made heal naturally and kindly. All this may happen, and the surgical success may be complete; although as regards optical success the operation may be a manifest failure, or its results may be long undeclared. Thus I have seen a cataract successfully removed, and a perfectly bright and transparent pupil restored, where the visual power of the eye was so much impaired by disease of its deeper structures that no benefit was conferred on the patient; and I often perform iridectomy in cases of turbid cornea, as a means of slowly clearing that membrane by the more active circulation which a diminished tension of the eyeball induces. I send the patient out of hospital perhaps at the expiration of a week, recording the procedure as a surgical success; but I must wait for six months, or perhaps for a year, before I can know whether it will be an optical success also, and will be followed by the results which it was my object and desire to bring about.

It would be manifestly improper, therefore, in speaking of the practice of the hospital during only a few months, that I should take optical success at all into account. Those of you who see that practice will, I hope, often witness the complete attainment of optical success, and will often observe evidence that it is on the way to be attained. But I cannot now take any statistical or percentage account of it without risk of being inaccurate in one direction or the other. If I should have the privilege of addressing your successors in this place, ten years hence, it will then be my duty to bring what I have done in the interval to this ultimate test; but, for the present at least, I estimate the success of a procedure by its surgical results alone.

With this preface, and before proceeding to describe operations themselves in detail, I come next to the means by which the various manipulations of opthalmic surgery are to be carried out—that is to say, to the instruments that are to be employed, and to the hands that are to employ them. Our surgical mechanicians have provided us with instruments in almost infinite variety; but the hands take precedence of them all, and require our earliest and most careful consideration. It was said, first I believe, by Beer, and has been said by many since his day, that an opthalmic operator must spoil a hatful of eyes before he cures one. Now, as the instruments of any period are a fixed quantity, and as adequate knowledge concerning when and why to operate may be attained by attentively observing the practice of others, it is manifest that the hatful of eyes must be a sacrifice offered up to the training of the hands. If we regard Beer's saying as a mere hyperbole (and I trust we may at least regard it as an exaggeration) repeated chiefly for its vivacity and its force, we cannot in like manner explain away the familiar maxim of political economy, that all learners spoil a portions of raw material. This maxim represents one of the necessary verities of life; and the surgeon who learns to operate upon the eye by primarily using the eyes of human beings to practise upon, and by thus in time gaining manual dexerity at haphazard—gaining by sheer practice the power to do those things well which he at first did badly, although he may eventually become skillful, obtains his skilfulness, I think, at an altogether unwarrantable cost. Instead of seeking such dearly bought experience, I would urge upon you first to consider carefully the nature of the mechanical acts which you are about to undertake, and to see how far, and by what means, the hands can be trained to the possession of the qualities required from them. There is, I believe, a contrivance, with a name in shop-keeper's Greek, by which persons learning to play on musical instruments may acquire strength and flexibility of finger without the production of sounds distressing to those in their vicinity; and on the same principle I think the hands of most persons may be so far educated, before an eye is ever touched, as to render the accidents which may result from sheer awkwardness unlikely, if not impossible. I say of most persons, because there are a few to whom the nature of their organisation denies the attainment of any kind of mechanical skill. Those cannot become ambi-dextrous who have been born ambisinistrous; and a want of power correctly to estimate nice differences in the degree and direction of exerted force seems to be, occasionally at least, the result of some structural peculiarity in the muscular or nervous system. I say peculiarity rather than defect, because this peculiarity constitutes, I think, one of the most marked organic differences between the sexes. It would be difficult to say how far it may be an accidental result of habits and of employment; but no one who carefully watches the muscular acts of women will fail to perceive a tendency to do them with a sort of rush, with a superabundance and a sudden exertion of force, rather than by the gradual application of the precise amount by which the end in view can be secured. The same thing may be observed less frequently in men; and I would counsel any such, if they have so far mistaken their vocation as to become surgeons, to turn their energies into some channel other than the performance of delicate operations upon the human subject.

The qualities of hand that are necessary to an accomplished eye-operator may be summed up under the following head :—

1. A high development of what has been called by physiologists "muscular sense"—the faculty by which we feel and estimate the degree of force we are exerting, either in pressure or traction.

2. The power of uniting the two hands in consentaneous movement, and of directing the intelligence to them simultaneously; so that they may work smoothly and harmoniously together as a single organ for the attainment of a common object, and may both at once be equally under the control and governance of the will.

3. The power to employ the left hand, indifferently with the right, for the use and guidance of cutting or other instruments.

4. Steadiness.

Each of these requirements demand a brief sepaate consideration.

It is by means of muscular sense that we are able to combine the precise degree of force that will accomplish our end, with the utmost gentleness in its application. There is a period in cataract extraction in which the operator may require important help from an assistant, and I confess that, unless my assistant is favorably known to me, I am always glad when this period is over. It is when an incision has been made into the anterior chamber, the aqueous humour has been evacuated, and the eyeball, rolled upwards by the reflex action of the superior rectus, must be drawn somewhat down before iridectomy can be performed. For this purpose, a pair of forceps should be made to close almost imperceptibly upon the conjunctiva and its subjacent tissue, and traction should be applied so gently that the eye rolls quietly down as soon as the resistance is overcome. There is then no strain upon its coats, and no gaping of the wound, unless the action of the superior rectus be quite spasmodic, in which case a little more chloroform would be indicated. But if the assistant snatches the conjunctiva hastily, and drags upon it rudely, it may well happen that the wound shall gape and the vitreous humour prolapse. The necessary gentleness does not imply slowness; but it implies that the first exercise of force shall be less than the amount required, and that the deficiency shall be made up by gradually increasing effort. The same principle applies not only to all forms of forceps traction, but also to all incisions, which, in the eye, are primarily punctures. In making them we must *place* the point of the knife or needle upon the spot to be penetrated, and increase our pressure in proportion to the resistance, but so gently that the speed of movement of the instrument is not accelerated when the resistance is overcome. In the aged especially, and in eyes that have been long diseased, we often find the cornea soft, thin, and flaccid; and if we were to apply to such eyes a knife or needle, with the full pressure that would be necessary to penetrate a healthy cornea, we should incur much risk of driving the point of the instrument further than we intended it to go, while the shock or concussion to the eyeball would tend to expel the aqueous humour, which it is often most important to retain. These matters must be dwelt upon in greater detail by-and-by, and are now only adverted to for the sake of illustration.

The requirement of consentaneous movement of both hands arises in great measure from recent progress, partly from the frequent use of two instruments at once within the eye, and still more from the practice, now become almost universal, of fixing the globe with forceps. The forceps may be used either to steady the globe in some required position or to move it somewhat against the edge of the instrument; and in either case they should exert no undesigned pressure, and should never be forgotten by the consciousness. It is very easy for a young operator, when using a knife or needle in one hand to bear heavily with the forceps in the other, and in this way to do mischief which a proper attention to, and governance of, the comparatively passive hand would have avoided.

The requirement of ambidexterity arises from the fact that, for most operations upon the eye, it is convenient for the operator to stand behind the recumbent patient, while, at the same time, the projection of the nose renders it necessary that nearly all punctures or incisions should be made from the temporal side. Many excellent operators have failed in attaining ambidexterity; and some of these have displayed sufficient human nature to contemn in others the accomplishment that was denied to themselves. By contrivances more or less awkward, by coming round in front of the patient for the left eye, and by committing fixation to an assistant, it is doubtless possible to do most opthalmic operations with the right hand alone. I have heard of one surgeon who places his patient upon a chair, and then seats himself upon the patient's knees, in order to get at the left eye with the right hand. This is undeniably ingenious, but the ingenuity is somewhat perverted. Sufficient ambidexterity for the use of the knife in the left hand follows, as a matter of course, from the training required for the proper use of fixation forceps; and full ambidexterity may usually be attained with a very moderate degree of practice and trouble.

For good operating absolute steadiness is indispensable, because the commencement or the course of an incision should not deviate by the fraction of a line from the previously formed plan of the surgeon, and because it is often desired to introduce a small blunt instrument, such as a hook or a closed forceps, through an opening previously made for the purpose. In order to avoid, in such cases, premature loss of aqueous humour, as well as to avoid any bruising of neighboring parts, the blunt instrument should be placed at once between the lips of the puncture prepared for its reception. Steadiness, as a mere physical attainment, is one of the results of training; but there is an unsteadiness produced by mental causes, by anxiety to do right, or by want of self-reliance, which can only be remedied by the gradual growth of confidence and the gradual influence of success.

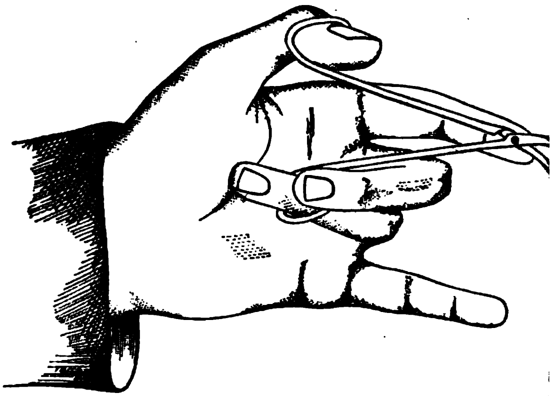
LECTURE I.—Part II.

You see here, gentleman, a contrivance intended to facilitate the acquirement of dexterity. It is a mask representing the human face, and set upon a jointed pedestal capable of being fixed at any angle, so as to render the plane of the face vertical, horizontal, or inclined. Behind holes corresponding to the orbital openings there are spring clips, capable of holding artificial eyes or the eyes of animals, and fixed to spiral springs, so that the inserted eyes obtain somewhat of the natural mobility. By the aid of this mask I shall be able to demonstrate many of the points to which I have to call your attention, and upon it you will be able to practise operating at your leisure.

Before the mask is used there is, however, a great deal to be accomplished by the aid of simpler means. I once met with some very good instructions for persons wishing to learn the use of the sewing machine; and the writer laid great stress upon the necessity of acquiring complete mastery of the treadle before the manual part of the work was even commenced. He asserted the sound principle that the power to accomplish a complex mechanical procedure should be obtained piecemeal; the eventual combination of different movements being far more easy than their simultaneous acquisition. On this principle, the first thing to be done, as a preliminary to operations upon the eye, is the practice of equal and consentaneous movements of both hands. Free drawing with both hands at once is perhaps the best exercise for the purpose. The paper should be placed on a desk, conveniently sloping so as to support the hands, and then two similar curves should be drawn simultaneously, so as to meet in, or to proceed from, the same point. Two pencils should at first be used, and it will be found that several difficulties have to be overcome. The left hand will lag behind the right, and will move with less freedom, so as to form a smaller curve, and to form it more slowly. When simultaneous and similar movement is at last obtained, there will still be differences in the amount of pressure exerted; and these differences may be rendered visible by substituting pens for pencils, so that the degree of pressure will determine the thickness of the line. To such exercises as these, many of the otherwise wasted intervals of leisure may be profitably devoted, and with the result that in no long time the two hands will be combined by the volition and intelligence into one machine, made up, indeed, of essentially independent parts, but still capable of equal and harmonious action. It may safely be laid down that the superiority of the right hand over the left, for the purposes of fine manipulation, is almost entirely due to the cultivation of the muscles of the former by the practice of writing and drawing; and drawing with the left hand may be cultivated indefinitely, and with much surgical advantage.*

There is one instrument only which requires, when used by the left hand, a special form of manipulation. Ordinary scissors are so made that the natural opponency of the right thumb and medius brings their edges into apposition, while the left thumb and medius tend rather to separate them. When taken in the left hand, the thumb should pass through the upper loop, and the ring finger through the lower, as shown in the subjoined illustration (Fig. 1). The medius should rest in front of the lower loop, and in firm contact with it, and the point of the index against the lower blade, a little in front of the joint, and should there make pressure. Thus held, scissors will cut to the points as well in the left hand as in the right. For excising a portion of the iris, it is generally possible to use the right hand for scissors with either eye; but for strabismus operations on the left eye, and for removing skin from the left eyelids, the mastery of scissors with the left hand, if not essential, is at least conducive to neatness and despatch. In a subsequent lecture I shall have to show you some new scissors, recently patented by Messrs. Weiss, which are equally convenient for either hand.

FIG. 1.



When the hands have become accustomed to work together, and with equal speed and force, by means of the drawing exercises, the mask may next be taken into use. At first the orbital hollows may be left vacant, while the learner practises holding the knives and needles required. The instrument should be held like a pen, in the thumb and first two fingers, while the remaining fingers and a portion of the ulnar margin of the hand rest upon the brow; and the position should be such that the fulcrum need never be shifted, and that all necessary movements may be performed by the simple flexion or extension of the grasping fingers. In this way the operator should be able to touch either the temporal or nasal margin of the orbital opening; and to execute any sawing movement within these limits. Next, the orbital hollows may be filled with something to represent eyes. Small wooden balls, covered with white leather, on which a circle is drawn to represent the corneal margin, are very convenient to begin with. The leather covering may be seized by fixation forceps, and the operator may proceed to mark the corneal margin by minute dots placed at close and regular intervals. If he use a fine pen, he will produce a permanent record of his power to place the point of an instrument precisely upon a selected spot; and he may next proceed to connect two dots together by a drawn line. After a while hollow balls of soft india-rubber may be substituted for the wooden ones, marked in a similar manner, and used to practise the art of puncturing or incising the eyeball with the proper degree of force, and in any required direction. The eyes of recently killed animals will come next in order, and upon them all the steps of the principal operations may be performed. The best eyes are those of pigs, cats, and rabbits; the two latter being especially well adapted for operations upon the iris. The animals should be killed by some means that does not produce extreme dilatation of the pupil.

It has been well said, gentlemen, that what common people call genius is little more than a great capacity for taking trouble. I have recommended you to take a great deal of trouble in order thoroughly to accustom yourselves to the manipulations of ophthalmic surgery; and if you follow my advice I am sure you will find the time well spent. You will come to the real work as skilful craftsmen, and not as awkward and blundering novices. It has more than once been my lot to see attempts to operate upon the human eye, upon the eye of a trusting patient, by a person who did not know how to handle the implements he was about to misuse; and I can conceive few things more painful than such a spectacle, unless it be the reflections suggested by the inevitable consequences of the so-called operation. Let me remind you, moreover, that the results of your early eye operations will have a very marked effect upon your future success, either as general practitioners or as specialists. If circumstances should lead you into rural districts, and you find there, as you well may, old people whom you are able to cure of blindness by the extraction of cataract, your work will command immediate and wondering appreciation, while if you condemn these same old people only to pain and disappointment, you will be inevitably and most deservedly distrusted. If your lot in life should be to undertake hospital duty, the early possession of manual dexterity, and of the well-founded self-reliance that it entails, will be the surest passport to the confidence of your colleagues and *confrères*.

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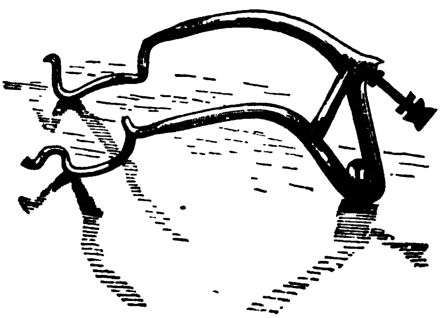
* It is not generally known that the difficulty of *writing* with the left hand depends almost entirely on the direction of the movement from left to right, and on the customary slope of the letters. It is perfectly easy to write with the left hand the *reverse way*—i.e., to execute with the left hand a movement analogous to that which we constantly execute with the right. Any person who is temporarily deprived of the right hand may conduct correspondence by writing with a style with the left hand, the reverse way, on thin paper, with carbonic paper underneath. The sheet must then be turned over to be read, and the words will run in the ordinary fashion.

I pass next to the consideration of instruments, of which, as I have said, there exists an almost bewildering variety. I think it is Charrière, of Paris, who has formed a sort of museum of forgotten eye instruments, and I am afraid to say how many specimens it contains. But the instruments now living, so to speak, and familiar to us, may be divided into two great classes: the essential, which are the representatives of inventive ingenuity, and the superfluous, which are the representatives of inventive awkwardness. The former class are contrived by good mechanicians, the latter by bad surgeons. There are certain things for which the fingers cannot be spared, or which they cannot do, and for these things instruments are essential. The fingers cannot be spared to hold open the eyelids; and, therefore, as the fingers of an assistant would be in the way, specula are essential. The fingers cannot fix the eyeball without exerting dangerous pressure, and therefore fixation forceps are essential. The uses of iris forceps, of hooks, of scissors, of cutting blades of different sizes and shapes, require no demonstration; and all these must be adapted to the work that they are required to perform. But in all ages and countries it is the bad workman who complains of his tools, and it is the good workman who produces the most varied results by the most simple means. A man who is very awkward, and whose awkwardness is perpetually bringing him to grief, hits upon a contrivance by which he hopes that that natural result may in some degree be obviated. He calls this contrivance an invention; and, like those persons of whom the Apostle tells us that they glory in their shame, he is often not a little proud of it. I have lately seen a description and drawing of a curiously deformed strabismus hook; with a claim that the deformity would cause it to hold the tendon securely, and without slipping. The inventor had not learned how to hold a common strabismus hook, or he would never have felt the need for his new fangle. Many surgeons of great and deserved repute have invented each a single instrument, such as Beer's knife or Tyrrel's hook; and some have invented more than one, chiefly because they have struck out some new procedure, for which new appliances were indispensable. But, as a rule, the invention of instruments by a surgeon is an irrefragable proof of his clumsiness; and when you find that any single operator has his peculiar scissors, and his peculiar hook, and his peculiar forceps, and his peculiar scoop, all called after his name, the best advice I can give you is to take care that he is never asked to operate upon your own eyes, or upon the eyes of any in whom you feel a personal or especial interest. Go rather to the man who has never invented an instrument in his life, but whose daily practice affords proof that he can use those invented for him by others.

In this introductory or general lecture, only the instruments common to nearly all operations, the specula and the fixation forceps, need to be described. The others will find their place when I have to speak of their respective uses. The speculum should possess two essential qualities. It should hold the lids quite away from contact with the eyeball, so that no spasmodic action of the orbicularis can exert injurious pressure, and it should be perfectly out of the way of the operator. I only know one form of instrument in which these desiderata are combined; and that, which is shown in the annexed woodcut (Fig. 2), is the invention of Mr. Foveaux, of the firm of Weiss and Sons. Its cross bar and screw-stop are too near the eye to allow of the parts in front of them being bent or closed by muscular exertion, and the curve removes the bar quite out of the way of the handles of instruments. The speculum is reversible, and may be used for either eye. It must be introduced gently, when nearly closed, and must then be opened so as to put the lids fully upon the stretch, and to carry them away from the eyeball towards the margins of the orbit. It is next fixed by a turn of the screw, and, when released prior to removal, it must be prevented from closing suddenly.

FIG. 2.



Fixation forceps are of two kinds: those which seize the conjunctiva and subconjunctival tissue, and those which penetrate the sclerotic. The former present a blunt extremity when closed, and one blade has two little teeth, which are received between three corresponding teeth of the other. The figure (3) shows the entire instrument, and the points, both open and closed, in somewhat larger size. Such forceps hold well in young and healthy eyes. But in the aged, and after prolonged inflammation, the conjunctival tissues often tear with extreme readiness, and the forceps not only produce troublesome lacerations, but may lose their hold at a critical time of the operation. To meet this difficulty the perforating forceps were contrived, at about the same time, by Mr. Bader, of Guy's Hospital, and by M. David, of Angers. Each blade terminates in a single fine sharp point, and the two points cross to the extent of a line, as shown in Fig. 4. In use, the forceps are closed until the points are about a line apart. These points are then placed on the sclerotic, *immediately* external to the corneal margin, and are driven in and closed by a simultaneous movement. They then hold the eye with a grasp which never gives way. They seldom perforate the anterior chamber; but their doing so would be quite unimportant, as they fill their own punctures until they are removed. I have never seen these punctures become the seat of any irritation, or give the smallest subsequent trouble, and my own practice is always to use the perforating forceps in order to incise the eyeball, but to lay them aside for the others in the subsequent steps of an operation; on the ground that the pressure required to give them their hold might do mischief, by starting the lens or vitreous body, when once the support of the aqueous humour was withdrawn. I have seen surgeons attempt to use these forceps by pinching up a fold of conjunctiva between their points; but in this way their action is very unsatisfactory, and I quote the example rather for avoidance than for imitation. The conjunctival fixation forceps may also be used in a right way and in a wrong. They should be held directed towards the centre of the eyeball, placed, a little open, upon the conjunctiva near the cornea, and closed while making gentle pressure upon the globe. They will in this way lift up a fold which contains not only the delicate conjunctiva, but also the tougher subjacent tissue; and the extremity of the closed forceps should be kept in gentle contact with the globe while traction is being made. If only a fold of conjunctiva is pinched up, and if that fold is lifted off the sclerotic, the membrane will probably tear, and the position of the eyeball will be very insufficiently controlled.

FIG. 3.

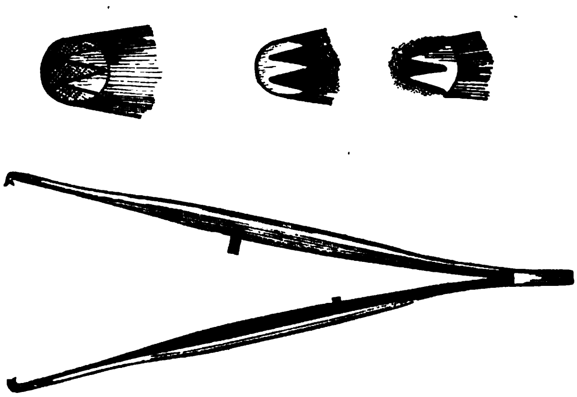
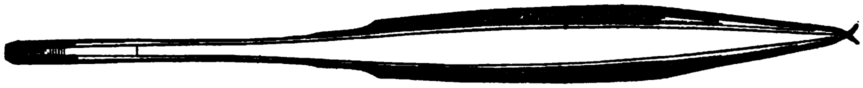


FIG.



In my next lecture I shall first discuss the employment of anæsthetics in ophthalmic surgery; and shall then proceed to the consideration of the operations for cataract.

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ABSTRACT

OF THE

Lumleian Lectures

ON

DISEASES OF THE MUSCULAR WALLS OF THE HEART.

Delivered at the Royal College of Physicians, March, 1872.

By RICHARD QUAIN, M.D., F.R.S.,

Physician to the Hospital for Diseases of the Chest, Brompton.

LECTURE III.

IN *fatty degeneration* the tissue of the heart is paler than natural, light-brown or buff in tint, the altered color being most marked in spots and mottlings. Its consistence is greatly diminished; sometimes it breaks down under pressure as would a lung consolidated by pneumonia. It is often greasy to the touch. In cases depending on local causes, as obstruction to the coronary artery, a small portion only may be affected, but in the highest degree. Where the cause is a general one, the change may affect the whole organ, but be less extreme. Under the microscope, it is seen that where the disease is commencing the striæ become fainter, and small dark dots appear within the fibre; a few of these dots are not inconsistent with a tolerably healthy state of fibre. Granular degeneration of the nuclei occurs in a very early stage, as Sir James Paget has pointed out. Where the change is more advanced, the transverse markings have disappeared, the black dots have increased in size and number, and some have become transparent in the centre. Finally, the whole area is occupied by granules and fat-globules. This is not such fat as is found on the surface or between the fibres, since the globules are smaller, appear to have a mere albuminous envelope, and are extremely like the oil-globules of milk. The diseased fibres appear friable, and break up readily into small fragments. This change is found most frequently in the walls of the left ventricle, next in those of the right ventricle, then in the right auricle, and lest frequently in the left auricle. It is generally more evident in the columnæ carneæ and inner layer of the muscular fibres than elsewhere.

Concerning the nature of this change, the lecturer, many years ago, in a paper in the Medico-Chirurgical Transactions, expressed his belief that the fatty matter was the result of a chemical or physical change in the composition of the muscular tissue itself, independent of those processes which we call vital. This opinion—now almost universally accepted—was founded on three groups of facts: firstly, that in dead animal matter removed from the body, the place of the nitrogenous elements is found, under certain circumstances, to be occupied after a time with fatty matter which could not have existed as such during life; secondly, that a similar change occurs in portions of dead animal matter within the living body; thirdly, that a change of albumen into fat very frequently occurs in living matter during the processes of nutrition. The change in albuminous matters out of the region of life is illustrated by the formation of adipocere. Chemists differ as to its precise origin; some thinking it is merely the remains of fat previously present as such; others, that it results from a change in albuminous matters. That the latter is the correct view is proved by microscopical examination, which reveals the normal elements in process of disappearance, and the gradual substitution for them of fatty matter which did not exist as such during life. The formation of adipocere in our preparation-jars corroborates this; healthy tissues there undergo distinct fatty degeneration. Analogous changes occur in the vegetable world. The quantity of oil in olives is increased by keeping them; and further, on a gigantic scale, in the formation of coal, a process goes on analogous in its essential nature to the formation of adipocere. In an early stage of the coal-forming processes from peat, fat can be obtained convertible into candles.

The occurrence of fatty degeneration in albuminous substances within the body was investigated by Wagner, who demonstrated its occurrence in animal substances enclosed in the peritoneal cavity. Michaelis corroborated these observations; and discovered, in the evolution of ammonia during the process, the destination of the nitrogen of the albuminous compounds. The change of albumen into fat during many nutritive processes is well known. The degeneration of cells, secretion of milk, involution of organs, and phosphorus-poisoning are all illustrations of the same process.

It might seem superfluous to dwell on these facts. Dr. Ormerod, however, has recently argued that fatty degeneration is an infiltration of fatty matter derived directly from the passing blood, or made on the spot under its influence, and that the process is not adipocerous transformation; and, further, that the fat thus infiltrated in man is oleine, and not the physiological form, margarine. But to the other instances already given incompatible with the former statement may be added the observations of Mr. James Salter, that the nitrogenous pulp in the closed cavity of a necrosed tooth becomes transformed almost entirely into fat, and that the process of fatty degeneration may be watched within the muscular fibres of certain larval shrimps preserved in spirit-and-water. Even admitting a difference of chemical composition in the fatty degeneration of the preparation-jar and of the living heart, it must not be forgotten that in the latter the fat inside cannot be kept distinct from the fat outside the muscular fibres; and so the result of chemical analysis can have little weight against the ascertained facts just alluded to.

The causative conditions under which fatty degeneration occurs are both general and local. The general states are those of impaired nutrition, starvation and analogous diseases, extreme anæmia, phthisis, cancer, acute adynamic diseases (as fever, in which it is sometimes the cause of sudden death), diphtheria, and some poisons. Among local conditions, obstruction of the coronary arteries is one of the most conspicuous. It was present in 25 out of 83 cases. The artery is sometimes found extremely ossified, and going directly to the only part of the heart affected. There are no communications between the two coronary arteries, and hence in obstruction of one no collateral circulation can be set up. Of course the disease in the arteries is itself an evidence of general impaired nutrition. Endo- and pericarditis are the local influences which frequently produce fatty degeneration. Out of 68 cases, they had occurred in 17. Possibly dilatation of the right side, and congestion of the veins of the heart itself, may have some influence. The disease is more frequent in males than females, in the proportion of 5 to 2. It is met with at all ages, but in progressive frequency as life advances. About half the cases occur in persons over 60. It is more frequent in the lower than in the upper classes. In fatty hypertrophy of the heart these proportions are reversed, and the same contrast between the two is seen in the other causative conditions.

This state usually occurs in association with hypertrophy, and so, in most cases, the fatty heart is altered in size. The new tissue is prone to decay; but in some cases increase in size may be due to an attempt at compensatory growth. The softness of texture which results is the cause of some of the most fatal accidents to which the heart is liable. The change also interferes with the function of the organ—an interference shown chiefly in the signs of diminished power. Coma, giddiness, headache, and syncope are prominent on the side of the nervous system. The latter is not seldom fatal. Out of 68 cases, death occurred in 21 by syncope lethalis. Sudden death after delivery may not improbably be due to this cause, and be an occasional attendant on the partially degenerative involution of the uterus, which must occur subsequent to its enlargement during pregnancy.

Interference with the functions of the chest is shown by shortness of breath, recorded as present in half the cases. It is sometimes of a peculiar character, as Cheyne and Stokes observed. The inspirations during each attack steadily decrease in strength and duration till they are almost imperceptible, and after a few seconds gradually increase, and go even beyond the medium to a distressing intensity, and then slowly subside. Pain was present in 26 out of 68 cases; in 16 confined to the region of the heart, in others more widely distributed, and in some occurring together with breathlessness, &c., in anginal seizures.

The diagnosis of fatty degeneration is often difficult, because dilated hypertrophy and fatty hypertrophy produce so nearly the same symptoms. The physical signs are essentially those of weakness—the visible impulse limited, and often so feeble as to be imperceptible. The cardiac sounds are feeble, and the first is short. The pulse is always weak, often irregular, and in frequency normal, or either too fast or too slow. Such signs, together with symptoms of cardiac weakness—breathlessness, faintness, and pain,—in a person of advanced years who presents other evidence of systemic degeneration, or in a younger person during some chronic disease, may lead us to assume more or less justly the existence of the lesion. In treatment we must bear in mind the cause of the affection. When due to a general state, we may hope to do good by tonics; but when caused by a local condition, we can only lessen the trouble by imposing as little as possible on the weakened heart; let the conditions in which it works be as healthy as possible, by attention to secretions and hygienic measures. And further, as Dr. Fothergill suggests, strengthen such fibres as remain by the administration of digitalis. Many cases will improve considerably.

Rupture of the heart may be said never to accur spontaneously when the heart is healthy. The nature of the morbid change has been variously described; most commonly it is fatty degeneration. The rupture itself is sometimes barely large enough to admit a probe; sometimes it is an inch or more in length. It may be longer on the outer or the inner surface. There may be more than one. The edges are jagged and irregular and sometimes ecchymosed. Where the rupture is due to abscess or ulceration these appearances may be different. Among causative conditions the influence of age is conspicuous. Out of 88 cases 63 occurred in persons over 60 years of age, 33 between 60 and 70, and 24 between 70 and 80. The two sexes are affected with equal frequency. It usually occurs at the moment of making some effort or at a period of mental excitement. The final attack is sometimes preceded by symptoms referable to the heart, but in most of the cases no such symptoms are mentioned. The lesion itself, where the patients have lived long enough to describe their sensations, has always been marked by intense cardiac suffering, more or less distress in breathing, restlessness, rapid and irregular pulse, faintness, pallor, coldness of skin, sometimes vomiting, and various nervous symptoms. The duration of these symptoms varies: out of 100 cases death was sudden—that is, within one or two minutes—in 71. One patient, however, lived eight days, one six days, one three days, and five lived over 48 hours. These differences depend partly on the size, partly on the seat of the opening, whether in the septum or outer wall. The process of tearing through may be very slow. The physical signs at the time of the rupture, so far as they could be ascertained, were some increase in the amount of cardiac dulness, muffled heart sounds, and weak and intermitting pulse.

Comparing the seat of rupture in 100 cases, we find the left ventricle was ruptured 76 times, 45 of which were in the anterior wall; the right ventricle 13 times, 9 being in the anterior wall; the right auricle 7 times, the left twice; the septum 4 times. These numbers accord with those of Elleaume and other observers.

The state of the cardiac walls permitting the rupture has been described as softening, ulceration, fatty degeneration, and interstitial hæmorrhage. Out of 100 cases the heart was the subject of fatty degeneration in 77; in 6 it was "softened." In one case the rupture was due to the bursting of an aneurism, in one to an abscess. In 12 the heart was said to have been healthy, or not examined; but in most, mention is made of endocarditis or changes in the coronary artery. No doubt its immediate cause is always some undue strain. When there is a small spot of extreme softening the contraction of the adjacent healthy fibres may produce the rupture. In nearly all cases blood escapes into the pericardium, which separates into clot, surrounding the heart, and serum filling the cavity.

As far as we know rupture is always fatal. Wounds of the heart occasionally heal, but then they have occurred in healthy organs; rupture only in those already extensively diseased. In treatment, the great object should be to secure complete rest, and to avoid any active treatment which might excite the heart.

Aneurism of the heart consists of a pouch or sacculus situated in the substance of the muscular walls, or projecting outwards, and forming a tumour on the exterior, communicating with one of the cavities. A case was published by Galeatti in 1757, and since then many have been recorded and memoirs have been written. The external shape of the heart is sometimes normal, but usually altered and irregular; a projecting tumour existing at one part which may be so large as to produce the appearance of a double heart. There may or may not be a constricted neck to the sac. The walls of the aneurism are formed of the thinned and altered cardiac walls, all the layers of which may generally be detected near the base, but in many cases over the rest of the wall the muscular layer has disappeared. Occasionally bony plates are found in the sac. The aneurismal swelling may be adherent to the pericardium over the whole or part of its extent.

This affection seems to occur with nearly the same frequency at different ages. Out of 56 cases collected nearly the same number died in each decade between twenty and seventy. Of the same number 39 were males and 17 females; and in the statistics of Thurnam and Hasse the preponderance of males is equally marked. Concerning the mode of origin of this lesion various opinions have been held. Probably some cases originate in inflammation, either by the mere softening of the wall, or the production of a fibroid cirrhotic state which yields to the internal pressure, or by the bursting of an abscess into the cavity, the sac of the abscess constituting the aneurism. Fatty degeneration may also give rise to aneurism either by causing local softening, which permits dilatation, or by permitting hæmorrhage into the wall, from which a cyst may be produced that shall ultimately communicate with the interior of the heart. Whatever be its origin, there is invariably some chronic thickening and induration about it, and a considerable development of fibroid tissue in its walls.

With regard to the seat of the lesion, in all 304 cases are available for comparison, including those recorded by Thurnam, Hasse, Lobh, and Hartmann, of which 138 were at the apex, 101 at the base, and 64 at the intermediate part. Rokitansky suggests that the frequency with which the apex suffers is due to the proneness of this part to be affected with chronic myocarditis.

Symptoms were absent in 13 out of the 56 cases. When present they consisted of pain, dyspnœa, lividity, palpitation, and irregularity of pulse. In 10 cases murmurs were present. Thus the symptoms are not special, and it is only in rare cases that by a peculiar extension of cardiac dulness the existence of an aneurism might be guessed at. The result is usually death, either slowly by embarrassment of the heart's action, or suddenly by rupture of the sac. Rarely it may undergo conservative changes, as in a case recorded by Dr. Wilks, in which the walls of the sac had become bony, and its cavity been filled up.

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Clinical Lectures

ON

INFANTILE PARALYSIS AND ITS RESULTING DEFORMITIES.

By Richard Barwell, F.R.C.S.,

Surgeon to Charing-Cross Hospital.

LECTURE II.

Gentlemen,—A malady which has received the name of the "temporary paralysis of Kennedy" places the immediate treatment of the disease we are considering in a most unenviable position; while, on the other hand, a belief in the frequency of such transient paralysis often prevents any clear and decisive action for days, perhaps weeks, after the first attack. It is a very thankless task to treat a paralysed child with all care, and then, when the patient recovers, to feel that it may have been one of those cases which would have done well without our aid. Nevertheless this is far better than, believing in the temporary nature of the disease, to temporise, to delay still a little longer, and at last to find the limb wasting, and to discover that by loss of time we have also lost, or all but lost, the chance of regaining a useful limb.

My conviction, taken from a large number of cases, is, that the temporary paralysis is a very rare, the true infantile (non-central) paralysis only a too common disease. The distinction in the first few hours is impossible, but in from twelve to thirty-six hours it becomes clear and indisputable. When, therefore, first called to a paralysed child, it is necessary to ascertain all concomitant and pre-existing circumstances. If there be or have been no illness, no prolonged or severe fits, no thermometric elevation either general or merely cephalic, nor other sign of central mischief, the medical man will examine the state of the secretions, ascertain the absence or presence of worms, the condition of the mouth and other parts which might give rise to reflex paralysis,* and if he find there no clue to the cause of the disease, his next object will be to discover whether the case be one in which the condition is likely to continue, or one in which the loss of power is merely temporary. For this purpose, as well as for subsequent treatment, he must have recourse to different forms of electricity, the use of which should, if possible, commence between the twelfth and thirty-sixth hour of the attack.

To gain information from galvanism it is necessary to use two sorts of battery—the one a single or double cell, with an induction coil and a connexion-breaker or hammer; the other a simple galvanic apparatus for producing a constant current of great tension and small quantity. Although for the first-named or induced current any battery with a small coil and means of regulating will do, yet Stöhrer's large double-celled battery is by far the best apparatus I have yet come across. The many means of regulation give great power of minute adjustment; the connexion, by standards, of the secondary current enables us to use the least painful stream with the greatest effect; and the attachment of the striking plate of the hammer to a spring enables us to make the widest possible interruptions. For the constant current a battery made originally for me by Messrs. Weiss is the best I have yet found. It consists of fifty elements, three-quarters of an inch wide by two inches and a half long. In a certain number of cases many less cells will answer for treatment, but a smaller battery would, for purposes of diagnosis, fail in so large a proportion of cases as to be almost useless.

The behavior of muscles to one current and to the other, and the alternation of such behavior, afford, when properly studied and understood, a mode—indeed, the only mode of diagnosing the condition and prognosing the future of an infantile paralysis. In the form of this malady (the only paralysis really peculiar to infants) which is not cerebro-spinal nor reflex, sensibility of muscles to the induced current rapidly declines and becomes extinct. This phenomenon commences between the twelfth and thirty-sixth hour. It is usually complete within four days. If, then, the induced current have been used twelve hours after the occurrence of a paralysis not heralded by either severe illness or accident, and the muscles be found tolerably sensitive, we must remain very doubtful as to the nature of the case. If within twelve hours more the sensibility have diminished, we have to do with a case at all events of no temporary paralysis. If in a few more hours faradaism produce no muscular contractions we may feel sure that the disease is one of peripheral origin.* If about the time when the power of the induced current decidedly declines—perhaps two or three days after its disappearance—the muscles begin to respond with abnormal sensitiveness to the constant current, but at other times remain flaccid, the case is undoubtedly true infantile paralysis, which will not get well save by careful and more or less protracted treatment. If, however, the case be brought under skilled management sufficiently early, and treatment carried on with patience and watchfulness, one may in most cases set every muscle free, and this merely by the proper use of galvanism and faradaism. Hence I must be permitted to speak very strongly on the impropriety of postponing treatment under the impression or the hope that the case is but temporary. A muscle or a set of muscles which do not recover power in three days will scarcely ever of themselves regain any useful amount of action. If we attack paralysed muscles while they are still sensitive to galvanism we may in a few weeks restore power; if we wait till not only the induced but the constant current has lost all influence, years will barely suffice to restore any action; to do this other means, as the unusual employment of potent drugs, becomes necessary, and then the result is problematical.

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* Certain conditions of the genitals will form the subject of a future lecture.

* In an article published by me last year in "St. Thomas's Hospital Reports," I have proved very clearly that infantile paralysis is no central disease—neither congestive, inflammatory, nor nutritive.

As to the mode by galvanism I shall say in this place very little, as by relating the details of a case —somewhat rapid in its success, it is true—all essentials may be taught. Certain facts and conditions only need be pointed out—firstly, the necessity of having all the instruments in good order and free from rust; secondly, of never using more power than absolutely necessary, and, especially with children, avoiding pain or alarm as much as possible; thirdly, since every muscle has a point where even a feeble current may cause contractions, while everywhere else a current strong enough to produce severe pain will have no effect on the organ, these points should either be so studied that there never is a blunder about the place to be touched, or else the practitioner must construct a chart, and, while using galvanism, have it in front of him.* Lest the term "constant current" mislead, it will be well to explain that the muscular contraction does not depend on the quantity or intensity of the current at any particular moment, but on alternations and amount; hence the twitch only takes place on closing, occasionally also breaking the circle, and not while the stream is flowing uninterruptedly through the organ. Constant current" is therefore a misnomer; the word should have been "direct current," had not such term already been appropriated to another use.

Emma T——, aged four months, was paralysed during the night of May 10th, 1870. On the 15th she was brought to me—i. e., about a hundred hours after the occurrence. The child was put to bed perfectly well on that night. She had cried a little unusually on being bathed, which the nurse attributed to sleepiness. She also seemed well the next morning, save that the left leg was completely paralysed, but was in nowise either tender or painful. I found no tenderness anywhere; no heat, thermometric or otherwise; no sign of ill-health or disease. She is rosy, laughs easily, and has evidently suffered nothing. Previous to my seeing her the patient had had an emetic administered, and had been purged. The excretions are in perfect order.

I tried faradaism gradually, increased to the limits of the bearable; neither with the primary nor with the secondary current could the slightest twitch be produced. The battery current with fifty cells produced no movement. In sixteen hours I tried the battery current again; and a third time in twenty-four hours more—i. e., 140 hours after the attack. On the last occasion slight movement in the flexors of the leg, and a more doubtful movement in the extensors, was produced.

May 24th.—Every twenty-four hours each muscle has been separately galvanised. The flexores cruris—the deep layer at the back of the leg—act energetically to thirty cells; the extensors both of leg and foot, and especially the adductors femoris, respond more feebly to fifty cells. The sural muscles do not act at all, but let the current go right through them to the deep layer without showing a sign of life. There is neither voluntary power nor faradaic irritability in any muscle. An elastic band is fixed to the shoe, so as, without restraining it, to keep the foot more or less at right angles to the leg. The electric occurrences are these:—At an uncertain time, during the first 100 hours, faradaic irritability ceased; at the 120th—130th hour galvanic irritability in certain muscles commenced, and has increased. Other muscles then became galvanically irritable; and now all respond more or less, except the sural muscles. Prognosis guarded, but by no means bad.

June 3rd.—The muscles are now all sensitive, except the sural; these, though they respond slightly to the current, are sluggish, and act but little.

July 14th.—The response of all the muscles to galvanism has been becoming more feeble and uncertain. This day Stöhrer's battery was used; and I was pleased to find all the muscles respond, and surprised to find that the sural muscles, which never acted well to the constant current, and had also like the others refused to answer to faradaism, responded now to the coil as well as any. The prognosis now given was perfectly hopeful; patience and perseverance all that is required.

Dec. 1870.—I took occasion to examine this child. The leg is smaller than the other, and about one-third of an inch shorter; but she can move it in every direction; and in walking or running the limb is barely perceptible. I ought to say that elastic force was, during three months, used on one side of the leg to prevent a threatening deformity; but this part of the case belongs to another division of our subject.

Original Papers.

REMARKS ON PRE-SYSTOLIC CARDIAC MURMUR.

By A. Whyte Barclay, M.D. Cantab.,

Physician to St. George's Hospital.

Two valuable papers in the last volume of Guy's Hospital Reports having been chiefly devoted to the discussion of "pre-systolic murmur," perhaps a brief consideration of it may be useful, as the ideas of medical men are not generally very clear on the subject, and it is one which I believe may admit of a different interpretation from that which is usually given regarding it. To Dr. Gairdner the profession is indebted for first calling attention to this particular murmur, and associating it with the pathological condition which usually accompanies it. The explanation given by him has been very generally adopted; and, though differing from him in opinion, I should hardly have ventured to oppose the views of so distinguished a pathologist on speculative grounds alone, supported as he is by Dr. Peacock, Dr. Wilks, Dr. Hilton Fagge, and others, who have all subscribed to the doctrine that the murmur is "auricular systolic," as it was first called by Dr. Gairdner. The substitution of the term "pre-systolic" is, to my thinking, a mistake on the part of those who hold this view. The name carries with it an idea very much at variance with their theory of its production. Like post-systolic, it suggests a murmur accompanying, in the main, the systole of the ventricle, though in time dating a little earlier than the ordinary systolic murmur, just as the post-systolic murmur begins a little later. So long as such murmurs are recognised as systolic in character, the prefix "pre" or "post" is not disadvantageous, as modifying in some degree their essential character; but when, as is assumed, the murmur is not systolic at all, some other term would convey a truer import. Those of us who have to teach medicine systematically to students must think and speak clearly and teach dogmatically. For some years I have been in the habit of laying down to my students as a principle, that, when they have to examine a heart, their first question must be whether any murmur heard is produced by blood flowing into or out of the ventricle, and next by what inlet or outlet the blood passes. Until the mind is satisfied on these points, the diagnosis must be vaguè and imperfect in the extreme.

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* A number of charts in Ziemssen's work—although they contain, I think, some errors—would answer the practitioner's purpose extremely well.

Another point to which, as an old observer of cardiac disease, I attach some importance, is the maintaining a distinction between sounds and murmurs—restricting the word "sound" to those which we recognise as the first and second sounds of the heart in health, and employing the word "murmur" to distinguish an adventitious noise which may accompany or be substituted for either of the natural sounds of the organ, or may occur intermediately between them. The French name "bruit" ought to be similarly restricted, and, in my judgment, ought not to be used to express, as is done by French writers, some change which has taken place in the natural sound itself. It may be a question whether in any given case a prolonged first sound is an altered "sound," or is a "murmur" substituted for the "sound"; but we can have no hesitation in saying that a reduplicated sound is not a "murmur."

I have been led to make these remarks by finding Dr. Hilton Fagge, in his paper in the Guy's Hospital Reports,* offering two alternative explanations of the same phenomenon, which he says may have been either "a second sound" or "a pre-systolic sound." In such a relation the significance of the word is sufficiently plain, but it is unfortunate when, in discussing nice questions of diagnosis, words are used in either a vague or an improper sense. The reduplicated sound is most commonly called a "bruit" in France, and I have known the same name applied to it here, much to the perplexity of students, who cannot make out whether the so-called "bruit" is systolic or diastolic, obstructive or regurgitant. It is of great importance that language should be clear and intelligible, and the more certain our knowledge the simpler always will be the terms in which it is expressed.

The case which I am about to relate is by no means the first which has led me to call in question the usual explanation of the pre-systolic murmur, but it seems to me the most convincing which could be offered, and in this respect alone is worthy of being placed on record, as I can personally vouch for the accuracy of the report.

J. G. P——, aged eighteen, was admitted under my care into St. George's Hospital on the 5th July, 1871, suffering from hæmoptysis. He stated that he had had a slight attack of blood-spitting in the previous summer, but had enjoyed good health from that period until about a fortnight before his admission. After taking exercise he had brought up nearly a pint of bright-colored frothy blood, and had continued to spit it up in small quantities till he came to the hospital. He was a rather dark, sallow-complexioned lad of apparently feeble health, with considerable dyspnœa, a flushed face, though otherwise rather bloodless, with a pulse of 120, exhibiting very little force or fulness, and manifest increased action of the heart. On auscultation a very distinct harsh grating murmur was heard at the apex of the heart, which distinctly terminated in the first sound, the interval between the second and first sounds being preternaturally long. On cross-questioning it was difficult to make out how long he had probably suffered from cardiac disease, but he was very positive that he had never had rheumatism or rheumatic fever; the only illness he could recollect being an attack of scarlatina when young, but at what age he was unable to say. When first admitted there was a trace of albumen in the urine, which disappeared under treatment. He was ordered fifteen minims of tincture of digitalis with some ether every six hours.

Little or no blood was brought up after his admission. His pulse gradually fell to 76, but was very easily accelerated on the slighest exertion; it was essentially weak, while the heart's action was forcible, though much less easily excited than on admission. There was no evidence beyond this of the influence of the digitalis, which was continued for about ten days without producing any unfavorable symptoms. Tonics and food were then cautiously administered, and he seemed to gain blood and strength. No change was observed in the heart's sounds at this period; but the pulse fell as low as 56 about a week or ten days after the digitalis was left off, in consequence apparently on his being kept perfectly quite, and the horizontal posture being constantly maintained. On auscultation of the heart, the length of the interval between the second and first sounds was very remarkable; and it was observed that the contraction of the heart could be felt between the ribs during the pause prior to the occurrence of the first sound, and while the pre-systolic murmur was audible to the ear.

On the 3rd August, some days after the rules regarding rest had been somewhat relaxed, he was again attacked by slight hæmoptysis. The pulse had gained in frequency; the rough grating murmur was very much what it had been on admission, preceding and terminating in the first sound. The duration of the second interval was not so marked as it had been when the pulse was slower; still I could not divest myself of the feeling that the murmur coincided with the commencement of the systole. He was again placed under the influence of digitalis, and on the 7th August his pulse had fallen to 40 beats. On listening to the heart's action, the same peculiarity was observed which had been noted some days before, only in a more remarkable degree: the second interval had become lengthened in a manner which I have never before observed. The action was perfectly regular. The duration of this interval was most striking at the base of the heart, where the murmur was only indistinctly heard. Towards the apex it was noticed that the murmur was much less harsh and grating than formerly and on careful examination it was found that it was no longer single, but double; and that a short soft murmur began almost immediately after the second sound, which terminated before the harsher murmur commenced. The latter, somewhat modified in character, still occupied exactly the same position in time with reference to the first sound which it had done from the beginning—namely, it ran up to, and terminated in, the first sound. In fact, at the apex the long interval between the second and first sounds was entirely occupied by a double murmur, the first part being soft and short, the last harsh and prolonged. This was immediately followed by a short and sharp first sound, and an interval of silence of about the usual length, though possibly also a little shortened, during which no murmur was anywhere audible. The second sound always preceded the doubled murmur. At the base the double murmur could not be heard at all; the second sound was there clear and sharp, followed by an interval of complete silence before the harsher murmur made itself heard. A double murmur at the apex is so rare that it is always a subject of unusual interest, and I can state with confidence, both from my own observation and that of others, that the facts in this case are accurately recorded. I am certain that it consisted of two distinct murmurs, separated by an interval from each other, not of one murmur of different intensity at its commencement and termination; it was double and not continuous; the two murmurs being each audible at the apex, one of them alone audible at the base.

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* Guy's Hospital Reports, vol. xvii., pp. 311, 312.

The further history need not be traced. He went on after this date with no bad symptoms, and left the hospital considerably improved in general health. It would have been imprudent to have continued the administration of digitalis after the pulse had fallen to 40 beats, and as soon as it was left off the heart's action regained its ordinary frequency, and the murmur resumed the characters which it had presented on admission. There was no call for the renewal of the action of digitalis, and as the rhythm of the heart's sounds and murmurs had been clearly made out, I did not feel justified in resuming its use merely for the purpose of experiment.

In order that the discussion may not degenerate into a mere question of the meaning of terms employed by various writers, and to facilitate the understanding of the proposition I wish to prove, I would venture to lay down certain axioms which must be admitted by every one whose ear has been trained to recognise cardiac murmurs, and I would also ask the acceptance of a few postulates which seem to me to be essential to the discussion of this question.

1. The first and second sounds correspond to the systole and diastole of the ventricles, the movements of the auricles not being indicated by either sound.

2. Systole is equivalent to contraction, diastole to relaxation.

3. Blood must flow out of the ventricle during contraction, into it during relaxation.

4. The systole and diastole of the two ventricles are simultaneous in health, each producing only one sound.

5. The impact of the heart on the ribs, the first sound, and the wave in the aorta which originates the pulse, are synchronous. The greater the distance from the heart where the pulse is observed the further does it fall behind the first sound.

I cannot conceive any of the above statements being doubted, and I would further ask that it should be conceded—

6. That the sounds of the heart do not indicate either the commencement or the termination of the systole, the diastole being regarded as a merely passive state—the absence of contraction. It is quite impossible that the first sound and the impact on the ribs can occur until after contraction has proceeded to some extent; and similarly the ventricle must have come to a complete stand still, and a backward current must have commenced, before the second sound can be formed.

7. There can be but two states of muscle—contraction and relaxation. It would seem that there is or may be a fraction of time during which the entire muscle is relaxed, but "*le grand silence*," as the French choose to call it, is not a period of relaxation—the auricle is then busiest, and the ventricle begins to contract before it is concluded. The period of complete relaxation, if such there be, must be that which immediately procedes and accompanies the second sound. The second sound itself is the indication that blood is rushing towards the ventricle, forced backwards by the resiliency of the aorta—forced onwards by the columns of blood flowing from the lungs, a force which is immediately supplemented by the contraction of the auricle. When the ventricle is full the onward current through the mitral stops; then the systole begins, and before its termination the first sound occurs. As soon as the ventricle is empty, relaxation takes the place of contraction, and this is almost immediately followed by the second sound. The active dilatation which the older observers used to speak of is nothing more than the flaccid ventricle being pushed open like an empty bladder by the contraction of the auricle.

8. In perfect health the contraction of the two ventricles is simultaneous, and we hear only one first and one second sound. Sometimes, however, when there is no valvular imperfection, the contraction of one side is sustained a little longer than the other, and the blood begins to flow towards one ventricle a trifle sooner than towards the other, and the aortic and pulmonic valve-flaps fall together at different instants of time, so as to produce two second sounds. This presents the simplest form of reduplication, one first sound followed by two second sounds. More rarely the contraction of the two ventricles is not simultaneous, and two first sounds result, followed of necessity by two second sounds. We have thus either reduplication of the second sound or reduplication of both sounds. I have never heard reduplication of the first sound only, and conceive it to be, according to physical laws, impossible.

9. In general terms it may be said that sounds depend on the movement of the solid parts, murmurs on the movement of the blood itself. The murmur is a churning noise produced by vibrations of the fluid, in whatever manner these are excited; while the sound seems to be developed by the contact of portions of tissue which were previously apart. This is only true in a very general sense, and, as a matter of course, the sounds cannot be produced in the heart without the presence of fluid, and the blood can only be thrown into vibration by contact with the solid tissues. Still it helps us to understand the relation of sounds and murmurs, and that the murmur sometimes takes the place of the sound, sometimes is heard along with it.

10. The rhythm of the sounds is liable to great variations. The intervals are certainly very often prolonged or shortened, and, to my ear, the sounds themselves seem to be occasionally lengthened or shortened, and variously accentuated Cases must have come before all observers of cardiac disease, in which, from such changes having taken place, the first sound has been mistaken for the second, until the impression conveyed to the ear has been corrected by observation of the impulse.

If I might venture to express an opinion on such an obscure question, I would say that this alteration points rather to a shortening of the first sound than to a lengthening of the second, which is not very probable, if the ordinary interpretation of its causation be correct. It is, indeed, just possible that in a reduplicated second sound the two may run into each other in such a manner as to give the impression to the ear that the two short sounds are only one long one. In either case the following pause will generally be altered in its duration in an inverse ratio to the change in the sound. If the first sound be shortened, the first pause will naturally be lengthened; if the second sound be lengthened, the second pause will be correspondingly shortened; and thus, while the sequence is the same, the rhythm will be modified either to the extent only of making each sound and interval equal, or so far as to make the two appear to have changed places. It must be added that the extreme form is seldom met with except in cases of valvular imperfection, when murmur is also present, which makes the appreciation of such changes far more difficult.

In another way, similar changes seem to me to arise from a transposition of the first sound, with reference to the systole, from an earlier to a later period in the contraction of the ventricle. The sound is believed to be due to several causes; but one at least has been determined by actual experiment to contribute to its production—namely, the flapping together of the mitral valve, as was shown some years ago by Dr. Halford. His observations seemed to me to prove two things—that a sound must follow the closure of the valve, and that the sound so produced was shorter than the real first sound of the heart, which must therefore be dependent on other causes also operating at the same time. If, then, it were possible that this closure of the valve should take place either earlier or later in the course of the systole, we should have a lengthening or shortening of the two intervals as a necessary result of the transposition, and also not unnaturally a change in the accentuation and duration of the first sound itself, in consequence of the various causes of its production failing to be coincident, as they usually are, and the short sharp sound of the valve-flaps being heard separately. Assuming for the moment that circumstances might arise which could prevent the mitral valve from closing at an early period of the systole, it is clear that a murmur of regurgitation must almost of necessity be developed, which would interfere with our hearing the first sound distinctly. The various elements which contribute to the formation of the sound being separated by the postponement of the closure of the valve, it is highly probable that the shrillest and loudest part of the sound would alone reach the ear. This, so far as I know, is the flapping together of the two parts of the valve; and if, as we have assumed, something has delayed this occurrence we shall have an apparent lengthening of the interval and postponing of the first sound, with a shortening and sharpening of the sound itself, because the other elements which contribute to its formation, whatever they may be, are lost in the noise of the murmur, and it is represented only by the click of the valve. A marked alteration will thus be traced in the rhythm of the sounds and intervals; the first sound short and sharp, followed by a somewhat shortened interval; the second sound occurring about the usual time with reference to the impact of the heart on the ribs, and having a normal length, but followed by an unusually prolonged interval before the first sound again recurs. If due attention be paid to the movements of the heart as tracable by the finger, the hypothesis of a postponement of the first sound in such cases must, I think, be admitted as fairly probable. Sensible motion is developed long before the first sound can be heard; and so long as any such movement can be traced, it is certain that contraction is going on, even though during its progress no sound reaches the ear to show that the systole has commenced, except, as I shall endeavor to show, the murmur supplies the evidence which is wanting.

Bearing in mind the foregoing axioms and postulates, let us now consider the relation of the murmur to the movements of the heart. Every murmur is due to vibrations in the blood produced by motion. A valvular murmur is a vibration excited by some alteration in the condition of the valve, as contradistinguished from vibration due to changes in the condition of the blood itself. We except two classes of valvular murmurs—obstructive and regurgitant, as they are called: the one caused by the blood, in following its natural course, flowing over a rough channel; the other dependent on its flowing backwards and meeting an opposing current. If the opening through which it regurgitates happen to be also rough, the vibration will be intensified; but roughness is not essential. At the aortic valves, for example, imperfect adaptation, in consequence of a rent in the valve, may be so free from roughness that no systolic murmur is heard, and then the diastolic murmur will not be perceptible if listened for over the region of the valve, but will be perfectly unmistakable over the centre of the heart, where the two currents meet. But if the valves be rough, as well as unable to close completely, there will be a systolic murmur present, and the diastolic murmur will be louder, and traceable from the valve over nearly the intire cardiac region.

There is, however, another consideration involved, which is to my mind of the first importance in regard to the production of murmur, and is too often overlooked—namely, the force with which the blood is moved. The greater that force, the more easily is vibration excited; and this whether the murmur be obstructive or regurgitant. Anyone acquainted with the very elements of hydrostatics will admit that in proportion to pressure must be the perfect adaptation of the resisting sluices to prevent leakage; and exactly in the same proportion is the chance of any obstruction to a current exciting vibration in the fluid. This at once gives the explanation of the fact familiar to all auscultators, that an obstructive murmur at the base and a regurgitant murmur at the apex are the two most commonly heard. They are both systolic—both produced by the blood being impelled against the valves with the whole force of the ventricular contraction. The regurgitant murmur at the base, when due to roughness of the valve, and the obstructive murmur at the apex, must each indicate a greater degree of alteration of the respective valves, as they are produced by forces relatively weaker. I can scarcely conceive a mitral valve so altered by disease that it should excite audible vibration in the onward current, while yet remaining capable of closing so completely as to be unattended by regurgitation. If the roughness be such as to produce vibration when the blood is propelled only by the comparatively powerless action of the auricle, how can it resist the impact of that fluid when driven against it with the whole force of the ventricle?

I need scarcely add that, if we suppose both muscles contracted together, the direction of the current must be from the ventricle, and not to it. Yet it is a point of some importance when we consider the present view taken of the contraction of the heart—viz., that, like other involuntary muscles, it is vermicular. Commencing at the base, it gradually passes down to the apex in a wave-like manner, followed by a similar process of relaxation; and, inasmuch as the fibres forming the auricle are closely associated with those encompassing the ventricle, the two cavities do not contract and expand alternately, but the contraction of the one somewhat overlies that of the other. In ordinary circumstances this is of no consequence, because the mitral valve resists the return of the blood; but when the valve is much damaged, the more powerful contraction of the ventricle must overcome that of the auricle, if there be any such thing as antagonism between them. For my own part I should think it more probable that the blood is simply driven through the auricle, when in a state of contraction, into the veins until the backward current is arrested by the forward current pressing onwards from the lungs.

The absence of any special valvular apparatus at the entrance of the pulmonary veins has a very important bearing on this question. With no means of closing the outlet in that direction, it is plain that the auricle can exert really very little power in driving the blood forwards, and that in contracting it must empty itself backwards if any obstacle to its onward flow exist. It is not improbable that hypertrophy may be produced by such a state of things; but in its extremest form the auricle never acquires the thickness of even a thin ventricle, and I much suspect that the thickening of its walls is rather due to a large quantity of blood being driven back into it than to any obstacle to its onward flow. The hypertrophy does not so much represent what used to be called the concentric hypertrophy seen in obstruction of the current through the aorta, as the hypertrophy with dilatation seen in aortic regurgitation.

I venture, with great deference to the opinions of such men as I now find ranged as supporters of Dr. Gairdner's theory, to think that it will not stand the test of a rigid examination, but is quite untenable if the statements just made fairly represent our present knowledge of cardiac sounds and murmurs. The name which he adopted for this murmur, "auricular systolic," sufficiently indicates the idea he had formed of its causation. It is admitted by all who have written upon the subject that his description was a correct one of a rough murmur running up to and ending in the first sound; and it has been assumed, probably rightly, that only one murmur ever presents these characters; while post-mortem examinations seem to have proved that one special pathological state is constantly or invariably associated with it. Starting from the period when the diastole is supposed to be complete, a portion of the second interval in the heart's sounds has passed in silence when the rough murmur begins, which is uninterrupted until the first sound is heard as its conclusion. The vermicular movement of the muscular fibres of the heart has already begun at the base, some time before the first trace of this murmur is audible. The contraction has probably travelled downwards as far as the ventricle before the half of the pause has elapsed; for the stroke against the rib is the termination of this vermicular movement, and scarcely a perceptible interval, if any, separates the first sound and the heart-stroke. Can it be accepted as probable that the auricle keeps driving the blood through the mitral valve in opposition to the contraction of the ventricle? I am quite sure that Dr. Gairdner holds no such belief. He assumes, however, that the contraction or systole only commences with the first sound. This I conceive to be a mistake. The stroke and the first sound cannot take place till the whole muscle is thrown into a state of tension, and the systole begins when the first fibre assumes this state, persists so long as any portion remains contracted, and only ceases when the blood has been expelled from the cavity and relaxation once more takes the place of contraction. I contend that it is unreasonable to assume that blood can keep pouring into the cavity after the contraction of a single fibre has commenced; and still more that its flow should be uninterrupted and unimpeded until the stroke against the ribs shows that the whole muscle is in a state of contraction. I hold that the muscular tension is at its period of greatest intensity when the stroke is felt, even though it be admitted that a subsequent shortening of each fibre occurs while the blood is being expelled and the cavity diminishing in size. Here probably lies the fallacy. The term "contraction" is used in a double sense as meaning both tension and shortening, and it may perhaps render my meaning more distinct if the sequence be thus stated:—The tension of the ventricular fibres begins during the longer interval, and is complete when the impulse is felt on the chest-wall; but the shortening of the fibres goes on during part of the shorter interval, and ends before the second sound can be produced. The whole of this period is embraced in the term "ventricular systole," the first sound occurring somewhere in the middle of the contraction—at the point, in fact, where tension is complete and shortening begins.

It has been already pointed out that the contraction of the auricle must extend somewhat into the beginning of the tension of the ventricle—a weaker tension overlying, as it were, a more powerful one; and in connexion with this it has been noticed that there is no very efficient obstacle to the return of the blood into the pulmonary veins. It is, consequently, scarcely possible that one of the loudest and roughest murmurs ever heard in cardiac disease should be produced by the contraction of the auricle, even though hypertrophied to some extent. It is clear to my mind that if the obstruction be really of such moment as is alleged, the blood would chiefly flow backwards into the lungs, and the murmur produced by its passage through the valve would be soft and of small intensity. That very considerable disease exists, and that the murmur is a very harsh one, there can be no doubt; but the idea that it is produced by the forward current of the blood does not seem to harmonise with the ordinary laws of physics. On the other hand, I cannot conceive the diseased valve offering due resistance to the impact of blood when the ventricle contracts; and, as already pointed out, it is not probable that an amount of roughness of the valve should exist sufficient to produce so loud an obstructive murmur, while no murmur of regurgitation is traceable. The probability seems to be all in favor of its being a murmur of regurgitation, and not one of obstruction. If it be conceded that the tension of the fibres of the ventricle begins before the first sound is heard, no other interpretation can possibly be given. The essential character of the murmur is, that it runs up to, and terminates in, the first sound; and, consequently, it must be produced while the tension of the ventricle exists. The necessary result of such tension is that the blood is forced against the apertures, and in the ordinary course of events the mitral aperture would close; but the contracted hardened valve is not capable of ready adaptation, and therefore offers no efficient obstacle. The contraction of the auricle does not prevent the blood from being driven through it, as it meets with no resistance at the inlet of the veins. The unavoidable result is regurgitation, with its necessarily attendant murmur, rendered all the more harsh by the roughness and hardness of the opening. I cannot think that the very soft murmur which is sometimes heard after the first sound in such cases can be regarded as the true regurgitant murmur, corresponding to the very rough obstructive murmur which has been heard before it; yet such is the explanation usually given. Not that I would question the regurgitant character of the softer murmur, but that I feel a need to have some reason assigned why an obstructive murmur produced by the passage of blood over the mitral valve should be loud and harsh when impelled forwards by auricular contraction, while the vibration of the blood when driven backwards by the whole force of the ventricle against the opposing current should be often inaudible, and never elicit a murmur of much importance.

I am not quite sure that I understand the ideas entertained by some observers with reference to murmurs audible both before and after the pre-systolic murmur, and usually described as diastolic and systolic. Taking the statements of one of the most recent writers on this subject,* I find that he refers to the diastolic as being divided by an appreciable interval from the pre-systolic murmur, "the direction of the current of blood being the same during both" and through the same valve. I also find him speaking of the pre-systolic and systolic murmurs as "a somewhat prolonged murmur, the first portion of which is rough." &c. It would appear that Dr. Balfour believes a double murmur may be due to a continuous movement of the blood in one direction, while a single prolonged murmur may be produced by blood flowing first onwards and then backwards through the same valve. For my own part I cannot adopt such an interpretation, and I must confess that all the theories in that paper are very perplexing. The diastolic murmur in Case 4 is said to be most distinct on the *left* side of the sternum between the second and third ribs (p. 440), and at p. 442 is referred to the *right* side of the sternum, the "region of the pulmonary artery." Why the post-mortem revelation of granulations on the aortic valves is rejected as affording the natural explanation of this diastolic murmur, and "mitral stenosis" is credited with its production, does not appear.

Dr. Peacock, in his article on this subject, refers to two distinct cases as being both dependent on mitral obstruction: in one a loud systolic murmur at the apex "is immediately preceded by another murmur, so as to constitute in that situation a distinctly double murmur"; in the other "there is a rough harsh murmur, which clearly commences before the systole and terminates abruptly with it."* No one will hesitate to believe that the first fairly represents the ordinary but somewhat rare condition of obstructive and regurgitant mitral murmur occurring in the same individual; the second being an example of the pre-systolic murmur which Dr. Peacock classes as mitral obstructive, or, as Dr. Gairdner would say, "auricular systolic." He does not attempt any further explanation of the murmur, and, substituting the word "systole" for first sound, overlooks the possibility—nay, I would say certainty—of the tension of the muscular fibre having preceded the first sound, and being really coincident with this pre-systolic murmur.

Dr. Sutton's description, if accurate, as I believe it to be, carries with it the condemnation of the theory which he, in common with others, has adopted. He says † "it was a harsh, grating murmur, which occupied a great part of the period of silence, and ended abruptly as the heart's apex struck against the walls of the chest." The muscular contraction does not begin when the stroke is complete. If the muscle of the ventricle be in a state of tension, the blood cannot be flowing into the cavity.

Neither Dr. Wilks nor Dr. Hilton Fagge ‡ enters into the theory of the production of the murmur. They simply assent to Dr. Gairdner's view, and acklowledge the debt we owe to his careful investigation, which proved the association between the peculiar pathological condition of the mitral valve and the murmur under discussion.

The case which has been already given seems to carry us one step in advance in the attempt to explain the attendant phenomena. When the action of the heart was reduced to forty strokes in the minute, I was able to detect a soft murmur beginning after the second sound (post-diastolic) and terminating just before the harsh murmur began, which ran up to, and finished with, the first sound (pre-systolic). Taken in conjunction with what is known of the muscular movements of the heart, we are able to say positively that the first of these coincided with the tension of the auricular fibres and the relaxation of the ventricle, and was therefore produced by the onward current of the blood into the ventricle; and it seems equally clear that the second coincided with the tension of the ventricular fibres which precedes their shortening, and, consequently, must have resulted from blood flowing out of the ventricle, although the first sound had not yet been heard. The position of both murmurs, with reference to the apex-beat, proves clearly that the mitral valve was the seat of their production in each case. Still a great difficulty remains to be surmounted, in the fact that the pre-systolic murmur is arrested long before the ventricle has emptied itself of blood, and is not continued, as mitral regurgitant murmurs usually are, to the end of the systole. The reply must be purely hypothetical, but is closely related to the pathology of the valve in cases of pre-systolic murmur. Indeed, the idea first occurred to me in consequence of a remark made by the curator of our Pathological Museum, Dr. Whipham, when we were discussing the possible causation of pre-systolic murmur some time prior to the occurrence of the case related.

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* Clinical Lectures on Diseases of the Heart. By Dr. Balfour. Edin. Med. Journ., vol. xvii., p. 431.

* On the Diagnosis of Obstructive Disease of the left Auriculo-Ventricular Aperture. Med. Chir. Rev., vol. xl., p. 489.

† Remarks on Tricuspid, Regurgitant, and Mitral Pre-systolic Bruits.—London Hospital Reports, vol. iv., p. 301.

‡ Guy's Hospital Reports, vol. xvii.

If we contrast the state of the valve in the ordinary systolic mitral with that in the pre-systolic murmur, we find that in the one the valves are quite free to close, the edges being roughened by deposit upon, or outgrowth from, their surface; while in the other the valve is transformed into a ring, prolonged into a funnel-shape, with a thick inflexible margin, the two flaps being more or less adherent to each other. When the blood is forced towards the aperture in the systole of the ventricle, the flaps fall together almost as in health in the former of these conditions; and just in proportion to their want of adaptation does it happen either that the first sound is itself modified, or that the systolic murmur begins earlier or later, while the contraction is slighter or more decided, the force called into play less or more powerful. The greater the want of adaptation the earlier does the valve yield, the less is the force required to overcome its resistance. In the latter form of disease the conditions are completely altered. The thickened rim cannot close in the ordinary way, it stands almost permanently open, and the first effect of ventricular tension is to drive blood through the valve, not to close it. But, as contraction goes on, the blood presses on the sides of the funnel, and impels them against each other; and thus the click of the valves is delayed, because not the edges but the sides have met—not the earlier tension but the later contraction of the ventricle has closed them. As soon as they meet they must more or less arrest the backward current, and we have thus the explanation of the circumstance that the murmur runs up to, and terminates in, the first sound. In other words, the blood is driven through the patulous valve by the earlier tension of the ventricle, but is arrested by its further contraction, when the sides of the valve strike against each other and form the only part of the first sound which reaches the ear, everything else being obscured by the noise of the murmur. This, too, explains why the interval before the first sound is longer than usual, because the closure of the valve has been delayed, and the first sound has been thus far postponed. The noise of the murmur is so great that it is extremely difficult to make out whether it is in all cases followed by a mitral systolic murmur. That it is so in many instances is acknowledged by those who have most studied the subject; and if cases occur in which it is certain that no murmur follows after the valve has closed, they must be those in which the funnel is somewhat elongated and the sides come into close adaptation under the pressure of the ventricular systole. This is probably the correct explanation of the continuous murmur referred to by Dr. Balfour, which he assumes to be caused by blood flowing in opposite directions, because its first part is rough and harsh, the second soft and blowing. While the hard ring stands wide open, the blood is driven forcibly through it, and perhaps right through the contracted auricle, with a loud noise, as its current mingles with that coming from the lungs; presently the sides of the valve come together with a click, and the incompetence of the mitral is reduced to a minimum; only a small leakage takes place, and this minute quantity of blood, no longer meeting an opposing current, which has been already brought to a stand-still, produces only a very slight murmur. Hence the first part of the murmur is harsh and loud, the second slight and soft, with the click of the valve-flaps as the only representative of the first sound of the heart interposed between them.

The absence in such cases of an obstructive murmur such as might indicate the onward flow of the blood, if this be a murmur of regurgitation, is just what *a priori* might have been anticipated, and is what we constantly observe in other forms of mitral regurgitation. The experience of all observers of cardiac disease may be safely appealed to whether it has not often struck them with surprise that a degree of roughness of the mitral valve almost unknown at the aortic aperture had led to no obstructive murmur at the apex, when it would have necessarily produced one at the base of the heart. If my memory serves me right, a musical murmur dependent on a long fibrinous coagulum is, at the apex, always systolic—regurgitant, not obstructive. The double murmur at the apex is certainly a rarity at the bedside. Such facts, taken in conjunction with the normal relations of the auricle, suggest the extreme improbability of any loud and rough murmur being produced by its contraction. Its power to overcome any obstacle is so small that, when called into play, the natural, the necessary result of obstruction at the auriculo-ventricular aperture must be that the blood is driven back on the lungs, and that pulmonary congestion and hæmoptysis should follow. This was the result in the case of J. G. P——, and is constantly referred to by writers on the subject.

The explanation of the phenomena usually given is, that the blood accumulates behind the obstruction, and, incidentally, the action of the auricle is supposed to be delayed, in order to account for the length of the interval between the second and first sounds of the heart. It may be observed, in passing, that an obstructive aortic murmur does not give rise to any similar sequence; but much stress cannot be laid on this argument, because the circumstances are somewhat different, the integrity of the mitral valve in such a case saving the lungs from being very materially damaged, however much the current may be obstructed through the aorta. Such considerations only bring us back to that on which perhaps too much stress has been already laid: that there is no hindrance to the backward current through the pulmonary veins. The hæmoptysis thus finds a ready explanation, not in any passive accumulation of blood behind an obstruction, but in the backward flow, whether dependent on auricular or ventricular systole. In the ordinary mitral systolic murmur the congestion of the lung is almost a constant state; and, if my view be correct, the pre-systolic murmur would lead to the same result with even greater constancy.

In conclusion, let me repeat that we owe a debt of gratitude to Dr. Gairdner for having called special attention to this point in cardiac pathology, and that, in differing from him, and from many others whose position as clinical observers claims my highest respect, I feel that no other apology need be offered than an honest conviction that their theory will not explain the phenomena observed. In substituting another hypothesis for that which has been rejected, it must be remembered that it is only suggested as one deserving of consideration. It appears to me to meet all the requirements of the case, but to others it may seem inadequate. I commend it to the impartial consideration of those most competent to form an opinion on the subject. *Magna est veritas et prævalebit.*

Bruton-street, March, 1872.

CARDIAC INTERMITTENCY.[*]

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Cardiac intermittency is the last and most serious rhythmical disturbance of the heart, and is a symptom ordinarily of very grave import. It is evidence of great disturbance in the balance of forces by which the heart is regulated. This disturbance may occur in any of the three modes previously mentioned.[†] It is unnecessary to go over them again, and at present we will regard the symptom rather as it manifests itself clinically. Before thus leaving the question of causation it may be desirable to consider the question of the immediate mode of its production. Dr. B. W. Richardson, in his paper in the St. Andrews Graduates' Transactions of 1870, states that the arrest of ventricular action lasts over the period of a second auricular contraction; and further states that the distension produced by the second auricular contraction excites the ventricle to contract. When first read, this statement appeared to the writer as ingenious but totally unproven. That it was probable was unquestionable, and that the laboring ventricle should need an unusual stimulus to excite it to contract was quite in unison with views expressed elsewhere. It is the case however. But even a second auricular contraction is not always sufficient, and three, or may be more, may be required in cases where the intermittency is marked; for intermittency is merely an aggravated form of irregularity, differing indeed only in degree. The proof of this the writer got quite unexpectedly in pursuing some experiments on the rat to test the time of auricular and ventricular contraction in the mammal. The rat having been carefully chloroformed and the chest opened, at first the contractions went on normally, the auricular contraction immediately preceding the ventricular contraction; but as life flagged, two auricular contractions were requisite to excite the less irritable ventricle, and finally three auricular contractions were necessary. The ventricle needed goading; and firstly a second auricleful of blood became necessary, and finally a third, to produce such distension as should excite contraction. As to the method of immediate production of intermittency, Richardson's views are sound. As to the conditions under which it occurs, the writer finds it impossible to agree with him. Having abandoned the view of its cerebral origin advocated by him in 1867, Dr. Richardson has taken up the view of its being solely connected with exhaustion of the sympathetic. This view is a great improvement on the first; and while admitting that cardiac intermittency is due frequently to exhaustion of the sympathetic, the writer feels compelled to express his dissent from the view of this being its sole, or even its usual, causation. The chart[*] already given, which was drawn up before the writer became acquainted with Dr. Richardson's later views, gives the modes of disturbance of balance, and the various methods in which it may arise. At present the question will be regarded with reference to its appearance clinically, and the conditions under which it is found. Giving them in the order of relative frequency, commencing with the most frequent condition, cardiac intermittency is found with—1, fatty degeneration; 2, muscular exhaustion; 3, exhaustion of the sympathetic; 4, influences, through or in the vagi.

1. *Fatty degeneration.*—With degeneration of the muscular structure of the heart itself is intermittency most usually associated. It is under these circumstances found accompanied by a feeble first sound and subjective symptoms of cardiac failure. Thus in the generally degenerate condition of the circulatory organs known as atheroma, where the fatty condition is a necessary result of imperfect blood-supply and consequent tissue-degradation, intermittency is very frequent. And in the aged, with rigid arteries, fatty cornea, &c., when, amidst a number of feeble heart strokes, the long halt of intermittency is detected, occurring almost rhythmically, a most grave prognosis will be found best suited to the case, and a too early verification of it will often be met. The wearied muscular fibres, constantly becoming more scanty and unequal to the distension of the rigid and inelastic aorta, and thus constantly tending to a less and less sufficient blood-supply for themselves, become more and more tolerant of distension till, finally, a slumber too prolonged for the continuance of the existence of the organism results in cessation permanently. Thus, when in serious dilatation the imperfect contraction, and consequently imperfect aortic recoil, leads to degeneration of the muscular tissue in addition to its elongation, amidst the flutter of irregularity will be found the ominous halt of intermittency. This is a point of great practical importance. Except in great exhaustion, the muscular fibre is never tolerant enough of over-distension to allow of intermittency unless its own integrity is interfered with. When, then, intermittency is found along with other objective and subjective symptoms of failure of power, it is a symptom of the greatest gravity.

2. *Exhaustion of muscular walls.*—Next in frequency is muscular exhaustion as a cause of cardiac intermittency. Exhaustion of the muscular walls is often closely allied to exhaustion of the sympathetic. But in cases of aortic obstruction, &c., there is no exhaustion of the sympathetic unless after effort, and then the intermittency is more frequent: it is a simple disturbance of balance of power between the muscular walls and the obstruction offered to the flow of the blood. The two

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[*] Palpitation, The Lancet, 1870; Cardiac Irregularity, 1871.

[†] See chart in The Lancet, p. 133, 1871.

[*] See The Lancet for 1871.

most marked cases of cardiac intermittency—i. e., the most prolonged halt—which have come under the writer's notice were both cases of aortic disease. In these cases the halt extended over a period of time equal apparently to three or four ordinary ventricular contractions. The exhausted muscular fibre claimed a long rest before it could be again goaded into activity. The sensations of the patients during this prolonged ventricular systole were of the most painful description. One is a man of great intelligence and of a nervous temperament. His account of his sensations is most affecting. On the most vivid sensations of instant death is implanted a sense of acute agony. A look of horror passes over his face when even mentioning his sufferings. All sensations which are connected with parts supplied by the sympathetic are exquisitely painful, and the over-distension and inability of the ventricle to contract seems to instantly pass all over the system. During periods of temporary irregularity of the heart's action the writer has had occasion to note the unpleasant sensation which accompanied the ventricular arrest. During the prolonged halt in these two cases this sensation amounts to agony. The frequent instantaneous halts in the walk of the aged, and the instinctive clutching or snatching at some one, or even anything which could afford support, which are usually found connected with some lesion of the heart, are now relegated in the writer's mind rather to the list of unpleasant sensations of sudden failure from ventricular inability than referred to sudden temporary cerebral anæmia. That this may be produced by sudden cerebral anæmia mediately as a result of ventricular failure it is not attempted to deny, but the cerebral anæmia and the sudden sensation of impending death, or other catastrophe, are alike results of ventricular inability. Relief by rest in bed, and the proper therapeutic measures by reducing the demand on the muscular walls (so many more beats per minute accompany the erect position, and still more exertion) lead to a reduction both in the frequency and duration of these ventricular halts. So, too, in simple dilatation associated with Bright's disease. During an acute exacerbation, when the blood is more than usually laden with effete products, and thus the obstruction to the flow of blood increased (the mode of this has been referred to in previous papers) on the ordinary symptoms of cardiac inability is implanted the most serious admission of incapacity—namely, intermittency.

3. *Exhaustion of the sympathetic.*—Exhaustion of the sympathetic is unquestionably no uncommon cause of cardiac intermittency, and will be found without any evidence of organic mischief; it is merely an expression of a passing condition. Thus after prolonged over-exertion, mental exhaustion, fatigue of any kind, withdrawal of nerve-force to some other organ supplied by the sympathetic, &c., cardiac intermittency will occur. Instances of this were given in the paper on Cardiac Irregularity. The most remarkable series of cases illustrating this mode of causation of intermittency are given by Dr. Richardson in his paper. The list comprises cases where this was occasioned by terror in shipwreck, anxiety, grief, passion, excessive fatigue, adversity, hereditary predisposition, &c. In all the cases there were no objective or subjective symptoms of cardiac mischief, and many, too, are corroborated by post-mortem observations. A more valuable addition to our knowledge of cardiac symptoms it is scarcely possible to conceive; for, in the spirit of originality for which he is so remarkable, Dr. Richardson has shown that in a number of cases the most serious and justly dreaded symptom of cardiac inability may be associated with a condition of the sympathetic from which there is nothing to be apprehended. But while admitting all the merit in the matter of the paper, and the brilliancy of illustration also found, I must decidedly disagree with him as to the relative frequency of the occurrence of cardiac intermittency along with and without organic disease. In this, too, I may fairly claim to be in unison with the experience of the profession; but Dr. Richardson has abundantly shown that intermittency alone is not sufficient to warrant a grave prognosis, or that it necessarily follows that there must be organic disease. As to its being a forerunner of cerebral disease, apart from such as might fairly be supposed to be associated with a degenerate condition of the circulatory organs, the writer has no experience, and has, therefore, no opinion to offer. That temporary conditions of cardiac intermittency are due to exhaustion of the sympathetic, and thus of the muscular fibre under its control, is unquestionable, and, further, is no rare condition, it must be at once allowed, and a very illustrative case is given by the writer in an article on Cardiac Distensibility, Distension, and Dilatation, in the *Edinburgh Medical Journal* for December 1870. In many cases, or even in most, the condition will be found associated with prolonged over-exertion, and then, no doubt, exhaustion of the muscular fibre itself is found along with exhaustion of the sympathetic. It may not be possible to separate them; but when so associated, cardiac intermittency will be found to vanish with the conditions which gave rise to it. When the condition is advanced, a considerable period of comparative rest and appropriate treatment may be necessary to complete restoration to health.

4. *Influences through or in the vagi.*—Of this last we know little, and the cases fairly referable to this cause of cardiac intermittency are, in the present state of our knowledge, rather curiosities than matter of practical importance. As, however, analysis of cardiac rhythmical disturbance would be obviously incomplete without a reference to this form, it is necessary to consider it. The most generally known cases are those of voluntary arrest of the heart's motion. Many cases are no doubt the creation of the imagination, but still well-authenticated cases are recorded. The Indian conjurors seem to have a monoply of this form of cardiac intermittency; but one East Indian officer who had acquired this power gave exhibitions of his attainment, and fell a victim to a prolonged effort from which he never recovered.

Another case of this power to arrest the heart by an effort of will is known in the person of a physician of this town, but a due regard for the value of his life and the hazardous character of the experiment has prompted him to decline any further manifestations of his peculiar power. Of cardiac intermittency produced by disease implicating the vagi little is on record, and for obvious reasons. One most singular case is quoted by Romberg* from Heine's book. A short *résumé* may tempt those interested in the subject to look up the case. A man in the Viennese Hospital complained that his heart stood still, and further, that this caused him severe suffering. The intermission extended over five or six beats. The patient's appearence "told that something terrible was going on within him." A vivid consciousness enabled him to tell that, after a presentiment of a second or so, he felt acute physical agony, which aggravated his mental tortures. The attack varied in frequency, and returned at irregular periods, mostly excited by mental emotions. The diagnosis was made by Heine, Skoda, and Rokitansky, and was in every way remarkable. It was to the effect that this was due to tumour of the vagus and hypertrophy of the cervical portion of the spinal cord. The man ultimately died, and a remarkable corroboration of the diagnosis is recorded. This case is, to my knowledge, unique. The connexion betwixt the affection being one of the vagus and its exacerbation by mental emotions tallies with the arrest of the heart's action by voluntary effort. In this voluntary arrest there is no reason to suppose that the effort of will could manifest itself otherwise than through the vagi, which are in direct communication with the sensorium. The cases, however, where cardiac intermittency takes its origin in influences through or in the vagi will be too rare to give them any practical importance.

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* Diseases of the Nervous System, vol. ii., p. 340.

Thus we see that cardiac intermittency, like the other objective symptoms of heart trouble, is connected with disturbances of balance of power; this disturbance being serious or otherwise according as it is in the muscular or nervous balances. The surroundings alone can determine its relationship, and any diagnosis otherwise founded is merely chimerical. The age, the sex, the history, and the habits must all be entertained; the diathesis is often of primary importance: and all must be carefully weighed in giving an opinion. The apparently most prominent symptom, so prominent indeed as to give the ailment its denomination, if the patient has palpitation, irregular heart, &c., cannot be relied upon, and very frequently can hardly be said to be of any value. In the young the *a priori* probability is mightily in favor of functional disturbance, and yet the complaint may be much louder than where it is associated with grave degeneration. Thus functional or hysterical palpitation is much more complained of by the patient than when it is surrounded by evidences of failing power. Yet, like the anæmic bruit, it has too often caused needless anxiety in the patient, and often led to a loss of reputation with the physician, and will continue to do so until the value of these various objective symptoms be more carefully appraised. This, however, can only be attained by careful observation and reflection, by thoughtful generalisation, and by accurate induction and deduction; and any casual reader who may think that the writer imagines the subject to be completely considered here, or even that the conclusions can be accepted as final or other than tentative, will labor under a grave misconception. They are merely the outcome of his own troubles in attempting to diagnose the cases which have come under his notice, and of his dissatisfaction with the different text-books which do not attempt to convey any impressions as to the respective value of the different symptoms under totally different conditions.

A brief summary of a general nature may not be out of place in conclusion; but it must necessarily be of such an order of generalisation as the space will allow of, and therefore not be absolutely true in all cases. With this premonitory explanation, it may be stated that palpitation, as a laborious effort and evidence of inability, is in the young much more commonly connected with spasm of the lesser arteries and arterioles, and, though often extremely violent, is not significant of danger. In the middle-aged, it is most usually associated with exacerbations in Bright's disease, and, as a temporary result of a passing cause, stands in the same relation to it as hypertrophy or dilatation does to the chronic condition. As a matter of fact, it will very often be found along with other evidences of blood-poisoning, whether arising from inefficiency in the kidneys or liver, and as such is irrespective of the presence or absence of valvular disease. It may be associated with primary organic disease of the heart itself, but is then only very prominent, usually, on sudden exertion.

Irregularity is of deeper moment, and is still more generally associated with inability and failure of power in the muscular walls; it may be said to characterise the stretching of the muscular fibre by prolonged over-distension and incomplete emptying of the ventricles.* It may be found either as a delayed beat occasionally, or as a flutter of small beats in a cluster, the latter marking a condition of great adynamy. It will be found most marked after exertion, and pass almost entirely away with rest; in fact, the importance of examining the heart after exertion is not sufficiently estimated. Symptoms will then be manifest which do not present themselves in quietude, and will enable the observer to more accurately calculate the exact condition. On irregularity as a chronic condition may be implanted palpitation as a passing one, due to temporary disturbance, but the association is less favorable than simple palpitation.

Intermittency marks rather the condition of muscular degeneration, and is more associated with advanced life and evidences of atheroma. In this condition the impaired tissue is less capable of the effort which constitute palpitation, and exertion is more commonly followed by syncope. Thus when irregularity is accompanied by intermittency on exertion, the condition is much more serious than when palpitation is thus induced. Under such circumstances the ominous halt is significant of tissue-degradation, and the prognosis is necessarily grave. The loss of an entire beat in the midst of other evidences of organic disease is a symptom of the gravest import. This condition is often well illustrated in the very aged, and slight exertion becomes a source of imminent danger.

In attempting to estimate the condition from the radial pulse one great matter is overlooked. The failure may lie chiefly in the right ventricle, and may be very imperfectly represented by the radial pulse. The heart itself must be invariably auscultated in the most cursory examination. But yet even the gravest symptom—intermittency—does not necessarily prove the existence of organic mischief, and in attempting to appraise the value of any objective symptom all subjective symptoms must be carefully weighed. Otherwise opinions will be given which will not only unnecessarily alarm the patient and impair the reputation of the practitioner, but which will tend to perpetuate public belief in the mysteriousness of heart affections. And what is also of equal importance, the treatment of each case, to be at all rationally successful, must be founded on the individual necessities of each case, and not by rule of thumb under the tyranny of empiricism. No therapeutics can be worthy of any respect which are not founded on the careful estimation of the value of each symptom and the pathological conditions with which it is allied.

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* See the paper in the *Edinburgh Medical Journal* for longer explanation on this head.

Leeds, 1872.

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ON

TWO CASES OF RECOVERY FROM CHLOROFORM ASPHYXIA.

By James Hardie, M.D.,

Honorary Surgeon to the Clinical Hospital, Manchester.

It cannot be disguised that both the professional and public mind have been much disturbed of late by numerous lamentable deaths from the administration of chloroform, and that a certain timidity, which did not formerly exist, has crept into the minds of many men in the use of this agent. The accident is so sudden and appalling, and so quickly carries the subject of it beyond all means of recovery, that it is of the utmost importance that everyone should possess as clear an understanding as possible of its nature and of the means he should have recourse to under the circumstances.

It appears to me but right, therefore, that I should publish the particulars of two cases which I have myself met with of recovery from what those who witnessed them will agree with me may with propriety be described as apparently perfect death. I am the more desirous of doing so seeing that the means employed in these cases do not appear to be generally known in this country, and were not tried, so far at least as I have been able to judge from the accounts of the cases, in any which have proved fatal since my attention was directed to the subject in 1868.

Both patients were under my care at the Clinical Hospital in this city, and the entire credit of their recovery is due to my colleague, Dr. Gumpert. Dr. Gumpert usually undertook the care of the chloroform at operations in the hospital, and took the precaution of always having a galvanic battery in the room. This, he has since told me, was with the intention of using it in the particular manner to be described, should the occasion for it arise. The notes of the cases are transcribed from those of the house-surgeons in the case-book, written within a few hours after their occurrence. They differ slightly from my own impressions of what took place, particularly in the first case, but are more likely to be correct.

Case 1.—(Dr. Finlayson's notes.)—Harry L——, aged ten years and nine months, was put under chloroform (Dr. Gumpert officiating) for the removal of a cyst in the right scapular region. When the cyst was cut down upon an escape of fluid with curdy contents took place. (The tumour had previously been considered fatty.) This momentarily distracted Dr. Gumpert's attention from the patient, whose pulse was good a minute before, and whose respiratory movements were observed by the ribs. The patient was lying on his left side in a semi-prone position. Dr. Gumpert now noticed that he had ceased to breathe, and the pulse could not be felt. Cold affusion failed to induce respiration, and the patient had a very slightly livid appearance which soon passed off into pallor. The window was opened, and one or two artificial elevations of the ribs produced without any improvement. The larynx was felt to be open both by Dr. Gumpert and Dr. Hardie. The aspect of the patient now seemed very hopeless. The poles of an electro-magnetic battery (standing in readiness) were now applied to the sides of the neck, with the immediate effect of producing a spasmodic movement of the muscles of the neck and of the upper part of the chest to a slight extent; this was repeated again and again, the walls of the chest being depressed in the intervals. Slight and very superficial respiration now occurred spontaneously, and the heart's movements became visible. Occasional use was made of the battery as a stimulant to respiration and also as a general irritant. Warm fomentations were applied to the chest, and the pulse became better, though sometimes intermittent. The respiration became deeper, and the child began to cry when hurt in about three-quarters of an hour. Coffee was administered, and the operation concluded without chloroform.

Case 2.—(Dr. Maccall's notes.)—Elizabeth G——, aged forty-two, had right mamma (scirrhous) removed in March, 1870; the disease recurred in cicatrix, accompanied with affection of the axillary glands. On Aug. 11th, 1870, under chloroform, administered by Dr. Armistead, Dr. Hardie proceeded first to remove the axillary glands, the patient lying on her back with a very slight inclination to the left side. While he was engaged in dissecting out the glands the respiratory movements ceased without previous stertor or other warning; the patient's face assumed a death-like pallor with slight lividity; the eyes were staring and the pupils widely dilated; the pulse immediately afterwards became imperceptible.* The tongue was at once drawn out, but without response on the side of the respiration. The poles of an electro-magnetic battery were applied to the root of the neck over the phrenic nerves (cold affusion and a few acts of compression of the chest having in the meantime failed to induce respiration.) For a time there was no response, and the patient's condition seemed hopeless. At length, however, a short gasp came, then others, which soon became deeper and regular; the pulse became perceptible, the color returned to the face, and the pupils contracted.

To this account I would only add that, though in such circumstances it is of course very difficult to estimate time correctly, after the operations we compared our several impressions on this point, and I believe that I am within the mark when I put down three minutes for the first case, and fully one minute for the second, as the length of time they respectively appeared to be dead. Certainly it seemed to be much longer, particularly as regards the boy. In that case I had altogether lost hope.

Faradisation of the phrenic nerves in chloroform poisoning is based on the supposition that it is the respiratory process which is at fault. Since Snow wrote his book with his elaborate collection of cases, the opinion has been almost universal that, whatever may have been the exact pathological process, death nearly always originated at the heart. It must be confessed, however, that, as regards such cases, the vast majority of medical men have exceedingly obscure ideas, which may amount to this: that they are in some way attributable to an ill-defined diseased condition of the heart (oftener supposed than discovered at the subsequent post-mortem examination); and in the case of others, again, who have formed more elaborate theories, that these for the most part rest on the flimsiest possible evidence. For the other side of the question I would refer to papers by Dr. B. W. Richardson in the *Med. Times and Gaz.*, vol. i. 1870. It is not for me, in the present state of our knowledge of the subject, to attempt to judge between these opinions. I may, however, be permitted to point out that the evidence deducible from these two cases, as well as others of the same kind, is strongly corroborative of the view that it is the respiratory process which fails first. The direct employment of means to an end, and the satisfactory accomplishment of this end, so strikingly shown in both these cases, seem very convincing. In addition to this, in the case of the woman the continuance of pulsation in the radial was distinctly observed for an appreciable time after the cessation of respiration.

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* Dr. Armistead had observed that it could be felt after the respiratory movement had ceased.—J. H.

If it be granted that this may be the mode of death in any case of chloroform poisoning, it will readily be understood, from what we know of the effect of the interrupted galvanic current in stimulating muscular contraction, whether applied to the muscular fibre or to the motor nerves, that this agent is likely to be of the utmost service in exciting the paralysed muscles of respiration, and one would be prepared to expect a satisfactory response to its proper application in such cases. In 1842 Marshall Hall proposed sending a current through the respiratory muscles in still-born children by applying one pole to the neck, either at the back or at the side, and the other over the diaphragm. But this could only act as a superficial irritant, and will not affect the muscles in the manner referred to. Duchenne,[*] in 1855, first proposed the methodical faradisation of the phrenic nerve in asphyxia, and experimented on animals successfully. Ziemssen,[†] however, was the first who used the method in the human subject. This was a case of suffocation by charcoal fumes, and life was not quite extinct. In 1858 Friedberg first used it in chloroform asphyxia in the human subject.[‡] In this case (the case of Otto Krause) respiration and pulsation were unsatisfactory for some two or three minutes, during which cold water, liquor ammoniæ, and other irritants were used. Respiration and pulsation then completely stopped, and for three minutes artifical respiration was tried without avail. Then galvanism was applied to the phrenic nerves, and after the application the diaphragm responded and the patient gradually recovered. Ziemssen records several cases, in which he himself also used it, of suffocation from poisonous gases; and since then I believe that several cases of its successful employment in chloroform accidents have from time to time occurred in Germany.

The poles should be large enough to cover a considerable surface, and should be placed behind the posterior border of the sterno-mastoid, one on each side, a little below the middle of the neck, and be pressed deeply inwards and forwards, as if to get under the edge of the muscle. In this way the phrenic nerves, as they pass downwards in front of the scalenus anticus muscles, cannot fail to be brought under the influence of the current, and these muscles themselves will also be stimulated to contraction. As soon as an inspiration has taken place the poles should be raised, while an assistant compresses the chest walls to imitate the expiratory movement. When this has been done and the pressure has been removed, the poles are again applied, and so the process should be rhythmically continued until the danger has passed or the efforts are clearly unavailing.

It is not my wish to decry other means which have their advocates for averting a fatal issue in these accidents. I am only desirous that this method should be brought prominently under the notice of the profession, and that, whatever theory anyone may hold as to the mode of death, due weight may be given to the fact that this plan of faradising the phrenic nerves has in these two cases and certain others been the means of rescuing patients from death. Nor am I sure that it is asserting too much regarding it to say, that of all means which have been tried it has been proved to be the most successful, taking into consideration the condition of the patients. I think, therefore, seeing that one can never be sure that on any occasion an accident may not occur, it would not be giving too much weight to these cases if the knowledge of them induced every medical man to make one of the many convenient portable batteries the invariable companion of his chloroform bottle.

Manchester, March, 1872.

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ON

PHLEGMASIA DOLENS: ITS ORIGIN & ITS CONNEXION WITH ERYSIPELAS AND OTHER INFECTIOUS DISEASES.

By Robert J. Lee, A.M., M.D.

It is important that the pathology of phlegmasia dolens, so far as its practical treatment is concerned, as well as its probable connexion with certain infectious disorders, should be clearly understood by those who are required to give attention to patients attacked with it. Some recent writers on this subject leave the reader in doubt of the origin of the affection, and the mode in which the condition observed after death is produced; so that a confusion of ideas exists in the minds of those who have not had much clinical experience of this disease, and have enjoyed no opportunity of pathologically investigating it. To some extent, the introduction of new terms imperfectly defined contributes to this result, as will be pointed out in the following remarks.

In the first place, the difference should be clearly established between coagulation of blood in veins and in arteries, and a more definite meaning ought to be attached to the terms "embolism" and "thrombosis" than is conceded them by some pathologists,

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[*] De l'Electrisation localisee.

[†] Die Electricitat in der Medicin. Second Edit.

[‡] Virchow's Archiv, 1859, Bd. xvi.

In the second place, it is proper to distinguished between cause and effect when considering the origin of coagula in the vessels of the circulatory system—that is to say, whether the formation of coagula be the primary disorder or simply arises from morbid changes of tissue, productive, among other results, of mechanical interference with the free circulation of blood through the vessels of the part affected.

With regard to the relation which has been suspected to exist between the causes which give rise to erysipelas, puerperal fever, hospital gangrene, phlegmasia dolens, and other forms of disease of similar nature, it must be allowed that we are unable to demonstrate beyond all dispute the identity of their origin. But we may fairly inquire whether we ought not to be satisfied with something less than material proof when we reflect upon the obscure and mysterious character of this class of infectious diseases, and, in the interests of humanity, whether we ought to reject presumptive evidence when it is all that the most careful investigation furnishes as our guide to practical conclusions.

These remarks are introduced as a prelude to a case of phlegmasia dolens to which the attention of the reader is invited, as it is one of great interest from the circumstances connected with its origin, and from the rare and serious form which it assumed. The unfortunate lady who was the subject of it was engaged in the anxious and arduous duties of matron to the Westminster Hospital. With more zeal than prudence she neglected the first warnings of constitutional disturbance which preceded the attack of phlegmasia, and which was occasioned by long and frequent visits to wards in which several patients were suffering from erysipelas of serious character, among whom were two of her own nurses, the especial objects of her attention. It may be mentioned that both in the medical and surgical wards there were cases under treatment; those in the former had been admitted for erysipelas, those in the latter were attacked by it during their stay in the hospital. It was in the late part of last year that this tendency to the occurrence of erysipelas showed itself, outside the hospital as well as in the wards. It was clearly noticeable that the slighter cases were not productive of ill effects upon the nurses or adjoining patients, but that it was from the severe cases, and particularly from one that was fatal, that the infection was spread.

The details of the matron's illness combine to produce one of those rare and serious instances of phlegmasia of the lower extremities in which the favorable progress of the patient is suddenly interrupted by the occurrence of the fatal symptoms of plugging of the pulmonary artery and coagulation of blood in the right side of the heart, which are referred to the detachment of a portion of clot and its impaction in the pulmonary vessels. The disease showed itself first in the right leg, in a few days subsided, and then attacked the left leg, and later again the right; but at the end of three weeks the patient was making favorable progress when the symptoms occurred which in a few hours terminated in her death. It was immediately after changing her position in bed that the palpitation of the heart and distress in the respiration were experienced. The very first sensation which preceded these symptoms was a peculiar movement, as though something had rapidly passed from the left inguinal region directly to the heart; and so distinct was the account which the patient gave of the occurrence that the question rose in the minds of Dr. Fincham and Mr. Holthouse whether it could not fairly be referred to the detachment of a portion of clot and its arrest either in the right side of the heart or pulmonary artery. To what extent we are justified in directly connecting the symptoms with the change of position in the body upon which they followed is deserving of consideration, as the same cause has produced the same result in other cases, and indicates the necessity of making a patient observe the greatest tranquillity, as well as of preparing the friends of a patient for so sudden a catastrophe. It may also be remembered that in this form of phlegmasia dolens the origin of the disease is not to be referred to the uterus, as is the case in uterine phlebitis, but is probably the same as in those cases of phlegmasia which occur occasionally in the male sex.

The lectures of Trousseau contain a few of the most apparent reasons for supposing that there is some connexion between erysipelas and phlegmasia, but no individual instance is recorded of the latter disease having followed exposure to the influence of the former, and although a single case may contribute but imperfect evidence that such is possible, it is sufficient to direct attention to a careful investigation of the subject.

Between cases of simple phlegmasia and "idiopathic" erysipelas there is a strong resemblance in the sequence of symptoms and the particular tissues affected. There is this difference, however, that in the former the superficial cellular tissue, in the latter the deeper structures, are the seat of disease. With respect to the coagulation of blood in the veins, it is not difficult to demonstrate that it is the consequence of inflammatory changes in the connective tissue surrounding the vessels. If in a case like the one under notice a careful examination be made of the condition of the cellular tissue in the vicinity of the termination of the coagulum, it will be found that generally for a distance of from one to two inches above that point there is evidence of the change to which the term "cellulitis" is applied. The best method of demonstrating this is to dissect the artery from the vein with a scalpel till a slight induration of the tissue is observed and consequent adhesions of the vessels, so that greater force is required to separate them. The earliest evidence of the change referred to is always found above the point where the coagulum in the vein terminates; and, as I have frequently investigated the subject, I believe I am justified in supporting the view that the change of external tissue precedes and is the cause of the coagulation of blood in the veins as it advances in the direction of the heart; that it may likewise spread down the veins is highly probable; and that the effect of the inflammation of the cellular tissue, as it follows the course described, is gradually to arrest the venous circulation, occluding vessel after vessel in its progress more or less completely, the coagulum forming first in the minute veins and subsequently in the larger. If we take that part of the principal vein where the coagulum terminates, and, passing upwards, examine the orifices of the vessels which open into it for some distance above, we shall find that a coagulum has already formed in them, and that the terminations of the clots they contain project into the larger vessel, although the coagulum in the latter has not reached the same point.

In the case of phlegmasia which forms the chief subject of these remarks, the aorta and the vena cava were easily separable as far as their division into the common iliac branches. In this situation the vessels were surrounded by tissue, in which the earliest changes of cellulitis were observable. On the right side the artery and vein were firmly adherent where the division of the common iliac vessels takes place, and where the internal joined the external vein was found the termination of the coagulum, which extended down the latter vessel to those of the whole lower extremity. On the left side the vessels were effected in the same way, as high as Poupart's ligament, but above were quite pervious, and the cellular tissue in the pelvis presented a perfectly natural condition.

When I stated in the early part of my remarks that certain terms were used inappropriately by some pathologists, I referred more particularly to the use of the term "thrombosis" in the case of all kinds of clot as the cause, and not the consequence, of previous change of tissue. It may be desirable to retain the word, but with the necessary predicate defining the cause of the clot; for "thrombosis" means simply "a clot" and nothing more. I need not point out the necessity of such addition to the term, as it may be applied to the coagulum in an aneurismal sac, in the termination of a ligatured artery, to the consequences of an embolism, and to other mechanical causes; and when I say that by the introduction of such terms as "embolism" and "thrombosis" a confusion of ideas has arisen, I wish to imply that the opinion entertained by some that the clotting of blood in veins is the cause of phlegmasia is erroneous, and that they overlook the real origin of it, thus mistaking cause for effect.

Maddox-street, March, 1872.

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CHLORAL IN HYDROPHOBIA.

By J. D. Sainter, L.F.P.S. Glas., L.S.A. Lond.

On Wednesday, November 29th, 1871, about 9.30 P.M., I was hastily summoned to visit Mr. M——, whose residence is in an adjoining street. He is about forty years of age, and I have known him for some years as a person of active, business-like habits, temperate and always enjoying good health. On approaching his house I found the neighborhood in a state of great excitement, and his bedroom was nearly full of people. He was yelling and struggling in frightful convulsions on the bed, and it required the united efforts of eight or ten people to keep him upon it. Upon inquiry, I learned from Mrs. M——, that after her husband had been at home for half an hour, he complained of feeling very poorly, and said he would go to bed. She likewise added that he had not been well for the last week or more. Shortly after he had gone up stairs she heard a disturbance in the bedroom, and upon going to see what was amiss, she found Mr. M—— floundering on the floor, apparently unable to stand upright, quite unconscious, and screaming with agony. She immediately left the house for help, and found two or three friends who returned with her, and with some difficulty they dragged the poor sufferer from under the bed, where he had crept, and placed him upon it. After I had witnessed the terrible scene above described, and having waited until my patient had become moderately tranquil, I noticed first the peculiarly wild expression of his features, his eyes being very brilliant, with the pupils fully dilated. He sighed now and then, seemed timid and anxious, and upon recognising two or three friends around the bed, he asked them, "What they were doing there?" "What was the matter?" Pulse about 95, and slightly intermittent. He complained of an uneasy sensation about the epigastrium, and was inclined to be sick; also of stiffness about the neck, and a sore-throat. Occasionally he ejected frothy mucus from the mouth in a whining tone of voice, with incoherent raving. After a short interval, upon offering him some tea, the muscles of his face began to twitch; he grinned, and, looking wistfully at it, after some little hesitation he swallowed a little with difficulty. Soon after this, his breathing having become greatly embarrassed from spasmodic action of the diaphragm and the muscles of the larynx and pharynx, he sprang up suddenly, foaming at the mouth, in a perfect frenzy of delirium, and tossing his hands about in all directions, he made desperate efforts, with hideous howls and shrieks, to jump off the bed. This was luckily prevented by the bystanders, as on the previous occasion; and after becoming exhausted by his fearful efforts, he sank gently down on the bed, and covering himself overhead with the bedclothes, he lay in a state of stupor. Presently another attack seemed imminent, he became restless, and, throwing off the clothing, he pointed to the pit of his stomach, tremulously exclaiming, "Now I feel it coming on; its working up; lay hold of me!" Convulsive difficulty of breathing, with constriction of the throat, then advanced apace: he snapped his teeth, hawked and spat, and became furious, being nearly suffocated; and similar measures were adopted by way of restraint as I have mentioned before. These paroxysms of excitement, attended by more or less intensity of suffering, succeeded each other about every ten minutes. On one occasion my patient exhibited a biting propensity. During a very severe attack he laid hold of my overcoat sleeve, which is a thick double-milled cloth, and took a piece clean out of it the size of a half-crown. He likewise snatched at one of his friends, and seized one of the buttons of his coat. Such were some of the most prominent and leading symptoms that presented themselves to my notice. That some of them were of an hydrophobic character I had every reason to believe, more especially when recollecting that I had cauterised Mr. M——'s hand for a bite from his own dog some four or five months ago.

With respect to the treatment, shortly after I had been called in, as I have before stated, he complained of nausea, and I went home for an emetic. On my return I met C. J. Sanford, Esq., surgeon 2nd Royal Cheshire Militia, whom I invited to see the case. After the operation of the emetic (which required two ounces of ipecacuanha wine given twice before vomiting was produced), my patient appeared to be relieved, the spasms being less violent. By-and-by, however, they became more active, so much so that it appeared to me exhaustion would sooner or later be the consequence unless something was done. I therefore suggested to my colleague that we should try the chloral hydrate. Accordingly twenty grains in simple syrup were administered every hour. After the third dose the violence of the symptoms began to moderate. After the fourth dose there was a still greater improvement. The next dose brought on slumber, and this soporific effect was kept up by giving the same dose of the chloral at longer intervals. For the next twenty-four hours nothing of any consequence occurred, with the exception of slight twitchings of the face and jerkings of the arms and legs during sleep. These were allayed at any time by an extra dose of the chloral. Beef-tea, mutton-broth, common tea, or water-gruel were given him occasionally, which he swallowed without much objection when fairly roused up. During the next three days the somnolency was kept up by the medicine, with only a few twitchings showing themselves. On the morning of the fifth day he awoke out of a gentle slumber, and said to his wife: "I feel as if I should like to bite somebody." This was the last symptom noticed of an hydrophobic character. After the fifth day the chloral hydrate was discontinued, and the quantity taken altogether amounted to 360 grains.

In this case I have to remark that there was not so much thirst or foaming at the mouth as I have seen in others, neither was there so much objection to swallowing fluids when deglutition was practicable and he was not affected by sounds or currents of air. There is not much doubt, however, that if the attack had lasted longer the symptoms would have assumed a still more aggravated form. I have great faith in the chloral hydrate in many affections, and, in this instance, I should have given it by injection rather than have omitted to use it.

It may be easily imagined, after what I have reported, that the physical powers of my patient had been severely taxed. It required some weeks before he was able to leave the bedroom, owing to extreme nervous depression and general prostration. His mind was kept as much as possible in a state of repose for fear of bringing on any remaining tendency to spasm. Little or no allusion was made by any person to him as to what had been the nature of his illness; and after a time, when fit to travel, he went among some friends in Derbyshire. He has lately returned, able to attend to business; and I have gleaned from him the following particulars with reference to the premonitory symptoms of his case. He states that a fortnight before his attack he felt a pricking sensation about that part of the hand which had been bitten, followed in two or three days by swelling, and a pain striking up the whole arm, which afterwards became numb. He thought it was rheumatism. These symptoms increased, and he began to decline in health. His appetite failed; he had chills and heats, with an occasional headache; felt confused, anxious, and irritable; was easily startled and alarmed. When walking along the streets he would suddenly stop or turn round, and did not know the reason why. If a bird flew out of a hedge, or any unusual noise occurred, he felt agitated. When at chapel on the Sunday previous to his being laid up he experienced a sudden impulse to spring forward and jump over the front of the pew, and he restrained himself from the attempt by laying hold of the seat with both hands. With respect to the history of the dog, Mr. M—— states that before it bit him he had noticed a change in the animal's temper for some time; it had become shy, peevish, and sullen; and one night, as he was tying it up to the kennel, without any provocation it laid hold of his hand. The dog was destroyed on the day following Mr. M——'s attack of illness.

Macclesfield, Feb., 1872.

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THE

CONVALESCENCE FROM TYPHOID FEVER.

By T. J. Maclagan, M.D.

The convalescence from typhoid fever, even when there is no complication, is long and tedious; and during its progress there is a greater tendency to the development of pulmonary mischief and other debilitating disease than is found to exist after other forms of fever. Typhus, for instance, may bring the patient as near the point of death, and leave him as thoroughly prostrate, as an attack of typhoid; but as soon as the fever is gone he begins to rally and pick up; the tongue cleans; the appetite returns, and soon gets keen and sharp; digestion is as vigorous as in boyhood; the richest diet may be taken with impunity, and with a relish hitherto unknown; wine and other stimulants may be taken with equal relish and impunity, and in quantities which to the uninitiated seem almost sinful. The patient enjoys the good things of this life as he probably never did before, and rapidly and steadily gains strength. It is not so in typhoid. The appetite is less keen; digestion and assimilation are less perfectly performed; great caution requires to be exercised by the patient both as to the quality and quantity of his diet; and he finds that his convalescence is not only long and tedious, but that it is apt to be interrupted by discomforts and anxieties which are unknown to the convalescent from typhus.

I have suffered from both forms of fever since I entered the profession, and can bear personal testimony to the difference in the sensations of the two classes of convalescents. The convalescence from typhus was one of the most enjoyable periods of my life; that from typhoid was bereft of great part of the enjoyment which would otherwise have accompanied it by the slowness with which strength returned, the discomfort caused by any indiscretion, and the constant feeling that care was needed to prevent a recurrence of such discomforts and possibly greater evils.

Why is this? In what respect does the convalescent from typhoid differ from the convalescent from other forms of fever? His chief peculiarity is to be found in the condition of the small intestine. It contains numerous ulcers, resulting from the destruction of the glands which were specifically affected during the fever. The number and extent of these ulcers bear a direct relation to the severity and duration of the attack from which the patient is recovering. The protracted duration of the convalescence from typhoid fever is usually attributed to the presence of these ulcers and the length of time that the patient is under the influence of the poison of the fever. The true explanation of the peculiarity alluded to is to be found, not in the *presence*, but in the *mode of formation* of the ulcers. They result from the destruction of glands which play an important part in the assimilation of the nutritious ingredients of the chyle.* The destruction of such glands cannot but be followed by a serious interference with the process of assimilation. Such interference affords a sufficient reason why convalescence should be slow, and why it should so often be further complicated by the appearance of pulmonary mischief in those at all predisposed to tubercular disease, and by the occasional onset of other diseases attributable to mal-assimilation. It is not so much that the food is not digested as that it is not assimilated. A convalescent from typhoid fever may consume (though seldom with the same relish) as large an amount of real nourishment as a convalescent from typhus, and may digest it too, but it is not assimilated in the same way; and food which is not assimilated had better not be taken, as it tends only to produce discomfort and retard convalescence.

Such is the condition of every convalescent from typhoid fever. When he is reduced to skin and bone by a long and trying illness, and when he requires all the nourishment that can be given, a number of the channels by which the chyle enters the system are destroyed, with the inevitable result of greatly retarding the return of strength. In time nature accommodates herself to the altered circumstances; the other absorbent parts of the intestine do the work previously done by the glands which have been destroyed, and by-and-by they are not missed. Such a compensating increase of function requires some time for its completion, the time varying in different cases according to the number of glands which have been destroyed. Until it is completed health and strength cannot be fully restored. Every physician knows that the diet and regimen of a convalescent from typhoid require to be carefully regulated. A typhus convalescent may be left almost entirely to the cook, and may be sent away for change as soon as he can move about with a little comfort. It is not so with the convalescent from typhoid; diet must be so regulated that he does not have more than the altered condition of his intestine permits of his assimilating. The quality should be good, but the quantity not great; neither should too long an interval elapse between each meal. Nourishment should be given frequently, say every three hours, and in small quantities, rather than in large quantities at wider intervals. It should also be very gradually increased. The uninjured absorbent portions of the bowel accomodate themselves to the increased work gradually, and not completely for some time. The work given them to do should therefore be also increased gradually, and full diet should not be returned to until there is good reason to believe that it may be properly assimilated. Nothing is so apt to retard convalescence as sudden changes from a comparatively poor to a rich diet. During the first week or ten days of convalescence I generally confine the patient to milk, beef-tea, and the lighter farinacea, given in small quantities and frequently. The object is (1) to give only such articles and such quantities as may be assimilated before the seat of the destroyed glands is reached, and (2) to guard against unnecessary depression by giving a second meal as soon as the first is digested and got rid of. By gradually increasing the quantity of food the action of the absorbent parts of the bowel is stimulated, and the patient is in time brought in safety to his wonted health and strength.

There should be no unnecessary exertion or excitement for some weeks; the patient should not try his strength, but should rest content with the assurance that digestion and assimilation are being properly performed, that he is gradually gaining ground, and that no local disease is manifest. As assimilation is imperfect, no needless waste should be created by undue exercise of either mind or body. All that can be taken up by the lacteals is required for restoring to the normal the various organs of a body which has been wasted by the long duration of the fever, and which has had an important part of its restorative apparatus destroyed during the course of that fever. Therefore there should be no unnecessary waste. Again and again I would urge the necessity of abstaining from directing to the muscles any undue portion of the comparatively meagre supply of nutriment which gets into the system, and every bit of which should be utilised in warding off as far as possible those serious organic changes (the results of mal-assimilation) which are so apt to show themselves in the lungs and other organs. A convalescent from typhoid fever should not take much exercise until health is pretty well established, and should not return to business for at least two months after he is able to leave his room.

In an interesting communication read at the last meeting of the British Medical Association, Dr. T. Clifford Allbutt, of Leeds, gave the notes of five cases of marasmus in which the wasting was attributed by him to mal-assimilation consequent on destruction of the absorbent glands during an attack of typhoid fever. Such extreme cases I have never seen, but could quite fancy their occurring.

Dundee, March, 1872.

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ALCOHOLIC PARESIS AND PARAPLEGIA.

By J. Lockhart Clarke, M.D., F.R.S.

The habitual and excessive indulgence in the use of alcoholic drinks is so frequently followed by partial or complete paralysis of the lower extremities, that no doubt can be entertained that alcohol and paraplegia do often stand to each other in the relation of cause and effect. Many striking cases that have come under my own care support this inference in a very convincing way, so that I think Drs. Handfield Jones and Wilks have done good service in bringing the subject prominently before the profession.

On examining the bodies of persons who have died either in a state of intoxication or during a course of excessive indulgence in the use of alcohol, particularly in its undiluted state, almost every organ and tissue is found to be more or less altered in appearance. The mucous membrane of the pharynx, œsophagus, stomach, small intestines, and bronchial tubes is red and injected. The liver and kidneys, the substance and membranes of the brain and spinal cord, are in a variable state of congestion. In chronic cases of alcoholism we find more or less atrophy of the cerebral convolutions, and effusion of fluid on their surfaces, with thickening and adhesions of the membranes.

On examining such persons during life, we find a train of symptoms that are quite in accordance with these post-mortem appearances. The mucous membrane of the fauces is unusually red and injected; a viscid, tenacious, or cream-colored mucus hangs about the back of the pharynx and soft palate, exciting vomiting on rising in the morning; a sensation of rawness or heat is sometimes felt along the œsophagus, particularly on drinking warm or spirituous fluids; the stomach is irritable, and vomiting is easily excited. A viscid and tenacious mucus collects during the night on the membrane of the bronchial tubes, causing more or less shortness of breath, or oppression of the chest on waking in the morning, and exciting cough, which is frequently attended by vomiting. The skin has frequently a dirty-yellow tint, although sometimes a remarkable clearness and freshness of color is preserved, especially in fair persons. The conjunctivæ are often thick, injected, and dirty-yellow.

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* See a paper on the Bowel Lesion of Typhoid Fever, its Nature and Treatment, in The Lancet for April.

Different individuals suffer from a variety of nervous symptoms—from giddiness; a sensation of being lifted from the ground, or "taken off their legs," as I have heard them express the feeling; an alarming sensation of choking or suffocation on dropping off to sleep at nights; occasional numbness or "pins and needles" in the fingers and toes; or acute pains beneath the nails; a dull, aching pain across the loins, or an acute pain on bending or on rising from the sitting posture; a darting or lacerating pain in the lower extremities during the act of progression.

The muscular system is more or less affected by weakness, by spasm, or by the withdrawal of voluntary control. The hands and arms, and sometimes the whole body, are tremulous, with frequent fibrillar quivering of the muscles, particularly about the face. Voluntary movements are imperfectly or awkwardly performed. Even when there is no tremor of the hands, delicate operations —such as writing—are clumsily performed, and are irksome or distressing. The walk is more or less unsteady; and I have often noticed that in progression each foot is alternately carried inwards, and almost in front of the other. The articulation is sometimes peculiar and indistinct from partial loss of control over the muscles of the lips. The individual loses his accustomed energy, is careless of his own interest, and feels himself unfit for the performance of his ordinary duties, until he has resorted to his usual potation, or obtained from the druggist a "pick-me-up." For this distressing state I have found nothing so useful as full doses of nitro-hydrochloric acid, with a little of Battley's sedative solution; two or three doses, even, sometimes act like a charm. After relieving the bowels by saline aperients, I combine the acid with quinine, perchloride of iron, or small doses of strychnine. The occasional use of the Turkish bath is also beneficial.

If the individual persists in the excessive use of alcoholic drinks, partial or complete paralysis, and particularly paraplegia, not unfrequently results. I will only briefly mention two or three of the most striking cases out of a number that have come under my care.

A cab proprietor and his wife, both remarkably fine and naturally healthy persons, between thirty and forty, were addicted to the most immoderate abuse of neat spirits. The wife, after two or three years of excessive indulgence, with many of the symptoms above described, became very fat and bloated, and one morning found that she had complete paralysis of the lower extremities; sensibility was also almost entirely abolished. At the end of ten weeks she quite recovered, and I subsequently lost sight of her, but heard that, after a renewal of her old habit, she died of some stomach or bowel affection. Her husband, who, as I was told, would drink daily as much as a quart of gin, had only partial paraplegia, with great numbness and pains in the legs. Unlike his wife, he became thin and sharp-featured; and I have seen this originally powerful and magnificent man, who was six feet two inches in height, look down upon his tottering and wasted legs, and burst into tears at the recollection of what he once was; and yet he has straightway gone to raise his spirits by a repetition of his accustomed potation. He went to Bath for the waters, and died there.

Another striking case was that of an old sergeant who had served through the Peninsular War—a tall, stalwart Yorkshireman, seventy-two years of age, who assured me that for the last twenty years he had never taken less than three-quarters of a pint, and sometimes a pint of rum daily, besides sundry glasses of gin and beer. He smoked, also, almost incessantly from morning till night. One day he lost the entire use of his lower extremities without much impairment of sensibility. In six weeks he quite recovered, and became much more moderate in his habits; but two years after I saw him again with almost complete paraplegia. He had frequent vomiting, a brown, parched tongue, with that peculiar smell of breath which so often precedes dissolution, and at the end of a week he died.

Harley-street, W., March, 1872.

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OUTLINE OF A

CASE OF OBSTRUCTION OF THE BOWELS, AND THE TREATMENT.

By J. Batteson, M.R.C.S., L.S.A.

Mrs. J——, aged thirty-three, healthy and well nourished, suffered from habitual constipation. On December 1st, 1871, she had had no action of the bowels for five days. Before the passage of the last stool she experienced great pain. Ordered sulphate of magnesia in one-drachm doses every three hours, and a powder of calomel and jalap at bedtime. No action. Castor oil was then given, but with no result. The abdomen was now distended, there was general tenderness but not great pain, except on pressure in the left iliac region. A dull, aching pain extended over the lower part of the abdomen. On percussion the abdomen was tympanitic along the course of the cæcum and colon. In the left iliac region a dull sound was produced. No marked anxiety of countenance; no vomiting, and only small quantities of pale urine were passed; pulse quick and small. On examination per rectum, the bowel was found empty, the uterus, which felt heavy, interfering somewhat with the examination. The œsophagus-tube passed up the rectum about eight or ten inches, and then met with some obstruction, beyond which it would not go. Only from fifteen to twenty ounces of fluid would remain by injection. Calomel and opium were at first administered, but after the first day of this treatment the calomel was discontinued, and opium only, in one-grain doses every four hours, was given. Enemata were used frequently. Small quantities of milk allowed.

Dec. 7th.—Met Dr. Moxon in consultation. At his suggestion the patient was kept hungry and thirsty, no food being given, and only small quantities of ice to moisten the mouth being allowed. Hydraulic pressure was tried, but failed. Opium was now given in quantities of one grain every three hours.

9th.—Met Drs. Moxon and Madge in consultation. Abdomen was now much distended; occasional spasmodic pains; slight vomiting of mucus, but no stercoraceous matter; more urine voided. It was decided to continue the same treatment. Bougies were used, but could not be made to pass the obstruction. On the evening of this day patient had a severe attack of syncope, so brandy was freely given; but after each dose vomiting occurred. There being great restlessness, fifteen minims of nepenthe were injected subcutaneously, and much relief obtained.

11th.—Patient much weaker; countenance anxious; vomiting of offensive, dark, chocolate-like fluid. I called upon Dr. Moxon and explained condition of the case. We decided to invite Mr. Bryant to perform Amussat's operation at once, if after again seeing my patient I considered her able to bear it. On returning I found the patient in a condition so nearly approaching syncope that I deemed it advisable to postpone the operation. On visiting her in the evening I found the tendency to syncope passed away. Administered another injection, and again tried hydraulic pressure, but with no good result.

While thinking what could be done for the poor creature, it occurred to me that if the reports of prisoners secreting instruments in their rectums were correct, it might be possible to pass the hand into the bowel; so, after explaining the matter to the patient and her husband, and having obtained permission to do as I liked, I proceeded as follows:—Having bared my left arm, well oiled my hand and wrist, and put my fingers in the form of a wedge, I gently passed my hand through the sphincter ani into the rectum. I then rested a minute, for my hand was painfully cramped; then passed my fingers up the sigmoid flexure of the colon, where they met with a stricture. At first I could only get the tip of my middle finger in the ring; but by steady pressure and moving the finger about, I pushed it up as far as the first joint: I was then able to get the tip of the ring finger through the opening. Having again dilated it, I passed the œsophagus-tube along the palm of my hand and through the opening, and then it easily passed several inches up the bowel. My assistant then injected a pint or more of warm water; this was allowed to remain in about five minutes, and then I removed the tube and my hand, when instantly there was a great rush of fœcal matter and fluid of a most offensive odor. After further injecting the bowel, six pounds or more of the pent-up secretion were liberated. The patient felt much relief, and rallied for a time, but afterwards relapsed into the condition of syncope, and died about three hours after the operation.

On inspecting the body thirteen hours after death there was no emaciation, there being much subcutaneous fat. On opening the abdomen, about a pint of fluid was found in the peritoneal cavity, most of which had probably escaped from the bowel, which had been perforated by an ulcer at the seat of stricture at the lower part of the sigmoid flexure. The small intestines were much distended, the large ones empty, and both were much congested. The strictured piece of bowel I have preserved.

Bow-road, E., Jan. 1872.

REMARKS ON A CASE OF EPILEPSY AND DELIRIUM TREMENS.

By JOHN MOIR, L.R.C.P. Edin., &c.,

District Surgeon to the Duke of Sutherland.

ABOUT 8 A.M. on Sunday, Jan. 14th, I was called to see Mr. B——. The messenger instructed me to hurry as fast as I could, as, though the patient was only a short distance from my residence, he did not think he could live till I got there. I learned on my arrival that his illness was first noticed by his housekeeper at five o'clock that morning; she heard him groaning, and on going to his bedside found that he could not articulate distinctly; she could not understand what he was trying to say, and suspected an attack of paralysis.

From the previous history of Mr. B——, and particularly that of the past month, I was led to form the opinion that the case was one of excessive indulgence in ardent spirits. His tongue was extremely swollen, dry, and of a bright-red color, filling almost the whole cavity of the mouth, so much so that he could not put it out past the teeth; nor could I see anything of the state of the fauces. He could swallow fluids only with the greatest difficulty, solids not at all. I ordered him twenty-grain doses of bromide of potassium in an infusion of angostura bark and quassia every four hours, mouthfuls of iced water, an occasional drink of coffee and milk, and hare soup, as beef-tea was not to be got at the time. At 4 P.M. he had a very severe epileptic fit, and bit his tongue badly; it lasted upwards of half an hour, and on coming out of it he said he felt better, and his articulation was certainly very slightly improved. He had no more fits, and during this day he had broken snatches of sleep, starting up every now and then, but was quite sensible until night, when sleep left him and delirium tremens set in violently. On Monday, Jan. 15th, he continued the same remedies, as also during the night and on Tuesday morning. His tongue was now much better; he could put it out a good way, and articulate distinctly enough to be thoroughly understood, sometimes quite distinctly, and occasionally talked sensibly for a minute or two, but got now no sleep whatever. At 2 P.M. on Tuesday I therefore ordered him twenty-five grain doses of chloral hydrate in simple syrup every four hours. After the first dose he got little relief; but after the second, at 6 P.M., he slept uneasily for a good while, so that I deferred the third dose until midnight. Unbroken sleep followed, and on awaking the hallucinations were very slight; they gradually passed off during the forenoon, and did not return. A dose of chloral was given at night only for the next two nights, but, as the patient complained greatly of soreness and pain at the cardiac orifice of the stomach as if it was raw, and spasmodic contraction at every time food either solid or liquid was passing into the stomach, I ordered him a twelve-ounce mixture of bismuth, tincture of nux vomica, and infusion of gentian, which speedily relieved him entirely of the pain and spasms. By careful attention to diet, rest, &c., in four or five days afterwards he was able to get up a little each day, and then to go out for a short walk, until in the latter end of February he resumed his usual occupation, and has since continued in a very fair state of health.

Tongue, Sutherlandshire, 1872.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

> Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

UNIVERSITY COLLEGE HOSPITAL.

CASES OF STRICTURE TREATED BY A NEW WEDGE DILATOR.

(Under the care of Mr. Berkeley Hill.)

The accompanying woodcut depicts a modification of the principle adopted for Holt's, Perreve's, and Richardson's stricture dilators, which has been used with success by Mr. Berkeley Hill in University College Hospital. The two following cases are selected from a series:—

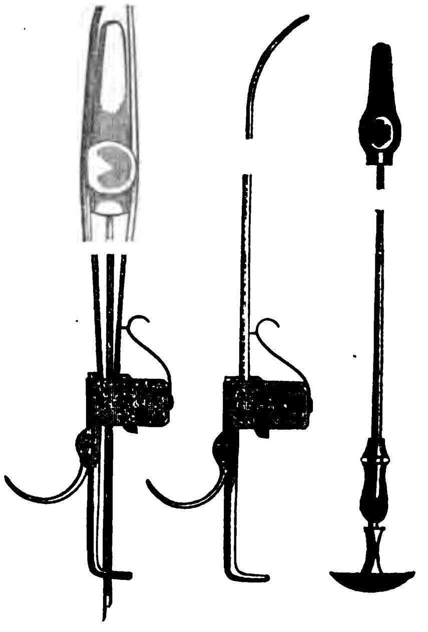
Case 1.—C. J——, aged thirty-nine, admitted Sept. 4th, 1871, formerly in the Marine Artillery, has suffered from stricture for many years, and occasional attacks of retention of urine during the last ten years. Has been in hospital in a provincial town; but the surgeons under whose care he has been for the last six years at different times have not been able to introduce an instrument into the bladder. In despair he has come to London for further advice. On admission, the patient is thin, and looks much older than he really is. He is constantly agonised with desire to pass urine; this escapes in drops, or, if he forces violently for a few seconds, in a stream. The urine is dark, offensive, and one-fourth is ropy pus. In the perineum a mass as large as the finger, and apparently as hard as wood, occupies the middle line. There is no swelling of the superficial parts, and the prostate is not enlarged. During many trials with bougies of various sizes and kinds, and the involuntary explorations of old false passages, on two occasions a fine silkworm gut bougie was got through the stricture, to the slight improvement of the flow of urine. These attempts occupied more than a fortnight, in which time the patient's sufferings were treated by warm baths, rest in bed, suppositories, and subcutaneous injection of morphia.

On Sept. 22nd, Mr. Hill succeeded in passing a No. 2 silver catheter under chloroform. This was tied in, and replaced the next day by a flexible one. In a few days, when the irritation had subsided, the patient was again put under chloroform, and the dilator passed, a No. 13 catheter being immediately introduced, tied in, and the urine drawn off. No reaction took place.

The patient was trained to pass an instrument for himself, and was discharged on October 6th, passing No. 12 easily, his urine being quite clear and free from albumen. He lay all night without passing urine.

Case 2.—A groom, aged twenty-six; admitted Oct. 5th, 1871. Nine years before he had contracted traumatic stricture by falling astride a bar. For the pain, bleeding from the urethra, and inability to pass water, he was treated at Guy's Hospital. Five years later, retention having again occurred, he went to Guy's, and his stricture was again dilated by the daily passage of instruments. After each discharge from hospital he neglected the injunction he had received to pass a bougie from time to time. When admitted to University College Hospital, a large, hard mass was found in the perineum, and the patient was passing urine every quarter of an hour; urine clear. After some difficulty Mr. Hill passed a No. 1 flexible catheter through a stricture four inches and three-quarters beyond the meatus. This was tied in; and on the 12th the stricture was easily split to No. 14, and a No. 12 catheter tied in for twenty-four hours. In a few days the patient could pass a catheter for himself, make a good stream, and was discharged with the strict injunction not again to neglect the occasional passage of a bougie, No. 10, which was given to him.

These cases have been selected from several others treated by the dilator as samples of copious induration of the urethra giving way instantaneously before the wedge on the exertion of very little force. As will be seen by a glance at the drawing, the instrument consists of a split sound which equals the calibre of a No. 2 or No. 3 catheter. The halves



of the split sound can be separated by passing between them a wedge fixed on a slender stem. The wedge is prevented by two dovetail grooves at its hinder part from leaving the sound, which acts as the guide during the passage of the wedge down the urethra. When the instrument is used, the urethra is first explored by an olive-headed bougie, graduated on its slender stem, by which the situation of the stricture is ascertained. The split sound is next passed along the urethra till the beak reaches the bladder. The wedge is then inserted between the blades, and pushed swiftly down till it has passed the stricture. It is then withdrawn, the blades of the sound fall together, and can be removed easily. A full-sized No. 12 or No. 13 catheter is at once passed into the bladder to draw off the urine, and may be taken out or tied in for twenty-four hours. On the third day a No. 12 or No. 13 bougie is passed, and again every other day for a week, while the patient learns to use the instrument himself, and not to let more than a fortnight pass without doing so. The instrument is adapted for the same cases as Mr. Holt's—namely, strictures situated in the bulbous portion, not in the penile part, where the division of a stricture should be a clean cut, in order that the cicatrix may as little as possible block up the erectile tissue and interfere with erection.

The advantages claimed for this instrument are: simplicity of construction; the central guide of Holt's instrument is not needed, hence the split sound can be passed through narrower strictures. Next, and chiefly, diminution of resistance, and, consequently, the more immediate application of the rupturing force—the wedge—to the impediment to be overcome. The force needed to push Holt's dilator is sometimes very great, and the attempt has been abandoned, or the instrument has broken, even in skilful hands, from this cause. Much of the force is expended on the continuous friction outside the tube along the split sound, and inside along the guide. In the wedge dilator the friction surface is reduced to two dovetail grooves, which together do not exceed half an inch. For this the force required is so small that one hand suffices to overcome the resistance.

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SEAMEN'S HOSPITAL, GREENWICH.

DORSAL DISLOCATION OF HIP OF FIFTEEN DAYS' STANDING REDUCED BY MANIPULATION.

(Under the care of Mr. W. JOHNSON SMITH.)

THE subject of this case is a Swedish sailor, aged twenty-seven years, who, fifteen days before his admission into the Seamen's Hospital, was struck violently over the back of the pelvis, the body at the time being bent forwards. The man states that he was thrown down, and that, in the fall, the left thigh was forced outwards to a considerable extent.

On the day of his admission, February 6th, 1872, the patient presented well-marked symptoms of dislocation of the head of the left femur upon the dorsum ilii, the limb being shortened and adducted, and the foot much inverted. In addition to the ordinary symptoms of this injury there was complete paralysis of the muscles below the knee, and also loss of sensation in the integument of the leg, except over its inner surface and along the inner side of the foot. This condition had lasted ever since the accident.

Chloroform having been administered, reduction of the dislocated bone was attempted by flexing, abducting, and externally rotating the thigh. After very forcible abduction, the head of the femur slipped audibly into its socket, and the normal length and relative position of the limb were restored. The thighs were then bound together by a few turns of a bandage. On the following morning it was found that the head of the femur was again out of place, although not to so great an extent as after the accident. It was easily returned by manipulation without the administration of chloroform, and the limb was retained by a long splint, applied as in fracture of the femur. At the end of a week this was removed, and the patient allowed to get up. Locomotion was prevented for some time by the paralysis of the muscles of the foot. This was treated by galvanism, and the man is now able to walk very fairly with the aid of a stick. There is no perceptible difference between the two limbs as to length and position, and the movements of the left hip are perfect.

GONORRHŒAL INFLAMMATION OF AN IMPERFECTLY DESCENDED TESTIS; TREATMENT BY PUNCTURE.

(Under the care of Mr. W. JOHNSON SMITH.)

William M——, aged nineteen was admitted on January 17th with a painful swelling in the right groin. At the end of December, 1871, he had contracted a gonorrhœa, which was treated by copaiba. About five days before admission the urethral discharge suddenly ceased, and shortly afterwards the young man complained of tenderness in the groin, which speedily increased in intensity, and prevented him from working.

On admission, the left testicle was fully developed, and in its normal position on the corresponding side of the scrotum. The right side of the scrotum was empty. In the right groin, between the situations of the external and internal rings, was a firm rounded swelling, of about the size of a small apple. This was extremely painful and tender, and the integument stretched over it very red, hot, and œdematous.

On January 18th a deep stab was made into the swelling with a sharp-pointed bistoury. After some slight hœmorrhage had ceased, the affected groin was left without local application of any kind.

On Jan. 19th the patient stated that the pain was much less severe. The swelling could be handled and compressed without causing much uneasiness.

20th.—There is no pain in the groin, and the œdema and redness of the integument have disappeared.

The urethral, discharge which became profuse after the cessation of the epididymitis, was treated by the oil of yellow sandal wood, and on Jan. 27th the patient was discharged as cured.

LARGE SUBLINGUAL CYST TREATED BY PUNCTURE AND DRAINAGE.

(Under the care of Mr. W. JOHNSON SMITH.)

E. Z——, a Maltese, aged fifteen years, was admitted on February 14th, 1872, with a large swelling under the jaw on the right side, and considerable protrusion of the tongue. On examination these signs were found to be due to a large cyst evidently containing fluid, which was very prominent at the floor of the mouth, and passed backwards and downwards into the neck, where it formed a large fluctuating tumour, over which the skin was tightly stretched. No information of any interest could be obtained from the patient, except the fact that about a week before his admission the swelling in the mouth had been pricked by a surgeon in the south of England.

An incision was made into the tumour below the tongue, and about four ounces of blood-stained and viscid fluid let out. When the cyst was half emptied, a doubled silk thread was passed through its cavity by means of a needle set in a handle, which was introduced at the incision made in the mouth, and brought out through the skin under the jaw. The ends of the silk thread were then tied together outside the mouth, and the fluid contents of the tumour allowed to trickle away slowly from the orifices through which it passed.

On February 16th the silk seton was replaced by a drainage-tube, which was passed from within outwards through the tumour and the ends looped together under the chin. This set up suppuration, which did not spread beyond the walls of the cyst, and caused no inflammation or irritation of any kind in the surrounding soft structures. The discharge of pus, which at first was profuse, gradually diminished, and the seat of the cyst was marked by a hard movable swelling. On February 29th the drainage-tube was removed, and the discharge then became thin and scanty, and in the course of the following three days ceased altogether. The patient was discharge cured on March 7th, with no signs of the affection save a hard nodular swelling along the former track of the seton, and this for some days previously had been rapidly diminishing in extent.

For the notes of the preceding cases we are indebted to Mr. H. Cambell Pope, the house-surgeon.

LEUKÆMIA LYMPHATICA.

(Under the care of Dr. STEPHEN H. WARD.)

For the notes of the following case we are indebted to Mr. Adam Young, house-physician.

L. L——, aged twenty-four, was admitted into hospital March 21st. He gave the following history :—Four months ago he noticed some enlarged glands in the left axilla; about a week afterwards those of the right became enlarged; then in about a week or a fortnight the glands in the neck and about the jaw increased in size. There was no pain in them at first; when he raised his arms at work the glands in the axilla were painful. He attributed the enlargement of these glands to his being overworked when at Callao; at this time he was badly and scantily fed. Five weeks ago he went into the Cork Hospital; at this time his breathing was oppressed and hurried, and he was also troubled with a frequent tickling cough. He had some difficulty in swallowing his food, and complained of a tightness in his throat. "as if the pipes were constricted," he said. Till this present illness he has always had good health.

State on admission.—The patient is generally emaciated, extremely weak, and anæmic. The muscles of the legs are much wasted. The glands in the neck and about the jaw are immensely enlarged, as also those in the axillæ. His head has the *appearance* of being unusually small, and looks as if it were resting upon an immense base of enlarged glands. There is no enlargement of the inguinal glands. The left eyelid is œdematous, and the whole face pale and puffy. When the patient lies on his back or on the left side, the face becomes quite livid and the breathing greatly oppressed and quickened. Respiration 48 in a minute. He has a frequent harsh cough. There is a fair resonance over all the chest; respiration blowing; some crepitation at the base of both lungs. No night-sweats. Pulse 86, small and weak. Heart's action somewhat increased; no murmur. The abdomen is swollen and pendulous; the prepuce œdematous. The chest walls are œdematous; the superficial veins slightly enlarged and tortuous. The spleen is very little if at all increased in size; rather tender on pressure. Liver slightly enlarged.

On microscopical examination of the blood the white cells are seen to be greatly increased in number, as many as 200 being counted in one field. Urine: sp. gr. 1034, acid; urates and chlorides abundant; no sugar; no albumen.

From the 23rd to the 29th of March the condition of the patient was very little changed. Pulse 120; temperature 104°; breathing less embarrassed.

29th.—Breathing less embarrassed; has had a good night; swallows food with greater ease; pulse 113; temperature 103·4°. Measurement round the head in a line with the chin, 1 ft. 11 in.; measurement with tape passed under the chin and over the vertex, 2 ft. 8½ in. There is not so much œdema about the face.

April 6th.—Patient died this morning apparently from exhaustion; no special change had occurred in his condition since admission.

The treatment consisted of a drachm of the syrup of the iodide of iron three times a day; and good diet with wine.

Autopsy forty-eight hours after death.—Body much emaciated. The right pleural cavity contained five pints of clear straw-colored fluid; the intercostal and mammary glands enlarged; there were deposits under the pleura, on the under surface of the ribs, and on the lung. The left pleural cavity contained four pints of straw-colored fluid; and there were numerous growths of the same character as those on the right side. Several large glands were seen upon the diaphragm, in front of the pericardium, and others around the great vessels at the base of the heart. The posterior mediastinal and bronchial glands were greatly enlarged. The right lung was compressed against the spinal column; the left lung was healthy. The pericardium contained a considerable quantity of fluid; the heart was small, weighing 9½ oz.; cavities contracted, containing no clot. Mesenteric glands greatly enlarged; stomach healthy; both kidneys congested; liver congested; spleen not increased in size and apparently healthy, weight 6¼ oz. The axillary and external mammary glands greatly enlarged; the superficial and deep cervical, and the submaxillary glands were immensely increased in size.

The microscopic examination of a section taken from one of the cervical glands showed numbers of small round cells thickly packed together, and embedded in a fibrous network, the bands of which were in some places broad and very clearly defined.

MIDDLESEX HOSPITAL.

(Cases under the care of Dr. John Murray.)

The following cases, met with in the out-patient department of the above hospital, are worthy of note:—

Epileptiform Seizures of fifty years' standing relieved by Bromide of Potassium.—A hale-looking and intelligent shoemaker, sixty-eight years of age, presented himself for advice, complaining of sudden attacks of "confusion in the head," occurring chiefly, but not invariably, in bed by night, and never lasting more than four minutes. For fifty years he had suffered from these attacks at intervals of three or four months; each visitation representing a variable number of fits, as he called them, and lasting over a week. The fit is sudden and without any aura. If walking during the onset of the fit he has difficulty in keeping his legs, but he has never fallen down or completely lost his consciousness; if he is attacked during sleep he is at once wakened up. The fit is not accompanied by convulsion or pain; he becomes very hot, and the attack passes off with profuse perspiration. The fits may occur several times during the twenty-four hours. With this exception he has always enjoyed good health, and has no cardiac disease. He has been treated in very many ways for his attacks, by blistering, cupping, cold shower-baths, &c. Dr. Murray, entertaining the idea that the case was of an epileptiform character, ordered fifteen-grain doses of bromide of potassium three times daily. This was on February 13th. On each of the three previous nights the patient had had five or six attacks, besides two or three during each day. On the first night after taking the bromide of potassium, however, he had only three or four, and he slept much better; up to March 1st there has been no recurrence whatever of the attacks. He has still some "deficiency" in walking, but says that he has never been so free from the attacks since he was first seized with them.

Extract of Nettles in Whooping-cough; peculiar rash following its administration.—There are at present several children suffering from whooping-cough who are being treated by extract of urtica, an old and popular remedy for that disease, and which Dr. Murray observed had been used in this affection, apparently with success, in India. Although the drug had been given in about a dozen cases, the evidence that it was a remedy was most equivocal. In one case, however, that of a boy eleven years of age, who the mother said had commenced to whoop only two or three days previously, and who had slight bronchitis, the extract was given in half-drachm doses three times a day, commencing in the forenoon of February 16th. On the following day the face became very puffy, and the skin over the whole body swollen, red, and very itchy. This lasted during the 18th, and the mother, getting alarmed, and believing that the mischief was due to the medicine, took the boy to the dispenser, who corroborated the mother's account of the swollen face. The mother now gave only half the quantity of medicine ordered. The swelling had disappeared by the 20th, when he was seen by Dr. Murray, but he had not whooped since he took the first dose of the medicine, and his cough had been much better. The patient continued the diminished doses of the medicine for a week without any return of the whoop, and the bronchitis had almost entirely disappeared. The existence of pertussis in this case is doubtful, inasmuch as the mother's account alone cannot be depended upon, and the other characteristic symptoms of the disease were not evident. The fact, however, of the general affection of the skin appearing whilst taking the urtica is interesting.

Cases of Sick Headache treated successfully by Indian Hemp.—Under care at present are two female patients, the one forty-one, the other fifty years of age, and both married, who have been for many years subject to sick headache. They have been greatly benefited by the administration of ten-minim doses of tincture of Indian hemp three times daily, between the attacks. The hereditary character of the affection is well exemplified in both. In one case there is a history of the mother and four sisters suffering from similar attacks; while the other patient states that her aunt was subject to sick headaches and died insane, that a sister suffered from frequent severe headaches without sickness, and that a brother was a deaf mute and had sick headaches.

COMPOUND COMMINUTED FRACTURE OF THE LEG; FOUR ATTACKS OF ERYSIPELAS; PLEURISY; TREATMENT BY ANTISEPTICS; GOOD RESULT.

(Under the care of Mr. Hulke.)

T. D——, aged thirty-six, a stout, heavy baker, admitted July 11th, 1871, with a compound fracture of the left leg, caused by slipping off the step of an omnibus. An oblique wound of the soft parts, at the inner side of the leg, about two inches long, communicated with the fracture, which was a hand's-breadth above the ankle. The wound was washed with a watery solution of carbolic acid (1 to 40), and closed with a pledget of lint soaked in carbolised oil, and covered with a layer of gutta-percha and plaster. The limb was placed on a Bell's back splint, with side splints, and ice continually applied. On admission the pulse was 72, temperature 97·6° F. On Feb. 13th the temperature was 102·6°. In consequence of hæmorrhage the dressings had to be changed. On the 14th the pulse was 132, temperature 103·4°; leg slightly swollen. On the 15th the temperature was 105°, the pulse 124, full and hard. Some cough and oozing of blood from the wound still continued. Some ecchymosis observable, extending up the leg to the upper third. On the 18th the leg was considerably swollen and red, with a bulla on the external malleolus. His urine was high-colored, and contained some bile, and yellowness of conjunctiva was observable. Pulse 104, temperature 103°. On the 19th the erysipelas had increased. A lotion of sulphate of iron (ten grains to an ounce) was ordered to be applied to the limb. The next day the erysipelas had increased, and there was much bronchitis. Incisions were made in the limb, and a lotion of chloralum was applied. On the 21st a mercurial purgative was given, and the incisions were washed out with a solution of sulphurious acid (1 to 8). From this date he steadily improved till July 31st, when the pulse was 100, and the temperature 98·5°. A splinter of bone had been removed from the leg on the 28th, and the erysipelas had quite subsided.

On August 5th he had some return of his troubles, and two days later his temperature rose to 102°. He had sore-throat, some cellulitis on the inside of the thigh, and a slough formed on his heel. On the 23rd this condition of things had subsided. The splint was readjusted, the leg looked quiet, the incisions still discharged, no union had taken place. The wound was still syringed with the sulphurous acid lotion, and dressed with unguentum cretæ.

On Sept. 7th the erysipelas returned for the third time; the leg was very red and swollen, the redness extending along the course of the lymphatics about the knee to the upper part of the thigh. He was ordered a dose of calomel and a mixture containing perchloride of iron, and the leg was dressed with sulphate-of-zinc lotion. This continued till the 18th, when his temperature rose to 104°; and on the following day he was seized with pleurisy of the left side. The zinc lotion was continued till the 25th, when all redness had disappeared, but there still remained much brawny hardness and swelling of the leg and thigh.

He continued to improve till Oct. 23rd, at which date the sinuses and incisions had healed except opposite the fracture, where there was a sequestrum. A starch bandage was applied, the pressure of which caused rapid diminution in the size of the limb, so that on the 27th it had to be "taken in."

He went on well till November 25th, when, the wound opposite the fracture not having healed, it was dilated with a laminaria tent, and a sequestrum was removed. He now went on well with the exception of a fourth slighter attack of erysipelas, which was treated in the same manner as the former one, and with like results.

On December 21st he was discharged convalescent, with a useful limb.

We are much indebted to Mr. Barber for the great assistance afforded us in obtaining the notes of the above case.

LONDON HOSPITAL.

REMARKS ON DIFFICULTIES IN THE DIAGNOSIS OF THE CAUSES OF APOPLEXY, BY DR. HUGHLINGS JACKSON.

THE following remarks are *apropos* of cases in which the diagnosis of the cause of apoplexy has been difficult or impossible. No particular case is referred to; cases of difficulty are common enough.

The term apoplexy may be made to include all cases of suddenly occurring coma; and thus among the causes of apoplexy are such very different things as intracranial hæmorrhage (cerebral or meningeal), injuries to the head, uræmia, opium-poisoning, and drunkenness. Apoplexies from these several causes are so like that very able men occasionally make mistakes in diagnosis—such, for example, as declaring a case of apoplexy to be owing to poisoning by opium, when a large clot in the pons Varolii is found post mortem to be the real cause. Again, patients the subjects of cerebral hæmorrhage are now and then supposed to be only drunk. Nor is this to be wondered at, since the apoplectic condition ("only a symptom") may be the same from each of these causes. Students should be told plainly that diagnosis is in many cases not difficult only, but impossible. To such a remark it is often replied that in *most cases* our prediction is right. No one denies *that;* but what are these "most cases"? If we find the patient hemiplegic, or if we learn that his apoplexy began by hemiplegia, or that he had a convulsion, we infer, whatever further difficulties there may be, that the case is not one of opium-poisoning or of drunkenness. If we hear that the patient had received a severe blow on the head, traumatic apoplexy is likely enough. If a patient known to be the subject of renal disease slowly passes into coma, we say with confidence that there is uræmia. But it is almost an abuse of language to say that *we* make a diagnosis in such cases. It is as correct to say that the diagnosis is made for us. We speak now of the most difficult cases: for example, of patients found insensible in the streets, or of patients about whom we receive inaccurate or contradictory histories—more misleading than no histories at all. If a patient is brought to us universally powerless and deeply comatose, and if we have no history of the mode of onset, we really cannot in many cases make a diagnosis. In particular, we cannot answer the important question, Is the apoplexy the result of cerebral hæmorrhage or of drunkenness?—for the simple reason that a large quantity of alcohol will produce a condition identical with that which results from large cerebral hæmorrhage. Those who doubt this last observation should note carefully the condition of a patient who has been "sucking the monkey"—i.e., sucking wine or spirits out of a cask by aid of a piece of gas-piping.

It is not meant that we are to rest in this stage. On the contrary, the acknowlegement of the difficulties should stimulate us to more work. In those cases where the circumstances make the diagnosis for us, we should raise artificial difficulties; we should urge the student to ignore the history, and see if he can make a diagnosis from the *condition of the patient himself.* We should say to him, "We know that this patient's apoplexy is owing to alcohol, because he was found sucking raw spirits out of a cask at the docks; but suppose the patient had been found in the street comatose, how then would you tell what was the matter with him?" If the student were told plainly the difficulties in diagnosis, and if he were to begin by studying cases in a realistic manner, he might in after years discover means by which a diagnosis in the true sense could be made. From looks he will learn the symptoms of cerebral hæmorrhage those the result of injuries to the head, those of deep intoxication, &c. But when he knows these (we will assume that he knows them thoroughly) he will find that he is not so much helped as he might suppose when he is called to the bedside of an apoplectic patient; for the problem is then, so to speak, inverted. The patient is apoplectic: to which one of the many causes of apoplexy is his apoplexy owing? The student will, when he stands in this most responsible position, begin to find the real value of certain helps in diagnosis beginning by "if." "If the patient smells of drink, &c."—why then he may be either drunk or he may have taken a glass of brandy for premonitory symptoms of cerebral hæmorrhage. "If you find bruises on the head, &c."; but patients die of fatal injuries to the brain when no external bruises have been discoverable ante mortem. "If there be albumen in the urine, &c.," the apoplexy may be owing either to cerebral hæmorrhage or to uræmia. "If the patient has been uproarious, swearing, &c.," he may be either drunk or the subject of intracranial hæmorrhage (idiopathic or traumatic). Again, patients die apoplectic in whose bodies post mortem no cause for the apoplexy is discoverable, unless the commonly found venous congestion is allowed to make the diagnosis of "congestion of the brain." or the large quantity of serum filling the gap left by wasted convolutions—really a chronic affair—that of "effusion of serum."

Such a negative conclusion is naturally not liked; but no one is asked to accept it as a final conclusion. It is only asserted that, so far as we have yet learned, the diagnosis is often impossible. Nor has it been said that we cannot *guess* what is the matter. Possibly those who rely on "smell of drink," &c., will mostly be right. But since in the strict sense they do not make a diagnosis at all, they are only biding their time until guessing ceases to be lucky. The provisionally negative conclusion (or confession of ignorance) has at any rate this value, that (being aware that we really cannot tell in many cases the apoplexy of drunkenness from the apoplexy owing to more fatal causes) we shall be led to take care of every apoplectic patient. If we do not, some of our patients whom we have declared to be "only drunk" may die under the care of someone "who never has any difficulties in such cases," and who "is surprised to learn" that the real nature of the case was not known at the first. But it is "at first" that the diagnosis is most difficult. A case which nobody can diagnose "at first" may be clear to anybody in a few hours. Here it may be remarked that police surgeons and the resident medical officers of our hospitals see apoplectic patients in the most difficult stages—cases which may be clear enough when the physician visits the hospital next day. Those who wish to encounter the most difficult cases should arrange to be called at once to all patients brought apoplectic to the receiving-room. If anyone who freely admits that sad mistakes are made by *other* people, but who feels great confidence in his *own* powers of diagnosis, will do this, he will probably cease to wonder that the diagnosis of cerebral hæmorrhage had not been promptly made in cases of apoplexy which next day were plainly enough owing to that lesion, and wonderfully simple *after* the autopsy.

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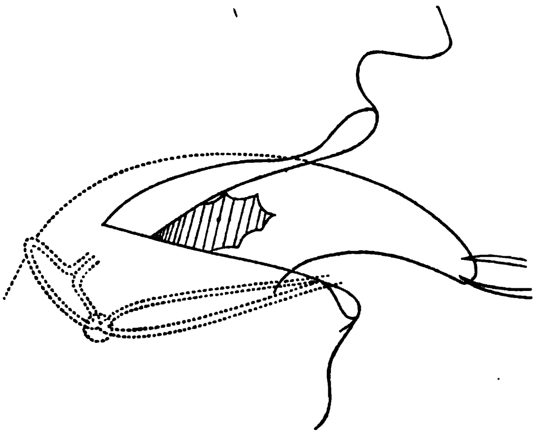
QUEEN'S HOSPITAL, BIRMINGHAM.

REMOVAL OF CANCEROUS TONGUE; RECOVERY.

(Under the care of Mr. Furneaux Jordan.)

On January 27th Mr. Jordan removed a cancerous tongue by a method somewhat different from that ordinarily employed. The cheek was divided in the way advocated by Prof. McLeod, of Glasgow; thus the cheek was transfixed just in front of the ramus of the jaw, and the knife carried to the angle of the mouth. The arteries were closed by torsion, and a troublesome vein by acupressure. The tongue was then seized close to its root between the finger and thumb of one hand, while the other carried under the tongue a strong curved needle threaded with four pieces of ligature. These drew back through the channel made by the needle the chains of two écraseurs, one after the other. One écraseur was fixed so as to divide the tongue near the larynx, the other was to separate it from the floor of the mouth. The instruments were worked simultaneously, and in about seventeen minutes clean removal was effected, absolutely without hæmorrhage. Dr. Underhill kept the patient efficiently under chloroform during the whole operation. The facial wound was closed by silver sutures.



The double dotted lines represent the position of the chains of the ecraseurs.

At his visit on February 9th Mr. Jordan remarked as follows:—It is not quite a fortnight since the operation, and the patient, who gets up every day, says she was never better in her life. Her articulation is fairly intelligible. The facial wound healed by the first intention, and leaves only a line of cicatrix, not very important, under the circumstances, in a woman past middle age. Through the uncut mouth the tongue cannot be thoroughly removed. The method of applying the écraseur through an incision in front of the hyoid bone has this serious drawback: it removes the submaxillary tissues (quite remote from the cancer) down to the submaxillary skin, leaving no floor to the mouth, and thus necessarily interfering with deglutition and articulation. You see here in the floor of the mouth a healthy, clean, cicatrising, convex (by muscular action) surface. I believe the method I have adopted has advantages over those of Syme, Regnoli, and others.

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HOMERTON FEVER HOSPITAL.

THE EFFECT OF QUININE ON THE TEMPERATURE IN ENTERIC FEVER.

By the kindness of Mr. Percival, the assistant medical officer, we were able to inspect the records of a couple of cases of enteric fever (both children) in which quinine was given for the purpose of depressing the temperature. Both of these cases showed that a fall in temperature had repeatedly taken place immediately after the administration of the quinine. The quinine was given in three-grain doses every four hours, and at no time were symptoms of cinchonism induced.

One case, in which the effect of quinine was particularly well-warked, was that of a little girl aged seven, who was admitted on the evening of the ninth day of an attack of enteric fever. Her temperature on admission was 105° F. Quinine (in the doses mentioned above) was at once given. The temperature immediately began to fall, and continued to do so till the morning of the eleventh day, when it had reached 97° F. (a fall of eight degrees). The quinine was then discontined, and the temperature immediately began to rise, and by the evening of the twelfth day had reached its old point of 105°. Quinine was again given, and by the middle of the fourteenth day the temperature had fallen again to 97° F. The dose of quinine was now much diminished (almost discontinued) and by the middle of the fifteenth day the temperature was 103° F. The old dose of quinine was renewed, and by the middle of the sixteenth day the temperature was 98°, and on the day following (seventeeth) 97·5°. Quinine was now completely discontinued, and the temperature rose (more slowly than on previous occasions) and towards evening on the nineteenth day stood at 101° F., when the quinine was recommenced and the temperature again fell, reaching 98·5° on the twentieth and 98° on the twenty-first day, after which there was no further rise, the quinine being entirely discontinued on the twenty-second day.

The other case was similar in all points of importance to the one we have given. The administration of quinine in both these cases was generally followed by profuse sweating.

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BIRMINGHAM AND MIDLAND HOSPITAL FOR WOMEN.

CASE OF PUERPERAL ECLAMPSIA AT THE FIFTH MONTH OF PREGNANCY.

(Under the care of Mr. Lawson Tait.)

J. C——aged twenty-seven, the mother of two children, and to whom two miscarriages had occurred, applied at the hospital on February 12th. She complained of pain in the back occasioned by a fall. The cause of the fall she asserted to be a sudden loss of consciousness, which lasted for some time. She stated that she was within a month of her full time—a circumstance which led to the examination of her urine. This was found to be intensely albuminous, and there can be no doubt that the fall was occasioned really by a uræmic convulsion. She was ordered fifteen-drop doses of the tincture of the muriate of iron and half a drachm of nitric ether in an ounce of water, every four hours, and advised to call in medical assistance at the earliest indication of labour. At three o'clock on the morning of the 15th she was seen by Mr. Tait and was found to be in labour. The pregnancy had not, however, advanced to the sixth month. The urine was found to be only slightly albuminous, but, as a matter of precaution, Mr. Tait kept her under the influence of chloroform for about two hours. Throughout the 16th, 17th, and 18th, the uterine action seemed in abeyance, but on the 19th she was delivered of a putrid fœtus of about the fifth month. Her labour was superintended by Dr. Poncia, who reports that there were no indications of eclampsia, and that she has recovered well.

Remarks.—We have here a very unusual condition of puerperal uræmia occurring early in pregnancy, and in a multipara, causing as usual the death of the fœtus, and discovered by the merest accident. Had the patient gone untreated, there is every reason to believe that a most serious risk of her life would have occurred. Sir James Simpson pointed out the value of the combination of iron and nitric ether, and this case seems to have been one in which its efficacy was well marked.

Medical Societies.

JUNE—JULY.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Mr. T. B. Curling, F.R.S., President, in the Chair.

ACCOUNT OF A CASE OF RESECTION OF THE SHOULDER AND ELBOW JOINTS IN THE SAME ARM, FOR GUNSHOT INJURIES.

BY WM. MAC CORMAC, F.R.C.S.,

Assistant-Surgeon to St. Thomas's Hospital.

This paper detailed the history of a French soldier who had been wounded at the battle of Sedan. The right arm, in the region of the shoulder and elbow joints, was extensively injured, the soft parts being much lacerated and the bones extensively comminuted by a shell explosion. The question arose whether it was at all possible to save the limb. Rather, however, than perform disarticulation at the shoulder-joint, Mr. Mac Cormac determined to excise both the shoulder and elbow joints—an operation hitherto, he believed, unique. The patient narrowly escaped death from pyæmia, but he afterwards made a satisfactory recovery; and the interest of the case centres in the amount of usefulness subsequently enjoyed by the limb. Some portions of necrosed humerus have yet to come away, and in the deltoid region a sinus still remains. The elbow is soundly healed, and regeneration of the excised bones has taken place. The operation was performed subperiosteally; and in the after-treatment the limb was supported on carefully adjusted cushions—a method Mr. Mac Cormac prefers in most cases to the employment of splints. The shoulder can now, after an interval of eighteen months, be moved freely backwards and forwards, but not raised from the side, the deltoid muscle having been almost completely destroyed by the original injury. Flexion, extension, and rotation are very perfect in the elbow, and the usefulness of the hand is nearly as good as before. Four inches of the upper end of the humerus, very greatly comminuted, were removed; and a long piece of ulna, in addition to the head of the radius and the articulating condyloid surface of the humerus, was taken away. Mr. Mac Cormac considers that he may fairly classify this case as one of successful double resection of the two principal joints in the same upper extremity.

The President remarked that the case was probably unique, and as such very interesting.

Mr. Croft thought that splints were worse than useless in cases of resection in the upper extremity; had always used sand-bags with very good effect; and asked the author what special means were adopted to keep the limb quiet.

Mr. Timothy Holmes thought that all excisions might be treated without splints; but he preferred in elbow cases to use a very light splint, which really did no harm, and was specially adapted for children. He quoted a case in which no splints were used, a result being that the ends of the bone eventually projected from the wound. He asked for Mr. Mac Cormac's experiences as to subperiosteal operations; believed that the amount of bone saved was often hurtful rather than beneficial; instanced a case in point; and remarked that in successful excisions of the head of the humerus the arm can always be moved some way from the trunk, though the want of power in this case may have been caused by the great amount of injury done to the deltoid muscle.

Mr. John Wood had discarded splints for some time; and, ten days after the operation, commenced and continued to alter the position of the limb once every twenty-four hours, believing that with careful and frequent manipulation a very fair olecranon may be produced. He presumed that the question of splints or no splints entirely depended upon whether a movable or an anchylosed joint was required.

Mr. Spencer Wells quoted a case that had occurred to a continental surgeon at Metz, in which a subperiosteal section of the entire humerus was successfully made. He believed that in active service splints were necessary, and that plaster-of-Paris bandages answered exceedingly well.

Mr. Thomas Smith failed to comprehend how splints could be safely or properly dispensed with, and averred that with children they were absolutely necessary.

Mr. Maunder thought that the question of splints wholly depended upon the sort of joint desired. He rarely used splints at all for the elbow, except in cases of children, and then only during the first week, but always applied them in knee cases, though he considered it an open question in excisions of the hip.

Mr. Callender was surprised that no reference had been made to the very capital anterior splint used in America, and asked Mr. Mac Cormac why no effort was made to save the limb in its entirety.

A Visitor (an American surgeon) said that the bent wire splint was much used in the United States, and believed that, in subperiosteal operations, mischief was sometimes caused by the subsequent growth of awkward spiculæ, which might, and indeed sometimes did, endanger arteries.

Mr. Savory commented sarcastically on the varied experiences and the various facts that had been enunciated in the course of the discussion. Believing it to be an axiom in surgery that repose was necessary to repair, he was surprised to hear surgeons advocating a "perpetual motion" style of practice with reference to excised joints. He also took exception to the title of the paper, inasmuch as about four inches of the humerus and a large mass of the ulna were removed by the operation.

Mr. Mac Cormac, in replying, answered this last objection by remarking that the fragments alluded to were detached, lying in the wound bathed in pus; so that there was no option as to their removal. And with respect to an observation by Mr. Callender, Mr. Mac Cormac pointed out that excision of the joint was the recognised method of treatment for gunshot injuries of the shoulder, and that it was attended by a far smaller proportionate rate of mortality than was conservative treatment—thirty-five per cent., as compared with fifty-two per cent. according to Billroth's tables.

ON THE SURGICAL TREATMENT OF SUPPURATING OVARIAN CYSTS, AND ON PELVIC ADHESIONS IN OVARIOTOMY.

BY T. HOLMES, F.R.C.S.,

Surgeon to St. George's Hospital.

A case was related in which chronic suppuration occurred in an ovarian tumour, after paracentesis had been performed for the first time. Ovariotomy was postponed for some months on account of the patient's condition. When it was performed the cyst was found extensively adherent in other directions, and so tightly wedged into the pelvis that it was impossible to reach its pedicle. It contained about a gallon and a half of fluid, of which about half was pure pus. The remains of the emptied cyst were dragged out of the abdomen, a clamp was applied to its neck (at a distance above the pedicle which could not be accurately ascertained), and the wound was closed. The patient recovered, and after her recovery no sinus was left, nor was any tumour to be felt. The symptoms of acute and chronic suppuration in ovarian cysts were discussed, and it was attempted to be shown that if the general condition admits of it the suspicion of suppuration is a reason for performing the operation instead of delaying it. The case was also used to show that, in some instances, the results of ovariotomy may be perfectly favorable, though pelvic adhesions have prevented the complete delivery of the tumour. If the neck of the cyst admits of being embraced in a clamp, the lower portion of it may be obliterated during the healing of the wound. The superiority of this method, when feasible, to the other courses which may be pursued in dealing with pelvic adhesions, was shown.

Mr. Bryant remarked that the accurate diagnosis of a suppurating ovarian cyst was most important; that wasting, a hot skin, a permanently high temperature, bad appetite, local pain and tenderness on pressure, all indicated suppuration. If any doubt as to the diagnosis exists, he would be still more induced to interfere quickly, to remove the cyst if possible, or to take away as much as could be removed. He quoted a case to show the desirability of not leaving ligatures, and one also that showed the useful peculiarities of catgut.

Mr. Spencer Wells thought that with care the diagnosis between a suppurating cyst and peritonitis could be ascertained without great difficulty. An elevated temperature night and morning, with or without increase of pain on pressure, were two positive signs of the presence of a suppurating cyst. And if peritonitis exist operative procedure is still more decisively indicated. A very serious case under his care, in conjunction with Drs. Farre and Watson, in which peritonitis occurred from the bursting of the cyst into the abdominal cavity, ultimately did well. He considered it undesirable to separate adhesions before tapping the cyst, that as much should be removed as possible, and that it was a good plan to leave a drainage-tube in the wound after the operation, to give free exit to the suppurative discharges.

Mr. Holmes, in replying, doubted if the diagnostic signs spoken of by Mr. Wells were positive as well as negative, because in some cases high temperature exists without suppuration, and in some cases of peritonitis the temperature has been persistently high.

THE ACQUIRED DEFORMITIES OF THE UTERUS; THEIR IMPORTANCE, EFFECTS, AND RESULTS, WITH A STATISTICAL ACCOUNT OF OBSERVATIONS ON THE SUBJECT AT UNIVERSITY COLLEGE HOSPITAL FROM AUG., 1865, TO DEC., 1869.

BY GRAILY HEWITT, M.D., F.R.C.P.,

Professor of Midwifery and Diseases of Women, University College; Examiner in Obstetric Medicine, University of London.

The object of the author was to demonstrate by facts and observations the importance of acquired alteration in the *form* of the uterus as a disease, and to establish for the *acquired deformities* of the uterus a prominent position in uterine pathology. To establish the frequency of acquired deformity of the uterus, the author brought forward statistics of the whole of the practice in the department for diseases of women at University College Hospital during a period of upwards of four years (1865—1869). These statistics, arranged in tabular form, comprised particulars concerning 1205 patients; from which it was shown that, of these 1205 patients, 714 were affected with symptoms referable, in one way or other, to the uterus.

Of the 714 patients presenting symptoms referable to the uterus, the diagnosis of the condition present was aided by a physical examination in 624 cases. In 90 other such cases no examination was made.

The 624 cases examined and presenting uterine symptoms are arranged in three classes:—First, general, including 65 cases; second, organic &c., including 182 cases; and third, acquired deformities, or change of position of the uterus, 377—making a total of 624. The first class include absence of uterus &c., 6; amenorrhœa and vicarious menstruation, 9; menorrhagia, 7; periuterine hæmatocele, 11; leucorrhœa, 12; hypertrophy of cervix uteri, 18; climacteric disorder, 2 cases—total, 65 cases. The second class include fibroid tumour or polypi, 96; cancer, 54; pelvic cellulitis, 32 cases—total, 182. The third class: flexions, 296 (retroflexions 112, anteflexions 184); prolapsus, 81—total, 377. From which it was shown that there were very few of those cases presenting uterine symptoms in which the uterus was not affected with a decided change of shape; this change of shape being, in the author's opinion, the underlying and principal element in the cases in question.

In the next place the author submitted particulars of each of the 296 flexion cases in a tabular form, comprising the age, condition, and main facts relating to the 296 cases. These cases were then analysed to show the severity of the symptoms and principal effects. The prevalence of *sterility* and the frequency of *abortion* were especially pointed out, and the following results obtained:—Of married there were 235 cases (100 retroflexion, 135 anteflexion). Of the 100 retroflexion cases, 21 were sterile, 12 had had no pregnancy, 9 had abortions but no child; of the 135 anteflexion cases, 60 were sterile, 45 had no pregnancy, 15 had abortions but no child. In reference to the frequency of abortion in the 296 acquired deformity cases, the results were as follows:—Of the 100 retroflexion cases (married), 24 had abortions; total number of abortions was 32; greatest number in one case was 3. Of the 135 anteflexion cases (married), 27 had abortions; total number of abortions was upwards of 54; and the greatest number in one case was 9.

Dysmenorrhœa was almost constantly present in these 296 cases of acquired deformity. Menorrhagia was very common. Chronic uterine inflammation was also common, but was evidently secondary to the alteration in shape. When miscarriages were observed, and retention of portions of the ovum occurred, such retention was very generally traced to the existence of flexion.

The *varieties* of acquired uterine deformity were then described, referable to two principal causes—bending of the uterus forward or backwards; the importance of the varying conditions of the uterine walls in regard to density, consistence, and thickness was next indicated; the connexion subsisting between these alterations in shape and the occurrence of prolapsus pointed out; the very important effects on the circulation in the uterus and on the nervous relations and susceptibilities of the organ were also indicated.

Together with the paper a series of nearly 100 outline drawings, exhibiting pictorially the diagnosis made at the time of the examination, was laid before the meeting.

Dr. Routh concurred with Dr. Hewitt's general conclusions, but took exception to some of them—such as that anteflexion and retroflexion were the main causes of dysmenorrhœa. In reality there were only a few cases of that kind. Besides, most unmarried women had their uterus more or less anteverted, and in them this condition gave rise necessarily to no discomfort. He did not think the diagnosis of the condition of anteversion difficult. He considered that dysmenorrhœa without flexion was more common than Dr. Hewitt said. The paper did not distinguish between congenital and acquired flexions. He thought the author ignored the former. In these there was often no pain during menstruation, and the same was sometimes true of the acquired condition. It was only when the fundus became inflamed that pain was produced. Some women with flexed uteri were sterile, some not; and the former only because the os was turned in the wrong direction. What was the meaning of "prolapse" as used by Dr. Hewitt? In reality prolapse was rare; the condition commonly mistaken for it—namely, elongation—was not.

Dr. Day asked what were the miscellaneous cases tabulated. He thought that many diseases of the uterine system and of the chest in young women were united or allied. Amenorrhœa was often only a symptom of the general condition.

Mr. R. J. Lee could understand the origin of the retroflexion following on a parturition when the woman gets up too soon. The uterus would then naturally fall backwards. He thought Dr. G. Hewitt referred to those of mechanical origin only. What was the difference between the congenital and acquired conditions?

Dr. Graily Hewitt, in reply, said he was quite aware that slight anteflexion existed in the virgin, or, rather, in the child. If badly fed, it might remain after puberty, but it was not natural in a well-developed young woman. With regard to anteflexion, he did not say the diagnosis was difficult; he only said it was often not made. In almost all the cases of flexion dysmenorrhœa was a marked feature. His experience as to congenital flexions was quite different from that of Dr. Routh. It was true that the painful symptoms only began when the fundus became inflamed, but that occurred in almost every case, and went away when the position was rectified. His sterile cases did not include those who were sterile after the birth of one child. By prolapse he meant a substantial descent of the uterus, and its appearance externally. He agreed with Dr. Day that a goodly number of cases of amenorrhœa in young women were associated with other bodily states. His cases comprehended unmarried who had no pregnancy as well as married. In the former the change in shape was due to a loss of tonicity or rigidity in the uterus, which allowed it to assume any shape. Their sedentary occupations might cause it to be anteflected. Flexion was rare without some definite cause.

ON A CASE OF ABDOMINAL ANEURISM CURED BY COMPRESSION OF THE AORTA.

BY WALTER MOXON, M.D.,
Assistant-Physician to Guy's Hospital;
AND
ARTHUR E. DURHAM, F.R.C.S.,
Assistant-Surgeon to Guy's Hospital.

The patient, a comparatively healthy-looking young man, twenty-seven years of age, presented himself among Mr. Durham's out patients at Guy's Hospital on August 2nd, 1871. On examination he was found to be suffering from an abdominal aneurism of considerable size, the pulsation and intumescence of which were manifest to the eye as well as to the touch. He was recommended for immediate admission, and was placed under the care of Dr. Moxon in the clinical ward. The aneurism could be distinctly felt over a space extending from rather less than an inch below the cartilages of the fixed false ribs above to a level with the umbilicus below, and from the right side of the median line across to about midway between the median line and the left border of the abdomen or rather further. Pulsation was full and strong, but could be controlled by careful and deep digital pressure. The patient was kept perfectly at rest in bed for eleven days, and on very spare diet. At 10.30 A.M. on the 14th August (no food having been taken since the preceding evening) chloroform was administered, and Mr. Durham proceeded to compress the aorta on the proximal side of the aneurism by means of Lister's abdominal tourniquet. There was just room to get the pad of the tourniquet between the cartilages of the ribs and the aneurism. The tourniquet was screwed down very slowly, and carefully adjusted in position and direction until the pulsation of the aneurism as well as that of both femoral arteries was completely arrested. The lower extremities were enveloped in cotton wool, and flannel and hot-water bottles were placed in the bed. The compression was absolutely maintained for ten hours and a half, the patient being all the time kept under the influence of chloroform. At the end of that period his general condition, as indicated by pulse and respiration, was such as seemed to render it undesirable to continue the treatment. The tourniquet was accordingly removed, and no more chloroform was administered. No pulsation of the aneurism nor of the femorals could be detected. A bright-red patch marked the spot where the pad of the tourniquet had compressed the skin. The lower extremities were cold, and marked here and there by purplish or livid patches, but they were in more favorable condition than they had appeared to be at an earlier period during the compression. In the course of a short time pulsation of the aneurism could be again detected; but the aneurism remained much smaller and harder than before the treatment. The patient passed a good night, and the next day had little or nothing to complain of except the exquisite tenderness of the skin at the compressed spot. There was no internal pain or tenderness, nor any indication whatever of mischief done to the visceral or other internal structures. It seemed remarkable that such severe treatment should be followed by so marked an absence of all serious disturbance, whether local or constitutional. No bad symptom of any kind arose. The aneurism gradually became firmer and smaller, and its pulsation less and less perceptible. At the end of rather more than a month, pulsation could no longer be detected. The femoral arteries, in which, as well as in the aneurism, pulsation had to some extent temporarily returned—had also ceased to pulsate; the aneurismal tumour gradually diminished in size; the patient recovered health and strength, and, indeed, was apparently in far better condition than he had been for a long time previously to his admission to the hospital. He was presented for examination by the Fellows present.

In commenting upon this case, the authors remarked that, so far as they were aware, it was only the second case of the kind upon record in which a similar method of treatment had been followed by similarly successful results. The first case occurred in the practice of Dr. Murray, of Newcastle (to whom they wished to ascribe all the credit of having initiated the method of treating abdominal aneurisms they had thus adopted). It appeared probable that in their case, as in that of Dr. Murray, the aneurism was connected with the aorta at or about, and probably involving, the inferior mesenteric. In such cases this method was most applicable. In another case, however, now under their care at Guy's Hospital, there was reason to believe that the aneurism was upon the superior mesenteric. Nevertheless, they hoped to succeed in applying this method, and with satisfactory result. Sphygmographic tracings were taken by Mr. Mahomed during an experimental attempt made by Mr. Durham to compress the aorta in the case referred to. Mere tracings showed in a most striking manner the effect produced upon the pulse at the wrist by compression of the abdominal aorta or immediately connected parts.

In conclusion, the authors expressed their opinion that the method described should be carefully, but fully, tried in all cases of abdominal aneurism (few though such cases be) in which it may be found practicable to compress the aorta on the proximal side of the aneurism in such way and to such extent as to arrest its pulsation, and at the same time without the exercise of such force as might seriously damage the viscera or other internal parts. On anatomical grounds compression of the aorta on the distal side of an aneurism would not appear to offer promise of much success.

A CASE OF ABDOMINAL ANEURISM TREATED BY DISTAL PRESSURE, WITH REMARKS.

BY THOMAS BRYANT, F.R.C.S.,

Surgeon to Guy's Hospital.

The patient was one of Dr. Pavy's, who asked Mr. Bryant to see the case with a view to treatment. It was a man aged thirty, with an abdominal aneurism in the epigastric region the size of a fist. Distal pressure was determined upon, as no other treatment was applicable. It was applied by means of a Lister's abdominal tourniquet, with the patient under chloroform. It was kept up for twelve hours, then removed for twelve hours, and reapplied for four hours; the treatment being given up as the man's powers seemed failing. He died eleven hours after the removal of the clamp. After death peritonitis was found to have been the cause of death from the bruising of the bowel and peritôneum by the clamp. An aneurism of the cœliac axis was present, which was filled with a perfect clot, evidently of recent date but ante mortem.

The post-mortem was made by Dr. Moxon, whose report was read.

In his remarks, Mr. Bryant pointed out how the danger of peritonitis seemed to be one that appertained to the application of pressure by an abdominal tourniquet under all circumstances, and was to be accepted as a danger which should always be considered. He dwelt upon the pathological fact, which the preparation and drawing from it taken from the patient illustrated, that a large abdominal aneurism may undergo mechanical closure by a clot from the application of distal pressure for twelve or sixteen hours. The case suggested the value of other forms of practice upon the distal side of an aneurism which are based upon the same principle, such as the temporary occlusion of an artery by ligature, acupressure-pins, or other means; for if, he remarked, pressure for a few hours upon the afferent or efferent artery of an aneurism—sufficient to arrest the flow of blood through its channel—is enough to bring about the mechanical closure of a sacculated aneurism by means of a clot, and consequently its cure, surely the application of other means which are calculated to fulfil the same purpose may be employed to effect the same end. He then alluded to Hunter's, Astley Cooper's, and Travers's temporary ligatures, and to Lister's carbolised catgut ligatures, which he regarded as temporary ligatures. He went on to show how contused arteries often became occluded ones, and that this was probably from some detachment of the inner coats. He suggested the adoption of some instrumental means by which these results were to be secured—by which the inner and middle coats of an artery might be divided and allowed to recurve without destruction of the external. He then alluded to the artery constrictor of Dr. F. Spiers, of New York, and showed the instrument, stating that his own experiment had gone to prove the truth of Dr. Spiers's statement. He believed that we had in this instrument an element of usefulness that wanted working out. He concluded by showing how successful the distal treatment of aneurism had been under certain circumstances, and expressed a belief that it was a form of practice well worthy of renewed attention in cases where all other forms of practice were inapplicable.

Mr. J. Astley Bloxam, F.R.C.S., surgical registrar at St. Bartholomew's Hospital, also brought forward a case of Aneurism of the Abdominal Aorta at its bifurcation, and involving the left common iliac, treated by pressure of the abdominal aorta, and which was followed by embolism and death. The case was under the care of Sir James Paget, Bart., at St. Bartholomew's Hospital in the year 1870. The patient, aged twenty-nine, was admitted on February 23rd. He had suffered with pain in the back and loins for eighteen months, and occasionally he had numbness in the lower extremities after running. When admitted the tumur presented the following appearance: it was irregularly oval, with its long diameter directed towards the left common iliac, equally firm all over, and pulsating very forcibly in all directions. Pressure was applied, with the aid of chloroform, two inches above the umbilicus, no less than four times within a period of twenty-four days, on the following dates: March 4th for two hours and twenty minutes; 8th, for one hour and thirty minutes; 14th, for four hours; 21st, for six hours. In each period it was especially remarked by Sir James Paget and all present how immediately the circulation was affected by the pressure. At the last operation the pulse became almost indistinguishable at the wrist, and remained so for hours, and on all occasions the arterial circulation seemed to have contracted to its greatest extent, and at the same time became exceedingly rapid, and at times intermittent. After the 21st, the last date on which the pressure was applied, he had frequent attacks of vomiting, and retched up a little blood, and appeared in a moribund condition up to the 25th, when he rallied a little. At this period he complained much of pain over the seat of pressure, became rapidly emaciated, and died on March 29th, eight days after the last application of the pressure. At the post-mortem, local peritonitis had taken place about the pancreas and spleen, and an abscess had formed in the gastro-splenic omentum. The superior mesenteric artery was found for more than an inch flattened out, of a dark purple color, its coats infiltrated, and on opening it, on the internal coat were clots of blood, firmly adherent; but a channel existed through the artery. The arteries to the spleen and pancreas were branches of it, and it appeared to Mr. Bloxam that the breaking up of their structure into abscesses might be the result of emboli, as several were detected along the main trunk of the superior mesenteric. The aneurism itself had eroded the bodies of the lumbar vertebræ, and was full of laminated clots. Mr. Bloxam simply drew attention to the peculiar condition of the circulation when pressure was applied over or near the semilunar ganglia.

Dr. Dobell exhibited two instruments, having for their object to reduce the intra-vesicular pressure in the chest, and that resulting from it on the surface of the diaphragm, and to effect a more complete change in the reserve air of the lungs, and thus indirectly of the residual air, in cases of emphysema, chronic bronchitis, carbonic-acid poisoning, and the like. The first instrument was invented by Dr. Berkart, and consists of a set of inspiratory and expiratory valves communicating between an orinasal mask and an air-pump. Dr. Dobell's instrument is of simpler construction than an ordinary respirator, and, though not so large, somewhat resembles it in shape. It is made by Maw, and is called a "Residual Air-pump." That of Dr. Berkart is manufactured by Hawksley, and is called an "Expirator."

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CLINICAL SOCIETY OF LONDON.

Sir W. W. Gull, Bart., F.R.S., President, in the Chair.

Dr. Hermann Weber read a paper "On a Case of Hyperpyrexia in Rheumatic Fever successfully treated by Cold Baths and Affusions." The patient was a youth aged sixteen, who had rheumatic fever (first attack) in August, 1871. The affection of the joints was well marked, but not excessive. The temperature of the body varied between the ninth and twelfth days of the disease from 101·6° to 102·2° F.; the pulse between 98 and 118; the respirations between 20 and 24. The lungs were free, and there was only the slightest indication of a murmur with the first sound near the apex. The medicinal treatment consisted of three grains of quinine three times a day. On the morning of the thirteenth day great restlessness, vomiting, excessive micturition, involuntary motions, delirium, tendency to coma, and lividity of face, supervened; simultaneously with a rise of temperature from 101·6° F. on the previous evening to 108·2°, of the frequency of pulse from 118 to 148, and that of respiration from 22 to 56. The patient was then placed in a bath of 71° F., and affusions of water of the same temperature were made. The mental condition and the appearance of the patient rapidly improved already during the first ten minutes; and at the termination of thirty minutes when he was removed from the bath, the temperature in the cavity of the mouth was only 101·8°, and further sank during the next half hour to 98·8°; while the patient fell asleep, and began to perspire freely. In the course of the same afternoon the temperature again began to rise rapidly—i.e., from 100·8° at 3 P.M. to 105·8° at 6.40 P.M. Pulse 148; respirations 38; slight delirium. The patient had then another bath with affusion, as in the morning, when the temperature further rose during the first five minutes of the bath to 106·2°, but afterwards fell rapidly, the mercury receding within twenty-five minutes (the duration of the bath) to 101°, and during the following hours to 98°. After the second bath without any further medicinal or hydrotherapeutic treatment the disease took the course of a usual mild form of rheumatic fever, leading to perfect recovery, leaving only a rather too long first sound. Dr Weber repeated the view stated in a former communication to the Society—that the nature of these attacks was the same as in common heat-stroke or insolatio; and thought that the "hyperpyrexia," as employed by Dr. Wilson Fox, more appropriate than heat-stroke, which conveyed the idea of causation by external heat. Acknowledging that hyperpyrexia was an accident which could occur in all febrile diseases, he maintained that it occurred infinitely more often in rheumatic fever than in any other disease; and, regarding the peculiarities of rheumatic fever, was inclined to ascribe it to the tendency to endocarditis and fibrinous deposits existing in this disease. He suggested that it might have a similar origin as chorea, and might possible be due to minute embolism, or to plugging of some small vessels in a certain portion or the nervous system, as the hypothetical centre of animal heat and chemical changes. As to prognosis and treatment, he pointed out that all cases formerly had a fatal termination when once the temperature had reached 108° Fahr; but that the hydrotherapeutic plan, if early and energetically pursued, and with watchfulness as to the possibility of several attacks, gave great promise of a speedy cure, as shown by the two complicated and yet successful cases published by Dr. Wilson Fox, the one by Dr. Meding and the one by the author.

Dr. Greenhow related particulars of a case, the subject of which died after treatment by cold affusion. On the fourth day of the acute attack the temperature was 101·5°, and pulse 116. Affusion was tried on the sixth day, the temperature then being 105·2°. Wet sheets were applied at night with temperature of body at 109°, and a reduction was effected to 104°, and subsequently to 101°. The patient then slept quietly for some hours; awoke with a temperature of 107°, and died suddenly the next morning, the heat of the skin being 107° almost immediately before death. He believed, however, that in this instance the treatment had not been fairly tried or properly pursued, and intended to repeat the trial systematically as soon as an opportunity offered.

Dr. Andrew had never seen such a case, and asked for information as to the therapeutic treatment adopted before hyperpyrexia supervened.

Dr. Greenhow replied that he was no believer in the mint-water scheme of treatment, and that, according to his knowledge and belief, alkalies were given, and, probably, one or more doses of quinine.

Dr. Wilson Fox thought that hyperpyrexia ought, as a most important subject, to be discussed independently of the treatment adopted, inasmuch as the condition may occur under any circumstances, and under any plan of treatment; that since clinical investigations had been aided by the thermometer, the propriety of cold affusions had been progressively confirmed by facts, and that though we know that in most fevers the course of the disease cannot be shortened, it is equally well known now that a temperature of 108° and upwards indicates a certainly fatal termination. It is therefore the duty of the profession to make known and to practise a plan of treatment that has, under these circumstances, saved the lives of at least five or six patients. Hyperpyrexia may possibly be a phenomenon of acute rheumatism alone; a case of puerperal fever has died under the affusion, and the treatment appears to have been unsuccessful in small-pox. He failed, however, to see how, if cerebral lesions exist, cooling the skin can afford relief.

Dr. Anstie, alluding to the existence of hyperpyrexia in diseases other than acute rheumatism, commented upon a fatal case of delirium tremens that had occurred in his own practice, in which the temperature rose to 109°, and so remained until death. A fatal termination was also indicated by a double undulatory pulse-wave shown by the sphygmograph.

Dr. Southey thought that some statis in the circulation might be the cause of the high temperature, and that, as the collateral blood-flux must be greatly increased by suppression of circulation through the skin, benefit might take place by the application of cold to the surface.

Dr. A. P. Stewart remarked that a typhoid state sometimes accompanied severe attacks of acute rheumatism, and spoke of a fatal case which occurred with a temperature of 109°, fluid blood being found after death in all cavities and vessels. He thought the plan indicated by Dr. Weber valuable, and worthy of an extended and careful trial.

Dr. C. J. B. Williams had only met with two fatal cases of acute rheumatism during his career as a hospital physician, thought that much had yet to be ascertained as to hyperpyrexia, and supposed that high temperatures indicated some affection of the nervous system, commencing at the outset of the disease. He remarked that a temperature of 104° stopped the vital motions of what he had termed "sarcophytes," and was disposed to come to the conclusion either that some change of general constitution has occurred during the last thirty or forty years, or that the condition called hyperpyrexia is due to the mode of treatment adopted.

Dr. Wilson Fox explained that, by a coincidence, Dr. Ringer was recording temperatures with one set of patients, and iron was being prescribed in other cases at the same time for the same disorder, so that the two sets of observations and results accidentally received special prominence.

Dr. Weber replied briefly, asserting that, according to a series of German statistics most carefully collated, it was shown that treatment by cold affusion diminished the duration of the disease, and reduced mortality.

Mr. Teevan read a paper on the Treatment of four Cases of Impassable Stricture. In each case, frequent attacks of gonorrhœa had been the cause of the stricture; and it was worthy of note that in no case had an injection ever been used for the cure of the gonorrhœa. The patient aged forty-five, had for many years suffered from severe stricture and frequent attacks of retention. Many surgeons had in vain tried to pass an instrument. He was ordered to rest in bed for several weeks, during which period Mr. Teevan made many attempts, without success, to get through the stricture. Leeches were then freely applied to the perineum, and afterwards a No ½ silver catheter was successfully passed into the bladder. Gradual dilatation by means of French bougies completely cured this patient. The second man, aged fifty-four, had for ten years passed all his urine through a fistula in the perineum. After a six weeks' confinement to bed, a very small whale-bone olivary bougie was passed into the bladder, and the patient was untimately cured of his fistula and stricture by gradual dilatation. The third sufferer, aged forty, voided all his urine through three fistulous openings in the rectum, perineum, and scrotum. Repeated trials with various kinds of instruments under the very favorable condition of complete and long rest utterly failed; and on January 22nd last, Mr. Teevan divided the diseased structures in the median line without any guide, and passed a large silver catheter into the bladder. All the urine comes by the penis, and the wound and fistulæ are nearly healed. The fourth case was that of a man aged fifty-six, who had a similar cutting operation performed on him four years ago for an impassable stricture complicated with fistulæ. The stricture was cured, the fistulæ healed up, and at the present time a large silver catheter can be passed into the bladder with ease.

Mr. MacCormac asked for particulars as to the symptoms that followed the incision from the anus towards the meatus, as described by Mr. Teevan, and the after treatment.

Mr. Maunder criticised all the cases adduced, and took exception to the plan of making incisions without the aid of any guiding instrument. There was no risk in puncturing the bladder per rectum, but pyæmia and secondary hæmorrhage were very likely to occur in laying open the perineum.

Mr. Heath supposed that there were as many modes of treating stricture as surgeons to treat the disorder, and commented upon the circumstance of Mr. Teevan's using whalebone instead of soft bougies, which he last year so strongly recommended, and prophesied that he would probably return to silver instruments in cases of tight stricture. He referred to the late Mr. Syme's operation for stricture with perineal fistula, and subscribed to Mr. Maunder's remarks as to the objectionable nature of cutting operations.

Mr. Barwell agreed with the remarks of the last two speakers, and said that leeching the perineum was by no means an unusual practice, Solly, Green, and South having frequently prescribed them in the treatment of stricture.

Mr. Thomas Smith said that he was in the habit of directing his patients to hold their water as long as possible, and usually succeeded in passing a small instrument into the bladder during the act of micturition.

Mr. Teevan replied briefly, quoted Sir. H. Thompson in defending his plan of making extensive incisions, and inveighed against all operative procedure that involved laceration instead of clean cuts.

A paper by Dr. Cockle and Mr. Christopher Heath was read, on "A Case of Aneurism of the Arch of the Aorta, treated by Ligature of the Left Common Carotid Artery." The patient (who was brought before the Society) was a man aged forty-eight, a farm-laborer from Cambridgeshire, who came under Dr. Cockle's care in January, 1872. He had experienced pain in the right side of the head, neck, shoulder, and chest, for four or five years, attributed to rheumatism. In April, 1871, he noticed a pulsating swelling in the hollow of his neck, and was in Addenbrooke's Hospital for two months without relief. After Christmas, 1871, he was compelled to give up work, from the increasing pain in the shoulder and neck. He presented marked symptoms of aneurism of the ascending and transverse portions of the arch of the aorta. There was displacement of the right sterno-clavicular articulation, with projection of the right and upper portion of the sternum; and the episternal notch was filled by a pulsating swelling. There was a strongly heaving and expansile impulse over the whole of the swelling, and marked dulness on percussion existed over the whole area of the tumour. No bruit could be heard, but a double concussion-shock was felt. The respiratory murmur was feeble or absent over the upper portion of the right lung. The hearts sounds were dull and muffled, but without appreciable murmur. There was no cough or difficulty of deglutition. The left radial pulse was decidedly feebler than the right; and the left pupil was much dilated, and the left eye congested. On exertion, he complained of shortness of breath, palpitation, and pain. Mr. Heath tied the left carotid artery above the omohyoid on February 26th, 1872. Carbolised catgut was used for the ligature, and the wound was covered with cotton wool. No constitutional disturbance followed. Within forty-eight hours, the patient could lie and sleep on his right side, which he had been unable to do before. The left pupil became natural, and the congestion of the eye disappeared. On the eighth day when the dressings were removed for the first time, the wound was found completely healed by first intention. Since the operation, the condition of the patient had in every way improved, the chest having become flatter, and the heaving impulse greatly diminished. The patient was free from all pain or inconvenience. He was brought before the Society before returning to the country, and the authors proposed to report upon his condition at a later period.

Dr. Anstie remarked that the two pairs of sphygmographic tracings (right and left radial) which were taken by him respectively before and after the operation showed interesting results. In the pre-operation traces that of the left was very much feeble and smaller than that of the right radial. After the operation this difference was greatly reduced; a large diversion of the blood-current into the left limb had evidently taken place.

Mr. Barwell asked for a more precise history of the case, and

Mr. Heath said that it would be remembered that in 1865 he had tied the right carotid and subclavian arteries in a woman with aortic aneurism, who was undoubtedly benefited by the operation, and lived four years after. Since that time Dr. Cockle and he had been anxious to try the effect of tying the left carotid in a case of aortic aneurism, on the ground that in two recorded cases of ligature of that vessel for supposed carotid aneurism the post-mortem examination some months after revealed an aortic aneurism filled with laminated fibrine and cured. The object in the present case had been to tie the branch next beyond the disease, and so far the result had been favorable. Mr. Heath demurred to the suggestion that rest alone might have produce the good effect, on the ground that so short a time had elapsed since the operation, and that the patient had by no means been kept strictly at rest.

The Chairman thought that the proper plan would be to appoint a committee of observation, and suggested Dr. Anstie and Mr. Barwell, who at once retired and examined the patient. It was also agreed that Dr. Humphry should be communicated with, in order that the patient might be kept under continuous observation.

Mr. Gant read a paper "On Spontaneous Gangrene of both Feet in a Boy." The patient was a messenger-boy, aged sixteen, who, when he was about seven years old, had a decidedly bluish aspect, was always breathless, and complained of feeling cold even in warm weather. The boy himself stated that he had suffered from shortness of breath, with occasional dyspnœa, ever since he could remember. Otherwise, the boy's general health had remained unaffected. Latterly, he had been much exposed to cold during very severe weather. After a day's exposure, during the night, when in bed, his feet "burned very much"; and the next morning, the great toe and two adjacent toes of both feet were nearly black, and quite insensible. In this state he walked to the Royal Free Hospital. After admission gangrene progressed until it reached the dorsum of both feet, presenting to some extent the appearance of senile gangrene. But the livid blueness and œdema of both legs, up to the knees, contrasted with the pale shrunken condition of the legs as observed in senile gangrene; and the gangrene itself, at this early stage, was more succulent or moist than dry. Pulsation was quite free in both the tibial arteries of the feet. A physical examination of the chest was made by Dr. Cockle. The heart's action was feeble, but regular and unaccompanied by any appreciable murmur. Pulsation could be readily seen in both carotids, and a diffused forcible pulsation in the external jugular veins. Pulse at the wrist 27 per minute, small, feeble, irregular. The only pulmonary signs were emphysema, and slight bronchitis at the bases of both lungs. Respiration was oppressed, 16 per minute. The temperature in the axilla was, morning 103·8°, evening 101·6°. The urine was scanty and high-colored, with a copious deposit of greyish-brown lithates and of albumen. The patient was lethargic, dull of understanding, and spoke with a stridulous voice and broken articulation. Occasionally, when the heart's action became very feeble and dyspnœa very urgent, fits of unconsciousness occurred, accompanied on one such occasion with epileptic convulsive movements of the upper limbs and face, this state lasting two hours. A line of demarcation having formed on the dorsum of the feet, Mr. Gant performed double amputation by modifications of Chopart's and Hey's operations. Both stumps united soundly in a week. But the cardiac and pulmonary conditions recurred at intervals with increasing severity; and in a fortnight the patient sank into a state of coma, and died quietly. The post-mortem examination showed that the heart, in its right half, was the seat of disease, the origin of which would seem to have been at a very early period of life. The right auriculo-ventricular opening was enlarged sufficiently to admit two or even three fingers, but the three segments of the valve were healthy; the auricular cavity was dilated and distended with blood to more than twice its natural size; the wall of the auricle, not thickened in any part of its extent, was, between the musculi pectinati, reduced to a membranous state; the foramen ovale was entirely closed; the right ventricular cavity was also somewhat enlarged, and equally distended with blood but the wall was not hypertrophied. The pulmonary valves were quite healthy. On the left side of the heart no morbid condition was discovered, either as regards the cavities or the valves. Recent pericarditis had occurred. Both lungs were emphysematous, but the texture generally was much congested, and the bronchial tubes were engorged throughout with frothy mucus. Recent pleurisy had taken place on both sides of the chest. The abdominal viscera generally were much congested, and the liver and kidneys considerably enlarged by interstitial deposit, passing into fatty degeneration. The brain exhibited marked venous congestion. Mr. Gant was disposed to draw the following two conclusions:—1. That the gangrene of both feet, depending evidently on systemic venous congestion, was caused by the enlarged state of the right auriculo-ventricular opening; and the accompanying dilatation of the auricle and ventricle, being thus essentially cardiac, although the immediate and exciting cause might have been exposure to cold. 2. That the immediate cause of death was pericarditis, pleurisy, and, more particularly, capillary bronchitis.

After an interval of profound silence, the Chairman remarked on the exhaustive nature of the paper.

Dr. Broadbent took exception to the cause of the mischief as set forth by Mr. Gant, hinted that the mode of death indicated renal disease, and that death was probably due to the latter, the cardial affection being secondary.

The Chairman observed that no record appeared as to the specific gravity of the urine.

Dr. Thomas Jones asked if ascites existed.

Dr. Cholmeley asked if the vessels of the extremities were closely examined.

Dr. Broadbent read a paper "On Tumour of the Pons Varolii and Medulla." The patient was a man aged forty-six, with a syphilitic history and aspect, who, while cutting grass in the sun, was suddenly seized with giddiness, and suffered afterwards from pain in the head, vertigo, double vision, and facial distortion. He entered the hospital six weeks after the attack, and was then found to have slight weakness of the right limbs; marked paralysis of the left side of the face, of the kind seen in hemiplegia, and not involving the orbicularis oculi, as in Bell's paralysis; lateral deviation of both eyes to the right, with double loss of vision; sensation over the area of the right trigemnius; difficulty of deglutition, and indistinct articulation, the voice also having a nasal twang, due to partial paralysis of the soft palate. The ophthalmoscopic signs were negative. There was no albuminuria, and no heart disease. The diagnosis given was a tumour in the pons and medulla. He remained under treatment for about three weeks, taking iodide of potassium in doses gradually increased to sixteen grains three times a day. The headache, from which he continued to suffer on admission, was greatly relieved, and he walked much better. At the end of this time he insisted on leaving the hospital, and a week later he was found one morning dead in his chair, having got out of bed in the night to smoke. The only morbid appearances discovered were two small tumours just beneath the floor of the fourth ventricle, near the median line—one in the lower half of the pons, the other near the lower end of the ventricle. The tumour in the medulla undoubtedly gave rise to the dysphagia, and was the cause of the sudden death, as it was in close relation with the nuclei of the glosso-pharyngeal and pneumogastric nerves. The question of greatest interest was, whether the tumour in the pons could have given rise to the paralysis in the left side of the face and the lateral deviation of the eyes to the right, as well as to the anæsthesia of the right side of the face and head. Had the paralysis simply been of the sixth and seventh nerves, from lesion involving their common nucleus, there would have been no difficulty; but both the facial and ocular paralysis were such as is met with in hemiplegia, and, if caused by the tumour in the pons, could only be due to interruption of the fibres which connect the nucleus of the sixth and seventh nerves with the corpus striatum of the opposite side immediately after their decussation. No lesion was discovered, by most careful search, either in the crus or corpus striatum; and the author was inclined to believe that the explanation given above was the true one, and the more so because the lateral deviation of the eyes, usually a fugitive phenomenon, was here persistent.

Dr. Buzzard referred to the experiments of Adamick, which showed that irritation of the corpora quadrigemina, and especially of the *nates*, would cause lateral deviation of the eyes, a symptom which, considering also its occasional appearance in the convulsions of unilateral epilepsy, he thought more likely to be spasmodic than paralytic in character. He asked whether the corpora quadrigemina had been examined.

Mr. Brudenell Carter asked whether the ophthalmoscopic examination had been made by the erect or the inverted image. He was convinced that Dr. Allbutt was right in saying that the fine changes visible by the former method might altogether elude observation if the latter only were employed.

Dr. Broadbent, in reply, stated that he had not used the direct method, but had made careful and repeated examinations under the old system, and did not think that anything of importance could have been missed.

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PATHOLOGICAL SOCIETY OF LONDON.

MR. JOHN HILTON, F.R.S., President, in the Chair.

MR. T. SMITH showed part of an Aorta with Aneurism of the sinuses of Valsalva. This specimen was taken from a man who died after ligature of the right subclavian for axillary aneurism. He then spoke of the clubbing of the fingers of the right hand in the same case. (Some casts of feet and hands showing clubbing of the toes and fingers were handed round.) On admission into the hospital the clubbing was well marked; after rest, and keeping the hand raised for four or five days, it became less, and entirely disappeared after ligature of the artery. In Mr. Gay's case of double aneurism of the fingers of both hands were clubbed. In his case the clubbing was apparently due to local congestion, as the axillary vein was found on autopsy flattened out by the pressure of the aneurism. We may conclude that clubbing of the fingers is due to obstruction in the pulmonary circulation or the systemic veins, or to anything obstructing the blood-stream, as seen in congenital malformation of the heart and thoracic aneurism, also in emphysema; it has also been met with in the course of a pneumonia, passing away on recovery. Suppurative diseases did not seem to produce it, unless there was suppuration in the thoracic cavity. Only in one case had he seen clubbing following disease of bone, that of a child suffering from disease of the pelvic bones, and here there was no obstruction to the circulation.

Dr. Squire said that neither emphysema nor asthma was an unfrequent cause.

Dr. Moxon said that clubbing of the fingers was of obscure origin. It bears upon the important fact to which Dr. Wilks drew attention—that, when people are ill, the nails are thickened, the thickening passing away on convalescence. He thought venous congestion from obstruction was a cause; but one cause was not enough, as seen in emphysema. Aortic disease frequently produced it, so also chronic phthisis.

The President asked Dr. Moxon if he had examined such fingers; and if so, what was the condition?

Dr. Moxon replied that he had never made sections; but, on post-mortem examination, the nail and bulb of the fingers were enlarged, and the parts below shrivelled.

Dr. Pye-Smith did not think clubbing was due to œdema. If so at post-mortem examination it would have passed away. When there was marked clubbing of fingers, the toes were the same, as though from a common cause.

Dr. Douglas Powell thought there was more than congestion, as patients complain of tingling at the ends of the fingers. In a few cases he had seen clubbing come on acutely (i.e., in two or three weeks) in advancing phthisis. It was rare to find any extreme clubbing of the fingers without the same condition being present in the toes.

Dr. Crisp thought it might be explained by cold causing congestion of parts, and so leading to enlargement.

The President spoke of a case of chest-disease in which there was clubbing of the fingers. Here keeping the arms raised did not remove the condition.

Mr. T. Smith, in reply, said he agreed with Dr. Moxon that clubbing of the fingers did not arise purely from congestion, nor from any one cause. He did not think it resulted from cold.

Dr. King showed a Malformed Heart taken from a boy, aged four, who died from abscess of the brain. The boy had never been cyanotic, and enjoyed good health till two years ago; since that time he had suffered from pain in the head, and lately a purulent discharge from the right ear. Eight days ago the left arm and leg became paralysed; he had a succession of fits, in one of which he died. A systolic murmur was heard at the apex during life. The two ventricles were found communicating at the base, the septum there being deficient. The foramen ovale was closed; the right auricle and ventricle were hypotrophied; the valves diseased. He thought it was the result of endocarditis during intra-uterine life.

The President spoke of a heart he had seen many years ago at Chatham Hospital. It had been removed from a soldier about thirty years of age. There was a large opening in the septum. The man had never been cyanosed. He had always been able to do his duty. He died suddenly after a long march.

Mr. Sydney Jones showed a tumour, weighing 4 lb., removed from the parotid region. The patient was a lady, aged fifty-eight. Mr. Travers, twenty-eight years ago, removed from the same region a tumour the size of a plum. A few months after she accidently received a blow on that region. At the end of five years there was a tumour the size of an orange. This gave little inconvenience till fourteen months ago, when the skin ulcerated, and hæmorrhage occurred. Since then there had been frequent recurrence of hæmorrhage. In March free arterial hæmorrhage came on. When seen she was blanched, and her pulse very feeble, and it was evident that some operation must be performed. The tumour was pedunculated, pedicle about three inches thick. A clamp was applied to the pedicle, and the tumour removed. The patient had now nearly recovered. On section there was a large development of cartilaginous tissue, common in these tumours; some parts were softer, presenting gland structure; there were a number of cysts containing fluid and gelatinous matter interspersed through the mass. Drawings of the microscopical appearances were handed round.

Mr. Hulke said that the drawings presented the appearance of the nest-like arrangement of the cells of an epithelioma.

The specimen was referred to the Morbid Growths Committee.

Dr. Crisp showed a part of the Wall of the Left Ventricle of a Heart, in the substance of which were three abscesses. The specimen was taken from a girl aged four. She had been in good health till February 24th, when she wore tight boots, which produced an ulcer on the right heel. This soon got well. Shortly after she complained of great pain in the middle of the right thigh; there was swelling and fluctuation; the pain was intense, and narcotics were of no use. The temperature was very high, 104° F. and 105°; pulse from 150 to 180. Abscesses came in various parts—the right temple, then the left, then on the chest; and the joints became swollen and painful. She died on the eleventh day. The lungs were hepatised and adherent.

Mr. T. Smith asked if the thigh was examined.

Dr. Crisp said he was not allowed to do so.

Mr. T. Smith thought it was a case of pyæmia arising from disease of the thigh-bone, and not from the heel.

Dr. Moxon said there was one point of interest in such cases—namely, the tendency of suppurative periostitis to cause abscess in the heart and kidneys, mentioning cases he had seen, and referring to eight cases in which Dr. Bristowe had found the same condition. In all cases of suppurative periostitis he examined the heart for abscesses. He did not think it arose from the severity of the pyæmia, for in other cases where the pyæmia was very acute and speedily caused death, the heart and kidneys were not affected, but rather that pyæmia arising in certain parts showed a tendency to affect certain other parts, analogous to cancer.

Dr. Payne said he had lately seen three cases of pyæmia without external wound, arising from periostitis and necrosis. The heart was not affected in any.

Dr. Crisp showed a largely Distended Colon from a patient who died from obstruction of the bowels. Post-mortem examination showed that the obstruction was caused by the rolling over of the colon, the meso-colon being very long; the colon was found distended with blood, about one pint and a half; no ulceration was detected. When he saw the patient, twenty-four hours before death, the abdomen was distended, and the colon distended and prominent. He suggested that puncture of the intestine with a fine trocar should be performed in similar cases.

Dr. Cholmeley said this had been done with good results; a fine trocar had been used.

Mr. Croft exhibited two specimens of Colloid Cancer of the Breast. One was from a women aged sixty. It had been growing for three years, and was the size of a hazel-nut; the nipple was retracted, and blood had oozed from it lately. The glands in the axilla were unaffected. The other was from a woman aged sixty-eight. It had been growing for two years, but more rapidly the last nine months; it was the size of an orange. The nipple had been retracted for thirty years; blood oozed from it occasionally. The tumour was adherent to the skin; no glands in the axilla enlarged. There appears to be two varieties of colloid cancer, the one *ab initio*, the other a colloid degeneration. The old theory that it arose from the breaking down of cells is now scarcely tenable. From the position of the groups of cells, the stroma would appear to be the source of the colloid material. He would like to know the opinion of others.

Dr. Moxon, in reply to a question from the President, said he had no experience as to whether jelly cancer came from the stroma or the cells.

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OBSTETRICAL SOCIETY OF LONDON.

DR. BRAXTON HICKS, F.R.S., President, in the Chair.

DR. AVELING exhibited his apparatus for immediate Transfusion, which he has now modified by adding a tap at each end of the tube, close to the nozzles. He had performed the operation successfully a few days previously. Dr. Aveling believed that the methods of preventing coagulation, adopted when the mediate mode of transfusion was used, were the cause of needless deterioration of the blood and of unnecessary delay.

Dr. Barnes suggested that, instead of filling the syringe and tube with warm water, it would be better to use a solution of phosphate of soda. In reference to the defibrination of blood in transfusion, he would observe that, however carefully the filtration might be performed, it was still to be feared that in the filtrate some fibrine might coagulate and act as minute emboli.

Dr. Playfair had found no difficulty in defibrinating the blood, and he considered defibrination to be the best means of retaining the blood fluid. In his own case a gravitation instrument had failed to act satisfactorily, probably because there was not sufficient force to overcome the resistance of the venous wall on the nozzle of the instrument.

Dr. Heywood Smith showed a modification of his Angular Scissors. The small scissors had curved blades, and when bent forwards would be useful for the removal of polypi, and, bent backwards, for cutting the sutures after the operation for vesico-vaginal fistula.

Dr. Hall Davis communicated a case of Inversion of the Uterus. The case came to the Middlesex Hospital ten months after labour. The patient was very prostrate from hæmorrhage. Ordinary efforts at reduction having failed, and the patient being much too weak for endeavors by sustained elastic pressure during consecutive days to effect reduction, the inverted organ was removed by the single wire éraseur. The patient was discharged convalescent on the thirty-third day. The inverted organ, which was exhibited, was much involuted; it contained parts of the ligaments, but not the ovaries.

Dr. Barnes read a paper "On the essential cause of Dysmenorrhœa, as illustrated by Cases of Menstrual Retention." The author sought, by comparison of different cases of dysmenorrhœa, to discover a common essential cause. Having discussed the question of "irritable uterus," and adverted to the evidence accumulated since the time of Gooch and Ferguson in proof that cases of neuralgic and constitutional dysmenorrhœa were being gradually transposed under closer clinical analysis to the class of obstructive dysmenorrhœa, the author stated the proposition that the essential condition of a large proportion of cases of dysmenorrhœa was really retention of menstrual fluid. He illustrated this by several cases typical of various kinds of obstruction, and especially by some of congenital or acquired stenosis and atresia of the genital canal. For example, one was that of a healthy woman who never suffered dysmenorrhœa until after a severe labour, which was followed by sloughing of the vagina and gradual atresia by cicatricial contraction; as the constriction advanced dysmenorrhœa became more and more severe, until, the vagina being completely obliterated, the phenomena of complete retention ensued. When the vagina was restored by operation, the dysmenorrhœa was cured. So in cases of imperforate hymen, there was dysmenorrhœa from retention. Some points in the history of this affection were discussed. The term "amenorrhœa," usually applied, was bad; menstruation went on, it was occult and painful. The sources of danger and the modes of operating were considered. The cases of dysmenorrhœa from flexion and from stenosis of the os internum and os externum uteri were compared with those of complete atresia of the vagina, the common condition being in all retention of menstrual fluid. Other illustrations were adduced to show that retained fluid or clots in utero caused symptoms similar to those of dysmenorrhœa. The effect of stenosis and atresia in producing retrograde or ascending dilatation of the genital canal was shown, and the mechanism by which retained or injected fluids found their way through the tubes was attributed to the contractile efforts of the uterus excited by the pressure of the retained fluids. One condition submitted was that, if the most common essential condition of dysmenorrhœa was retention of menstrual fluid, search must be made for the cause of the retention in order to remove it. This conclusion was as logical as the necessity always recognised of giving exit to the menstrual accumulation in cases of complete retention from imperforate hymen. Dysmenorrhœa was incomplete retention. The indication was the same, only less imperative.

Dr. Playfair believed that clinical facts were not in accordance with a theory so seductive from its simplicity as that advocated by the author. He had no wish to throw the least doubt on the mechanical theory as explanatory of a large number of cases, but he believed that cases existed far more frequently than Dr. Barnes allowed where no obstruction could be found. He referred especially to the cases of so-called congestive dysmenorrhœa, and to those usually included under the class of ovarian dysmenorrhœa.

Dr. Snow Beck said that nearly all acquired, as distinct from congenital, alterations of the uterine organs from a state of health, were attended with more or less increased pain during menstruation, and some affections of the neighboring organs give rise to a similar increase of pain. He had no hesitation in expressing his belief that obstruction to the exit of the menstrual flow formed but a small proportion of the cases attended with increase of pain during menstruation, and that treatment directed essentially to this object would in the large majority of instances be attended with unfavorable results.

Dr. Rogers also believed that dysmenorrhœa was not a disorder depending solely on obstructive causes. Many cases of dysmenorrhœa were cured before the theory of obstruction was first acknowledged. Many of the cases depended on ovarian, rectal, or purely neuralgic disorders. One cause of dysmenorrhœa, fundal endometritis, had been specially described by Dr. Routh.

Dr. Tilt thought the most frequent cases of obstructive dysmenorrhœa were those in which the obstruction was clearly of a spasmodic nature. He had repeatedly met with cases in which dysmenorrhœa was intense, although the cervical canal was perfectly free, and the discharge of a florid color and without clots. The dysmenorrhœa in such cases sometimes depended on chronic inflammation of the menstruating surface, sometimes on morbid ovulation and subacute ovaritis; whereas, in exceptional cases, the complaint could only be considered a neuralgia of the menstruating womb.

Mr. Spencer Wells believed that a very large proportion of cases of true dysmenorrhœa depended on mechanical impediment to the free escape of the menstrual fluid, and to be curable by removal of the obstruction. Sympathetic pains in the breasts or elsewhere, and nervous symptoms common at the menstrual period, should not be confounded with true dysmenorrhœa. He narrated cases in which severe obstructive dysmenorrhœa had been cured by replacement of the flexed uterus, by dilatation of the cervical canal, and by opening a closed vagina.

Editorial.

DISEASES OF THE HEART.

The subject of Diseases of the Heart is one of great interest, both practical and pathological. It is, comparatively speaking, a new subject. The older pathologists gave the heart itself very little consideration. There was only one criterion to them of its action—namely, the pulsation of the radial artery. Almost their whole attention was given to the blood which the heart pumped through the body: the condition of the pump itself was almost ignored. The diseases of the lungs were not so much overlooked. They rendered themselves more palpable by various objective symptoms, although the differentiation of the various diseases of these organs was only accomplished by the discoveries of Laennec, which have also enabled physicians to test the condition of the heart by various physical methods. It is very interesting to notice how little is made of the heart by older authors. Heberden discusses palpitation, and obviously regarded it as occasionally having relations with grave constitutional states, such as those produced by too much care and business, asthma, gout, and general failure of the powers of life known by the name of a broken constitution. He gives, as an appendix to his Commentaries, an interesting paper entitled "Remarks on the Pulse," in which he expresses himself dissatisfied with the minute distinctions of the several pulses made by physicians, saying that they do not exist save in the imagination; and suggests that they should attend only to such circumstances concerning the pulse as cannot be misunderstood, and he specifies the frequency of the pulse as a circumstance that could be accurately noted and should be chiefly attended to. Heberden clearly sighed for accuracy, and thought this quality could attach only to observations of the number of pulsations. Cullen scarcely seems to have thought of the heart as an organ capable of being affected with structural disease.

The detection and differentiation of the diseases of the heart; the relation of the most common of these to acute rheumatism; the study of morbid changes in the muscular structure of the heart; and the perception of the high importance of attending, even in cases of valvular disease, largely if not mainly, to the condition of the muscular tissue,—these are a few of the most characteristic and creditable points in the medical work of the present century. It is curious that Heberden, who, with great sagacity, as we have shown, specified the serious constitutional condition with which palpitation was frequently associated, contrived to leave out rheumatism. The honor of the discovery of the connexion of acute rheumatism with inflammations of the heart is, we believe, due to Dr. David Pitcairn, who first noticed the fact in 1788; but it was not till Sir D. Dundas's account of the disease called "rheumatism of the heart," in 1809, that this great pathological relation was made distinct which is now so well understood by every tyro in medicine. The detection and differentiation of heart-disease could not have advanced as it has done apart from the introduction of the stethoscope by Laennec. But it is gratifying to know that English, Irish, and Scotch physicians have greatly distinguished themselves by their labors in this branch of pathology, especially that the researches of Pitcairn, Dundas, Bright, Latham, and Hope have been brilliantly continued by Stokes, Corrigan, Graves, Quain, George Johnson, Richardson, Fothergill, and a host of others.

It is not our intention to pursue this line of thought further. But if anybody doubts the progress of medical science let him compare the knowledge possessed by physicians in the beginning of this century of the morbid conditions of the heart with the knowledge possessed by physicians now, and he will admit that the advance is enormous.

An ungenial critic may say, *cui bono?* To what end is all the elucidation of the different lesions of the different tissues if it does not lead to a more enlightened and efficient treatment? But this is just what it does lead to, and there can be no doubt that the medical treatment of diseases of the heart is much improved. It is especially improved in two or three respects. In the first place, much more attention is devoted to other local disorders and to the constitutional states with which heart-disease is associated. By a proper medical regard to these the suffering heart is often either completely relieved or greatly improved. Secondly, the physician of the present day directs his treatment to improving the vital tissue—the muscle—of the heart, and does not aim at impracticable alterations of the condition of the valves. He knows that much may be done to nourish, to rest, to save the muscular tissue, and his treatment has these objects chiefly in view. Moreover, the use and action of cardiac medicines are much more accurately defined than they were. Surely, too, we may hope that, despite the sceptics, a better treatment of rheumatic fever does tend to save the heart—to save its muscular tissue if not its valves. There is a third grand advantage in modern medicine. It can take a more hopeful view and give a more hopeful prognosis. We do not think it necessary to imitate that physician of whom Dr. Stokes has somewhere spoken, who, on hearing a bruit in a patient's heart, who up to that time had been working very well notwithstanding his bruit, said: "Sir, I hear your death knell"; and so felled the patient with prognosis. We see too much of the recovery of heart-power by rest, food, and proper medicines; we know too many pulses that are beating happily now that we knew to intermit or to be irregular years ago. We know on such authority as that of Dr. Stokes that the patient with heart-disease does not, in the great proportion of cases, die suddenly, but gradually, and with a failure that may by care on his part and a due regard to medical hints and helps be greatly retarded and averted. On such grounds as these heart-disease has come to be a creditable and hopeful chapter in medical science.

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HISTOLOGY OF THE NERVES.

In histological investigations it appears to hold good that with fresh means of research new facts are open to discovery, even in the most familiar objects. In the just published number of the *Archives de Physiologie*, M. Ranvier calls attention to several points in the histology of the nerves that have hitherto escaped observation. The first of these is, that the ordinary fibres or tubules present distinct constrictions at about a millimetre apart. Illustrations of the structure of the nerves have been published by various writers, and notably by Frey and Kölliker, in which such constrictions have always been vaguely represented; but they have been regarded as accidental, or as the results of manipulation. The only man who has depicted them with fidelity is Czermak. Even Schultze appears to have overlooked them in his recent article in Stricker's "Manual of Histology." M. Ranvier's observations were conducted on nerves prepared with picro-carminate of ammonia, with osmic acid, and with nitrate of silver. We must refer to his paper for the mode of preparing the first-named reagent; but the solution should contain about 1 per cent. of the salt. The nerve, immediately after its removal from the living body, should be placed in a drop of it, on a slide, and gently teazed out with fine needles for a short period. It is then gently covered with a thin piece of glass, and at once examined with powers magnifying from 300 to 600 diameters. The appearances then presented scarcely differ from those of the living nerve. The myeline is continuous, and neither the sheath of Schwann nor the nuclei nor yet the cylinder axis are visible. The annular constrictions, however, at definite distances from one another, are very obvious. Above and below the constriction the nerve is slightly thicker than elsewhere. In the course of a minute or two the myeline loses its transparency and homogeneity just above and below the constriction, and becomes granular, at the same time the cylinder axis makes its appearance, acquires a yellowish tint, and presents a double contour, the outer line being straight, the inner sinuous. After the lapse of some hours the cylinder axis is stained red at the constriction, though the constricting annulus itself remains uncolored. Even after the lapse of several days, the cylinder axis in the internodes (if we may so term them) remains uncolored—unless, indeed, owing to a sharp curve or kink in the fibre, the axis is brought into immediate contact with the sheath of Schwann, when it is stained to a corresponding extent; whence it appears that the myeline will not permit the passage of crystalloids. With high powers and immersion lenses, the constricting annulus is seen to be within the substance of or perhaps beneath the sheath of Schwann. At this point, as is indicated by the staining of the cylinder axis, the myeline is deficient. The constrictions are found only upon the double contoured nerves; the fibres of Remak have no constrictions. Experiments undertaken with osmic acid, which blackens fat, and myeline, fully corroborated the results obtained with the picro-carminate of ammonia; showing with great distinctness the annuli, which, under the influence of this reagent, appear as light transverse striæ crossing the blackened nerve, making the tubule resemble an alga, with septa at considerable distances from each other. M. Ranvier thinks he has been able to discern a faint stria dividing the annulus into two horizontal halves. The interval between the constrictions varies from rather less than one to nearly two millimeters, and is always less in the finer than in the coarser nerve-tubules. An important structural point he has made out with the osmic acid is, that there is only one nucleus for each segment of the nerve-tube comprised between two constrictions, and this nucleus is nearly equidistant from each of these constrictions. Between the sheath of Schwann and the myeline is a little protoplasm or other albuminous substance, which is not colored by the osmium. M. Ranvier points out the analogy of each such segment of a nerve as has just been described with a fat-cell: the sheath of Schwann corresponding to the membrane of the fat-cell; the nucleus and the protoplasm of the interannular segment being analogous to the nucleus and to the protoplasm, which renders the cell-membrane of the fat-cell double; whilst the myeline is equivalent to the fat. There is however, of course, no analogue to the cylinder axis in the fat-cell. Appearances indicative of precisely the same structure and demanding the same explanation were obtained by staining fine fasciculi of living nerves with a solution of nitrate of silver containing from one to three parts per cent. After a few minutes the nerve was cut out from the body and placed in glycerine. The surface then presented the sinuous contour-lines of broad, flat, epithelial cells, beneath which were the fine longitudinal striæ corresponding to the fibres; and scattered over these were minute black crosses, corresponding to the blackened constricting annuli and the cylinder axis, also blackened for a short distance above and below the constriction. The blackening of the latter is not uniform, but presents a series of transverse bars or striæ. Lastly, M. Ranvier, after alluding to the transverse striæ described by Frommann as existing in the cylinder axis, states he has discovered certain biconical thickenings in this body, the relation of which to the annular constrictions he does not appear to have been able to determine. M. Ranvier's observations require confirmation, but are certainly of great interest, and will probably pave the way to fresh investigations, which are much wanted, into the development of the nerves.

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THE ORIGIN OF PUS.

Two modes of origin are usually ascribed to pus-globules. It is believed that they may arise from the proliferation of certain cells—as from those lining serous or covering mucous cavities and surfaces, and those distributed through connective tissue,—or that they are only modified white corpuscles of the blood which have escaped from the vessels, and especially from the capillaries and smaller veins. A paper by Dr. Duval, strongly opposing the latter or Cohnheim's view of their origin, appears in M. Brown-Séquard's *Archives de Physiologie.* The fact of the origin of pus-globules from epithelium is easily shown in cases of inflamed bronchial or vesical mucous membrane, and perhaps still more readily in those of inflamed or purulent conjunctiva; for in each of these cases not only is the quantity of pus-globules thrown off wholly incompatible with the idea of their being exclusively derived from the white corpuscles of the blood, but the several stages of metamorphosis between a rudimentary epithelial cell and a mature pus-corpuscle may be easily traced in carefully prepared specimens. Their origin from, or rather their identity with, the white corpuscles of the blood, in some instances, was attempted to be proved by Cohnheim by exciting inflammation in the cornea and in the mesentery, and his results have received the very general sanction of histologists and pathologists. In his experiments on the cornea Cohnheim believed he had demonstrated satisfactorily that when inflammation was excited in that membrane, either by touching it with nitrate of silver or by passing a thread through its centre, it became cloudy or hazy owing to the infiltration of white corpuscles in a direction from the circumference towards the centre, the corneal corpuscles remaining unaltered. That the new corpuscles which made their appearance were really the white corpuscles of the blood he considered he had proved, since, after the injection of fluids containing aniline and other coloring matters in suspension into the blood, he found the particles in the interior of these new corneal corpuscles, it having been previously demonstrated that they are often taken up by the white corpuscles of the blood. In his experiments on the mesentery, again, he showed that mere exposure to the air is sufficient to excite inflammation; and that phenomena may be continuously watched under the microscope indicating the escape of the white corpuscles from the vessels, since these may be seen to enlarge whilst the blood-current through them diminishes in rapidity, and in the inert zone the white corpuscles accumulate in large numbers. Then, according to Cohnheim, small projections appear on the vessels, which become pedunculated, and finally set free. These projections are white corpuscles, and under the irritant action of the air become changed into pus-corpuscles.

M. Duval states that, with some slight modifications, he has repeated these experiments, and has arrived at quite different results. He finds that, on lightly touching the cornea with a point of nitrate of silver, the inflammation excited does *not* commence at the periphery and proceed towards the centre; but, on the contrary, that it begins at the centre of the injury and radiates outwards in all directions—in a word, is centrifugal, and not centripetal. He was never able to see, in the early stages, free white corpuscles. The corpuscles seen *always* resulted from a process of cellular proliferation; the *point de départ* of the metamorphosis being the plasmatic cellules of the cornea, which, far from remaining stationary, undergo hypertrophy and give origin to new globular products. In the case of the mesentery, which M. Duval has examined with very high powers (1600 linear), he contends that in the very act of opening the abdomen of the frog to withdraw the membrane lymphatic sacs are opened, and that from these a constant flow of lymph-corpuscles takes place, which naturally group themselves and form long rows in the depressions at the sides of the vessels, simulating the appearances that would be presented if they had escaped from these vessels. Again, M. Duval states that, in winter frogs, Heumann has shown that scarcely any white corpuscles remain in the blood, and that vessels may be watched for hours together without one being visible; yet that, if inflammation be established, plenty of corpuscles come into view on the exterior of the vessels; these, he thinks, must be derived from the corpuscles of the tissue themselves. In regard to the unmistakable elongation of the white corpuscles towards the walls of the capillaries at certain points, M. Duval also gives quite a different explanation from that of M. Cohnheim. The corpuscles external to the vascular walls, he believes, hypertrophy and a current of fluid from the blood sets towards them through the capillary wall, which becomes irregular and viscous opposite them; and it is at these points that the viscous white corpuscles within the vessels are arrested, and are drawn out into the drop-like form. These observations show, perhaps, that further corroboration of Cohnheim's views is needed, though we are inclined to adopt those of M. Vulpian appended to M. Duval's paper, to the effect that the theory of suppuration formularised by Waller and Cohnheim has too strong evidence in its favor to be set aside without a much larger amount of counter evidence than that furnished by M. Duval.

Medical Annotations.

"Ne quid nimis."

THE FORCEPS IN MIDWIFERY.

Dr. J. Thorburn, Lecturer on Midwifery and Diseases of Women, Manchester, has published in pamphlet form "Remarks on 118 Cases of Forceps Delivery," with, strictly speaking, only one maternal death. In a second case of death the mother was dead before the forceps were applied. The sole maternal death after the forceps was that of a middle-aged primipara after a tedious labour of about thirty hours, with adherent placenta that had to be extracted. On the third day adynamic peritonitis set in, of which she soon died. The maternal mortality from the instrument, in this series of cases at least, seemed *nil*. In 103 of the cases where delivery was effected by the *forceps alone*, the infantile fate was as follows:—85 were born alive, 14 dead, and 4 unaccounted for, but probably alive. These 4 excluded, the mortality of children was 14·1 per cent.; 9 of the 14 Dr. Thorburn attributes to the severity of the labours, and considering how fatal any prolonged delay in the second stage is to the child, especially in primiparæ—the proportion of which in these cases, by the way, is not given,—he is probably right. The forceps were applied at or above the brim in 28 cases, in the pelvic cavity in 42, and at the outlet in 48. The maternal lesions were slight, except in one case, with the face forwards, which would not rotate. The choice was between destroying the child and using force that rent the perineum. The child was saved, and the perineum healed without permanent fistula. The chief reasons for using the forceps in the cases were a slight disproportion between the propelling force, the pelvic space, and the bulk of the fœtus (48 cases); decided want of expulsive force (22 cases); and (in 19 cases) decidedly deficient space, &c. This series of cases only goes to enforce the established doctrine that the forceps is a most safe and valuable obstetric instrument when used with anything like care and propriety. Indeed this belief is so general now among British authorities that it appears to us that the time has come for asking of all candidates for obstetric diplomas certificates that they have seen a few cases of delivery by the forceps.

DOSIMETRIC MEDICINE.

We have received from M. Burggraeve, Professor at the University of Ghent, and senior surgeon to the hospital, a lithographed circular on the above subject, addressed to the medical press in general. The Professor states that, now-a-days, accurately defined therapeutical preparations should take the place of complex remedies; and that alkaloids, in the shape of granules, should exclusively be given. By very small doses the maximum and minimum necessary for each patient can be experimentally made out; and, if necessary, as in pleuropneumonia, the pulse and the temperature can be rapidly brought down by granules of veratine and digitaline; pain can be relieved by cicutine and aconitine, and nervous power sustained by strychnine. The great point with the author is to avoid the effects of the remedy on the mouth, and to insure its great division in granules. M. Burggraeve takes a rapid survey of fevers, specific diseases, neuroses, dyspepsia, anæmia, tuberculosis, diseases of children, &c., and thinks he has proved that remedies given in granules are most efficacious. He refers those who would drink deeply of his system to his work entitled, "A Guide to Dosimetric Medicine," in 18mo, to be obtained at the depôt of dosimetric medicines, No. 188, Faubourg St. Martin, Paris.

That bulky and nauseous medicines should be avoided all will allow, but such precautions may be left to the experience and good sense of the practitioner, and need not the building up of a new and universal system. These great blows always miss their aim. We may add that the manner of propagating this system smacks a little of the modes adopted by the advertising crew. Had not M. Burggraeve held the honorable position of University Professor and hospital surgeon, we should hardly have thought it right to notice Dosimetric Medicine.

WEST KENT MEDICO CHIRURGICAL SOCIETY.

A GOOD paper seems to be always forthcoming at the meetings of this local Society. There was no exception to the rule on the 5th inst., when Mr. Sidney Jones discussed, in a very practical communication, the treatment of nævus in its various forms. Mr. Jones's experience leads him to the conclusion that the use of the actual cautery is by far the most satisfactory way of getting rid of the more superficial nævi. In reference to those more deeply seated, Mr. Jones stated that, where it was especially necessary to prevent deformity from scarring, he was wont, where the nævus seemed to be encapsuled or more or less clearly circumscribed, to dissect off a portion of integument, and then to remove the nævoid mass by the knife, using the integument to cover over and neatly fill up the wound. In other cases, where large arteries were detected feeding the growth, he preferred to obliterate these vessels by the application of the actual cautery, applied more than once if necessary, and subsequently to attack and destroy the actual nævus by injection of perchloride of iron. Mr. Jones stated that he had not seen much deformity left by this treatment, and he gave the particulars of several interesting cases in support of his views.

ONE SOURCE OF SKIN DISEASE.

A CORRESPONDENT of *Nature*, who signs himself "R. M.," states that he has satisfied himself that certain attacks of eczema of the face to which he was formerly liable were due to the use of impure soap for shaving purposes. He declares that the "prime old brown Windsor soap" of the perfumers' shops is chiefly manufactured from bone grease. This, as extracted from the bones by the action of boiling water under pressure, is of a dark tarry brown color, and of an abominable odor. By various processes it is more or less defæcated, bleached, and deodorised, and is separated into two or three different qualities, the most inferior of which goes to the formation of railway or other machinery greases, whilst the better kind is saponified and highly perfumed. The natural brown color of the grease gives it the right tint in the cheapest way without the coloring by caramel, which was the original method of manufacture. Some bars of brown Windsor contain a rich admixture of small, sharp angular fragments of bone, which, before boiling, were putrid. "R. M." states that, on three separate occasions, shaving with such soap, whilst he was otherwise in perfect health, induced an attack of eczema, which resisted medicinal treatment but disappeared on ceasing to use the soap.

THE NERVES OF TASTE.

A FRESH contribution to our knowledge of this subject has been given by Dr. Ph. Lussana, in the last number of the *Archives de Physiologie*, his views being based on pathology. He gives the details of an Italian peasant woman from whom Dr. Vanzetti twice excised a portion of the lingual nerve; two years afterwards it was found that the tongue was normal in form, nutrition, and color on the affected side, but had entirely lost its gustatory sensibility, sweets and bitters being alike unperceived. This result he points out might have been anticipated by our knowledge of anatomy, since it is known that the lingual alone ramifies in the mucous membrane of the anterior part of the tongue to the exclusion of the glosso-pharyngeal, which is distributed to the posterior region of the lingual mucous membrane alone. He next proceeds to show that the gustatory fibres of the lingual nerve *do not come from the fifth*, and this he proves by adducing cases of complete paralysis of the fifth, yet in which taste was perfectly preserved. They come from the seventh, as is shown by the loss of taste when the facial was divided or paralysed, and are, in fact, contained in the chorda tympani.

PHYSIOLOGY OF THE BLADDER AND KIDNEYS.

DR. TRESKIN, of Mstislawl, Russia, has made some researches on the physiology of the bladder and kidneys in Hoppe-Seyler's laboratory, and has arrived at the following conclusions, which he has published in Pflüger's *Archiv fur Gesammte Physiologie*:—"1. The contents of the urinary bladder and those of the bloodvessels and lymphatics in its walls mutually diffuse into one another. 2. In consequence of this diffusive process the contained urine receives water, and gives off urea. 3. The urea found in the blood or lymph of an animal may consequently have entered these fluids by diffusion, and may have previously been excreted by the kidneys. 4. This diffusion also occurs when a more concentrated urine is secreted by the kidneys than that retained in the bladder. 5. The urine stagnating in the bladder in addition to water usually takes up common salt also. 6. The secretion of the two kidneys contains nearly equal quantities of urea, though the volume, and consequently the percentage proportion, of the urea secreted by the right and left kidneys in the same period may vary to a very remarkable extent.

TESTS FOR DETECTING STRYCHNIA.

THE *Popular Science Review* states that Dr. Filhol, in a recent paper on this subject, maintains that strychnia should, in cases of poisoning, be obtained in the solid state; the alkalinity of its solution should be ascertained, as well as its intensely bitter taste; its behavior with chlorine, and its blue coloration under the influence of sulphuric acid and oxidising substances, should also be seen; while, lastly, as a very delicate reaction, Dr. Filhol observes that, with chloride of gold, strychnia (in solution) yields immediately a crystalline precipitate, which, although slowly, is yet distinctly formed in solutions containing one-tenth of a milligramme of the alkaloid. This precipitate and that formed by chlorine are at once dissolved by concentrated sulphuric acid, and, chronic acid being added, the well-known blue coloration that strychnia yields with this last reagent is produced. The presence of alcohol in liquids to be tested for strychnia should be avoided.

THE MUSCULAR TISSUE OF THE SMALLER BRONCHIA.

Prof. Rindfleisch, in an article in the *Central-blatt*, states as the result of his observations that the smallest bronchia have a peculiar and well-marked layer of circular muscular fibres, which form a kind of sphincter where the bronchia are continuous with the infundibula. The fibres are capable of considerable elongation, and rest upon a very close-meshed plexus of capillaries, which resemble the pulmonary vascular plexus. The circular fasciculi send loop-like processes into the openings of the infundibula, which extend as far as to the fundus at two, three, or four points; bands of smooth muscular fibres also surround the infundibula circularly. These muscular bands are for the most part situated in the inward projecting portions of the alveolar septa. In the so-called brown induration of the lungs, all these muscular bands become hypertrophied. When once they have been seen under these circumstances they may readily be recognised in the normal lung.

MECHANISM OF THE SEMILUNAR VALVES OF THE HEART.

M. G. Cerredini, from his experiments upon these valves, arrives at the following conclusions:—(1) That the valves are not applied to the walls of the vessels (aorta and pulmonary artery) during the systole of the heart; (2) that their closure is an immediate effect of the cessation of the systole, and precedes the diastole; (3) that there is no regurgitation, and consequently no loss of force; and (4) that the interval between the first and second sound is the exact measure of the duration of the systole, whilst the interval between the second and the first represents that of the diastole.

Correspondence.

"Audi alteram partem."

ABNORMAL ACTION OF CHLORAL HYDRATE.

To the Editor of The Lancet.

Sir,—I consider the following worth recording:—I had been attending Mrs. S—— (aged about twenty-eight years) for three weeks. She was suffering from a severe attack of rheumatic fever. She, however, was nearly convalescent by means of the usual treatment. On account of some restlessness I was induced to give her a drachm dose of solution of hydrate of chloral at bedtime. The solution was of the strength of one grain of the hydrate to two minims of solution. I had given her the same dose in the early stages of her illness with great benefit. She, however, had scarcely swallowed the draught when she broke out into a cold perspiration; this was followed by acute pain in the muscles of the back, dimness of sight, palpitation of the heart with slight pain, twitching of the muscles of the thighs, spasmodic contraction of the hands, and also, most singular of all, opisthotonos. The spasms were, however, intermittent. There was also difficulty of breathing. All these symptoms lasted about three hours, and then passed off in sleep, and she was better in the morning. The opisthotonos and spasms were so well marked as to lead her husband to fear some mistake had been made with strychnia. I allayed his fears, however, and ordered her a little brandy; I also applied a mustard plaster on the back and cardiac region. I may mention that the urine was nearly suppressed. I consider the case curious, and worth some investigation.

I am, Sir, yours obediently,
H. A. Allbutt, M.D., L.R.C.P. Ed.

Leeds, April 2nd, 1872.

A STYPTIC "CURL-PAPER."

To the Editor of The Lancet.

Sir,—I desire to introduce to the notice of the profession a styptic paper which I have found useful, and which has been successfully used by several friends in this neighborhood. It is prepared by Messrs. Cutting and Son, of Leamington, from directions furnished by me. Sheets of household paper, of moderate thickness, are steeped in a solution of tannin, of such strength that each sheet is impregnated with two grains, and then dried in the air. I have found them most useful in cases of external hæmorrhoids, used as closet-paper, a portion being placed in the cleft of the anus after each use; also in excoriations around the anus caused by the acid discharge of diarrhœa in children. They form a ready styptic for stopping the bleeding from leech-bites, and in superficial excoriations from blows or falls. Many other modes of application may occur to your readers.

Yours faithfully,
James Thompson, M.B., M.R.C.S.

Leamington, March 27th, 1872.

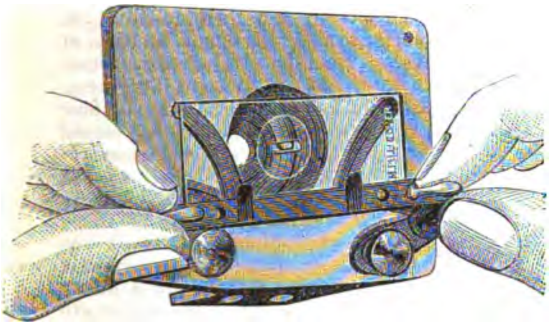
New Inventions

IN AID OF THE

PRACTICE OF MEDICINE AND SURGERY.

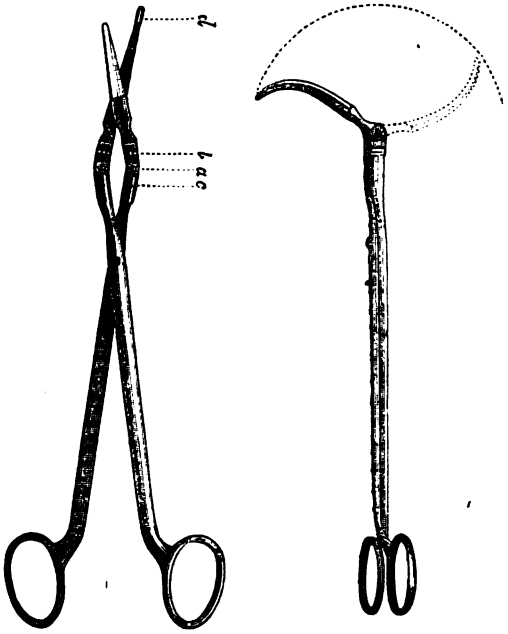
THE ST. THOMAS'S HOSPITAL MICROSCOPE.

We have had an opportunity of inspecting one of a number of microscopes recently constructed by Mr. Pillischer, of New Bond-street, for use at St. Thomas's Hospital. The instrument is of excellent workmanship, is moderate in price, and deserves to be generally known. The stand is constructed on the principle of Mr. Pillischer's well-known students' microscopes, but of larger and more massive dimensions; it is very firm and free from vibration, and has coarse and fine adjustments; the former of sufficient length to admit of a 8 in. object-glass being used, double mirrors, and revolving diaphragm. The stage adapted is of two kinds, one with a sliding fork upon which the object rests, which, with a little practice of fingering, is easily managed; the other is a new and exceedingly ingenious contrivance of the maker. A strip of metal of sufficient width for the object to lie on is connected with two jointed arms; these are screwed to the bottom plate on which they move. At each end of the strip is fixed a small handle, by which the stage can be moved in rectangular directions with perfect ease and accuracy. Considering the simplicity of the mechanism, we are surprised how well the object keeps in focus under a high power. We have tested the $\frac{1}{4}$ in. object glass which is supplied with this microscope, and found it to be of excellent quality both as regards magnifying power and definition. We understand that the microscope, as supplied to St. Thomas's Hospital, cost £5, or, with the addition of a binocular arrangement, polarising, and a quantity of other useful apparatus, £10 10*s*.



DR. HEYWOOD SMITH'S "ANGULAR SCISSORS."

Scissors with curved blades, by which a cut is made at an angle with the handles, are of some antiquity, and the principle of the double scissors is also not new; but the peculiarity of the "angular scissors" is that the small scissors can be used at any angle with the handles up to a right angle; and in the pair with curved blades the points may be made to cut in a direction exactly opposite to and parallel with the handles; while, if bent backwards, tissue or sutures can be cut, the handles being quite out of the way of the operator. Besides the pivot joints of the handles and scissors respectively, at the junction of the scissors with the handles is a somewhat complicated joint compressed into a small space, which has been very ingeniously carried out by the makers, Messrs. Mayer and Meltzer, of Great Portland-street. By means of the pivot joint (*b*) the small scissors are opened; *a* is the hinge joint, by which the scissors are flexed at the required angle with the handles; and *c* is a rotatory joint, necessary for the following reason: when the scissors are used quite straight, only the pivot joint comes into play (as in the ordinary double scissors); but when the joint *b* is bent, as each leg of the scissors describes an arc in a plane at a different angle to the arc described by each arm of the handles, and as these arcs are situate at a tangent with one another, and the points describing them are rigidly connected with each other, it follows that the point common to the two must describe an arc that must be the complement, so a portion of the arms of the handles rotates as an accomodation to the arcs described by the legs of the scissors. The director-point (*d*) would be of use in dividing the cervix uteri; for, in using scissors, it is difficult to guard and guide two separate points; whereas with these scissors the director-point can be passed into the os uteri, and the scissors then opened without the risk of their slipping or of wounding adjacent structures.



These scissors will be useful not only to the obstetric practitioner, but also in many surgical operations—e. g., those about the palate and throat. The curved ones doubtless may be of great service in some cases of polypi and difficult vesico-vaginal fistulæ.

EUCALYPTUS GLOBULUS.

Several inquiries having been made regarding the properties of a new drug obtained from the Eucalyptus Globulus, or Blue Gum-tree, the following information may prove interesting to our readers. The eucalyptus is a myrtaceous tree, a native of Australia, but recently cultivated in Corsica and South Europe. On account of its supposed efficacy in marsh and others fevers it has already gained the name of "Fever Tree" in Spain. Dr. Lorniser, of Vienna, reports that he employed it in the treatment of intermittent fever with success. Prof. Gubler has also strongly recommended this remedy in the *Bulletin de Thérapeutique* as a tonic and stimulant. It has also been used in bronchitis, and as an antiseptic application to wounds.

The eucalyptus has been subjected to extensive trial in Corsica by Dr. Carlotti, and his very favorable report has been translated into English. From this report we learn that the whole of this giant tree is strongly impregnated with febrifugal properties. He has used leaves, bark, and wood with success.

Professor Maclean, of Netley, has also furnished us with his experience of this drug in the October number of the *Practitioner*. Cigars made from the leaves of this plant have been employed in cases where the use of antispasmodic remedies has been indicated, and Dr. Maclean says, as the result of his experience at Netley, that he knows of no remedy, with the exception perhaps of the subcutaneous injection of morphia, so efficacious in allaying pain, calming irritation, and procuring sleep, in cases of chest aneurism involving pressure on the vagus or its branches, and in cardiac asthma, as the eucalyptus globulus. A tincture of the leaf has been used in two-drachm doses with success in Germany in the treatment of intermittent fever.

By a recommendation from the Minister of the Interior the eucalyptus was subjected to analysis by Professors Vauquelin and Leiciana. They obtained besides an essential oil containing eucalyptol or eucalypt-camphor, an extract resembling resin of cinchona. This extract yielded a substance capable of neutralising the strong acids, and forming crystalline salts. The sulphate crystallised in stars, like the quinine salt; this induced them to try the action of chlorine and ammonia on this substance and the green coloration of quinine was instantly produced. It would be curious if quinine should be found in other trees than cinchonas. The oil has a lemon-yellow color, but in other respects it strongly resembles cajeput oil. As its alkaloid and therapeutic uses resemble cinchona it has been given in similar doses.

The preparations made by Messrs. Savory and Moore are tincture, distilled water, solid extract, and fluid extract. The tincture contains its alkaloid, camphoraceous oil, &c. The distilled water is an agreeable vehicle for stimulants. The solid extract is for pills, and the fluid extract for mixtures, &c. They are all efficient modes of exhibiting the remedy. The dose of the tincture and fluid extract is a teaspoonful in water two or three times a day. The powder is given in the same doses as cinchona powder.

Foreign Gleanings.

SUDDEN DEATH: DOUBTFUL CASE.

M. **Trelat**, of Paris, extirpated, a short time ago a glandular tumour, of large dimensions, situated under the lower jaw, from a strong healthy man of thirty-two. This operation proved long and tedious, but succeeded perfectly. The wound was nearly healed, when, by the side of it, a small kernel was noticed occupying a space not larger than a shilling. The surgeon thought it prudent to remove it at once. On the 19th of December last the patient was brought into the operating theatre. Chloroform inhalations were, as on the first occasion, followed by rather violent excitement, which was succeeded, as usual with him, by complete resolution. He breathed three or four times with some stertor, and then quietly. M. Trélat thereupon made along the tumour an incision of about one inch; from the divided parts a little venous blood issued. At this moment the patient suddenly turned pale, and his countenance became cadaverous; pulse and respiration stopped. All the means used in such circumstances were at once resorted to. Electricity excited two feeble inspirations, accompanied by a faint blush on the face, and life became definitely extinct. The exertions were carried on for a whole hour.

At the examination of the body twenty-four hours after death, a small subcutaneous vein, an afferent branch of the jugular, was found to have been wounded and incompletely divided. This vessel, as also the jugular, were filled with black blood, divided in small columns by numerous interposed bubbles of air. The heart was opened under water, and a notable amount of air was found in the right cavities, whilst none was noticed on the left side. There was air likewise in the cardiac veins, but none in the rest of the venous system, which was carefully examined. The gases were unfortunately not analysed; but it was observed they were not ill-smelling, that they did not extinguish flame, that they did not burn when ignited, and that they were in all respects analogous to atmospheric air. The body presented no signs of putrefaction.

M. Trélat thinks that the fatal issue is owing both to chloroform and the introduction of air into the veins. M. Perrin (the case was brought before the Surgical Society of Paris on Feb. 21st, 1872) thought chloroform was the principal cause. M. Le Fort throws all the blame on the air, as he had a similar death in a case of tracheotomy. M. Giraldès had seen three cases of the kind; air was found, not only in the heart and venæ cavæ, but also in the pulmonary veins and the subpleural cellular tissue. He also quoted a case of the late M. Roux, where a man, inhaling chloroform for tetanic symptoms, died suddenly. Much air was found in the venæ cavæ. He thought that both chloroform and the penetration of air had a share in the death of M. Trélat's patient. M. Sée rejected the latter cause altogether. M. Panas justly observed that in ordinary autopsies air is often found in the cardiac cavities. M. Depaul ascribed the mischief to the air. And M. Trélat, in answering, alluded to the uncertainty of opinion still existing as to cases of this nature, and thought he might, though not absolutely, adhere to his first view—viz., that death had been caused both by the inhalation of chloroform and the introduction of air into the veins.

DIAGNOSIS OF MYOMA.

M. Gayet, of Lyons, has lately operated, with a fatal issue, upon a woman who suffered from such a tumour. Two years ago he had performed the extraction of a growth of this kind, also with an unfavorable termination, and had made up his mind not to touch them any more. The difficulty is to distinguish these tumours from ovarian dropsy.

In the second, or recent, case, the tumour was very large, and the symptoms of compression very evident. On tapping, several quarts of a yellowish-transparent fluid escaped. One of the minor cysts, however, had yielded a sanguineous serum. In a week the tumour was completely refilled. By a careful examination of the abdomen, a solid mass could be made out inferiorly, and to the left, sinking into the true pelvis. Under these circumstances, it was thought that the growth consisted of an ovarian cyst.

Very little information could be obtained from the patient, and when the operation was decided upon the tumour was rising as high as the ensiform cartilage. It was fixed in the cavity of the pelvis, and presented the usual characters of an ovarian tumour.

M. Gayet thinks that it is very difficult to ascertain whether there are adhesions or not; in this case it appeared as if none existed. When the patient stood erect the tumour sank behind the abdominal parietes without dragging the umbilicus. By a digital examination nothing particular was made out; the vagina was free, no uterine displacement was observed, and the cervix was movable. No examination was made per rectum.

At the very outset of the operation M. Gayet noticed the rose tint of the tumour, instead of the usual pearly white. From two to two and a half quarts were evacuated by tapping, and blood issued by puncturing another cyst. Several cavities were successively tapped, but the tumour diminished very little in size, and it was found very difficult to make it issue through the wound. An adhesion with a portion of small intestine was now observed. At last the tumour was extracted, and on its anterior aspect were seen the uterus, the Fallopian tubes, and the ovaries. The latter were healthy, and it was now clear that the tumour had arisen from the posterior aspect of the uterus. There was, however, an evident necessity of completing the operation. In the case operated upon two years before M. Gayet had divided the pedicle with the écraseur. This time he tried to enucleate it; made an incision into the peritoneum, and thus came upon the pedicle. The detachment of the latter did not prove very tedious.

The tumour was carefully and microscopically examined, and was found to belong to the class called by Virchow "cystic myoma." They generally are multilocular, presenting anfractuosities, some filled with a transparent and others with a hæmatic liquid. M. Gayet observed that it is impossible to diagnose these tumours from ovarian cysts. All the cases hitherto recorded had not been recognised (Spencer Wells, Kiwisch, &c.) though it is plain that they can be enucleated. The hæmorrhage in the present case was not considerable, but the patient died twelve hours after the operation.

THREE CASES OF TRAUMATIC TETANUS CURED BY CHLORAL AND WARM BATHS.

Dr. G. Lavo publishes these three cases in *Annali di Med.* (February, 1872). They were observed almost at the same time at the Hospital of Brescia, and had a very favorable issue.

The first case was a man of twenty-six, who met with lacerated and contused wounds of the scalp. The lesser ones healed in a few days, and the more extensive wounds in a fortnight. At that period difficulty of deglutition occurred, and soon the tetanic symptoms made their appearance. Chloral was first given by the mouth, and warm baths (99° to 100° Fahr.) repeated night and morning. Hypodermic injections of the same substance were soon had recourse to, and the chloral was alternated with laudanum, the warm baths being regularly and daily persevered in. On the thirteenth day the improvement was considerable, and after the application of a few leeches to the anal region, the patient was discharged in excellent condition. He had taken about eight ounces of chloral, and used twenty-five baths.

The second patient was twenty-one years old, and showed symptoms of tetanus a fortnight after a contused and lacerated wound of the foot. Woorara was first used at the patient's residence, but failed; and he was received in hospital fifty days after the infliction of the wound, and thirty-five after the first appearance of the tetanic symptoms. Ordered ninety grains of chloral, and a hot bath. These means were continued for a fortnight, with an occasional enema; and the patient was discharged cured after having taken some ferruginous cod-liver oil. He had taken altogether about six ounces of chloral and used fifteen baths.

The third patient was a man of forty-two, who met, by the falling of a stone, with a contused and lacerated wound of the foot. The tetanic symptoms appeared twenty-five days afterwards, and the man being removed to the hospital the same treatment was adopted which had proved so beneficial in the first two cases. In this instance, however, hypodermic injection of seven grains of chloral was used concurrently with the internal administration of the same. In about thirteen days the improvement was manifest, and the patient, who had seen the recovery and exit of his two companions in misfortune, could be kept in hospital no longer, and furtively left the institution.

"WHAT CELLS IN THE PEPSIN GLANDS CONTAIN PEPSIN?"

A paper with this title appears in the sixty-fourth volume of the reports of the Meetings of the Academy of Vienna by Ernst Friedinger. He points out that Kölliker first described the presence of two kinds of cells in the pepsin glands of the dog. Heidenhain and Rollet, who both investigated the subject very carefully, arrived at the same conclusion. They found that in mammals (the dog, cat, sheep, rabbit, and guinea-pig) there were two kinds of cells. One, formerly known as pepsin cells, but termed by Heidenhain "lining" and by Rollet "delomorphous" cells, were applied to the wall of the tubes, without, however, forming a complete and continuous layer between the membrana propria and the lumen. The second kind, previously overlooked except by Kölliker in the case of the dog, was termed "chief" cells by Heidenhain and "adelomorphous" by Rollet. These form the proper material filling the gland tubes. They bound the lumen, and where the "lining" cells fail are in contact with the membrana propria. The chief or adelomorphous cells have no well-defined contour; the lining or delomorphous cells have a distinct cell-wall. The neck of the glands usually contains structures similar in character to the delomorphous cells, though they are somewhat more spheroidal in form, In frogs, newts, and tortoises only cells corresponding to the lining cells are present. In a snake (Coronella lævis) the ends of the tubes contained only "lining" cells, whilst the neck presented large vesicular cells similar to those which Heidenhain terms "mucous" cells. In the dog Friedinger agrees with Heidenhain in finding, though not abundantly, "lining" cells in the beginning of the neck of the gland. Now, in regard to the question, which of these cells form the pepsin? Heidenhain thought the "lining" cells secreted the acid, the "chief" cells the pepsin. Ebstein supports Heidenhain. Friedinger, however, details experiments which tend to prove that those glands (near the pylorus) which contain principally Heidenhain's "chief" cells are really the most active, and that, consequently, as was always thought to be the case before the appearance of Heidenhain's paper, it is these chief or adelomorphous cells that contain the pepsin.

PECULIAR TREATMENT OF SYPHILITIC IRITIS.

Dr. De Magri, of Milan, has treated a series of cases of this kind by injecting calomel into the left arm (*Giornale delle Mal. Veneree*, February, 1872). He usually injects six grains of calomel suspended in glycerine; an abscess, mostly connected with sloughing, forms, and the eye instantly improves. Atropine is, however, freely instilled into the latter, and the improvement is attributed to the powerful counter-irritation exerted in the arm. Even cases of pannus and scrofulous keratitis are thus treated, and with equal success.

Now when it is recollected that it is maintained by some men of experience that atropine alone is sufficient in such cases, the faith in the revulsive action is a little shaken. Then it may be asked whether an abscess excited by other substances than calomel would not be as efficacious; or whether the author supposes that this salt acts on the whole organism. That calomel does good in iritis, whether syphilitic or not, is still believed by those who do not ignore the experience of the past. How much simpler then to give this mercurial chloride by the mouth, and have recourse to simple counter-irritation on the temple, not neglecting atropine. Hypodermic injections are the fashion of the day; they are useful, but they should not, without good reason, take the place of other methods of treatment. So with chloral, which is now tried for almost everything; we have, for example, Dr. Accetella (*L'Independente*, No. 2, 1872), who dresses sores, both primary and tertiary, with a concentrated solution of the hydrate. Some benefit was obtained, but it always remains to be seen whether other and older applications would not have done as well or better.

APPLICATIONS OF ELECTRICITY.

Professor Benedikt lately read a paper on the application of electricity for the removal of tumours before the Medical Society of Vienna, which is reported in a recent number of the *Wien. Med. Presse.* He points out the great improvements that have been made of late years in the construction and mode of application of apparatus, and refers particularly to the powerful current that can be obtained from the lead-zinc battery of Fromhold. He has himself constructed a carbon-zinc battery of great usefulness, which can be set into action by a concentrated solution of bichromate of potash and sulphuric acid (1 to 15). He mentions as the principle forms of tumours in which electricity has been found efficacious:—(1) Inflammatory swellings of the joints, in which, however, the current should not be applied directly to the inflamed part, but to the sympathetic spinal cord, or nerves supplying it; (2) fluid tumours resulting from inflammation, as hydrocele; (3) angiomata and aneurisms, in which several needles connected with the positive pole should be introduced; (4) lymphatic tumours, in which negative needles should be passed so as to break down the larger into smaller tumours, but the current should not be too strong, lest suppuration and ulceration occur; (5) indolent buboes; (6) neoplasmata; (7) carcinoma.

TANNATE OF QUININE.

Is this salt completely inactive or not? It would appear, from a discussion in the Academy of Medicine of Paris, that opinions vary. Messrs. Chauffard, Briquet, and others think it has no action; M. Mialhe, the eminent pharmacologist, thinks it has some. He qualifies, however, this view by saying that, in spite of some activity, the tannate of quinine is a *detestable* salt. Passing on from the tannate to the sulphate, M. Mialhe makes the following remarks worth recording. "Sulphate of quinine should never be employed as a basic sulphate, but as an acid salt. In this acid state the sulphate may compete with all other salts the base of which is quinine; for it should be noticed that it is the quinine itself, and not its saline combinations, which acts. The acid only serves as a vehicle for the introduction of the quinine into the blood; in the latter the quinine is set free by means of the alkaline or earthy bicarbonates which the blood contains, and then its modifying action begins. It is a mistake to believe that the sulphate of quinine, when this salt is administered, can be traced in the urine; the salts traced are the acid phosphate of quinine associated with the phosphates of lime and magnesia."

ECHINOCOCCUS IN THE ORBIT.

An interesting case of this disease was brought before the Medical Society of Vienna at the meeting held on Feb. 9th. It had been originally under the care of Arlt, but was tranferred to Billroth. The patient was a boy aged thriteen years. He had suffered for two years from a tumour situated at the outer and inferior wall of the orbit, and which, by its gradual increase, pressed the eye inwards and upwards. The tumour was elastic and somewhat fluctuating. The absence of inflammatory symptoms at any period of its growth precluded the idea of an abscess, and it was imagined to be a vascular tumour of some kind; but on the introduction of a grooved needle, clear fluid alone escaped. It was then regarded as a cyst. Sharp inflammation followed the puncture, and, suppuration occurring, its excision was determined upon. An excision was made over the swelling, which was found to be situated between the periosteum of the orbit and the connective tissue investing the ocular muscles. The cyst was divided, and a vesicle about the size of a hazel-nut protruded, which was soon recognised to be an echinococcus. This makes the fifth recorded case of echinococcus in the orbit.

THE EUCALYPTUS GLOBULUS.

The eucalyptus, discovered in 1792, belongs to the Australian continent. The tree has grown as high as 100 yards, measuring 28 ft. in circumference. The antipyretic properties of the leaves seem to have been long known to the natives, but became evident amongst Europeans about forty years ago, by the crew of the ship *favorite*, who, decimated by ague, stopped the ravages of the fever by the use of eucalyptus leaves. M. Ramel, in 1866, brought the virtures of the plant before the Academy of Medicine of Paris; and Professor Gubler began forthwith to make clinical trials. The properties seem to reside in an essence, or, rather, a kind of liquid camphor, to which the name of eucalyptol has been given. M. Carlotti, a physician of Ajaccio, considers the essence extremely valuable in ague, especially where quinine has failed; but M. Gubler has not had opportunities to experiment with it sufficiently in intermittents to give an opinion. He prefers administering the leaves in powder, the dose varying from 60 to 240 grains. A decoction has also been given in capsules containing 3 grains each. From 6 to 20 capsules may be given per diem. It would be very desirable to try the plant on a large scale.

CHANGES OF THE BLOOD IN SCURVY.

M. Chalvet, in a paper contained in *L'Union Médicale*, No. 121, 1871, states that his examinations of the blood of scorbutic patients showed that it was paler and thinner than natural; that it took a longer time than usual to coagulate; and gave a very small but dense blood-clot, with an exceptionally large quantity of serum. The proportion of fibrin amounted to about 2 or 3 parts in the 1000, but as improvement took place in the general condition of the patient the proportion fell to somewhat over 2 per 1000. The red corpuscles at the acme of the disease amounted to from 68 to 100 per 1000 parts, the normal quantity being about 130. Becquerel and Rodier, on the other hand, found from 162 to 176 of red corpuscles in the 1000. The amount of water in scorbutic blood amounted to 853·5, as opposed to 779·5 in health. The proportion of albumen was 72·3 per 1000, whilst that of the blood in health is 68·7.

CHLOROFORM AIDED BY MORPHIA.

Messrs. L'Abbé and Guyon, in a paper offered to the Academy of Sciences of Paris, propose, after experimenting on animals, to inject a solution of morphia before chloroform is inhaled for the purpose of anæsthesia. Claude Bernard has already shown that the narcotic effect of chloroform is obtained more rapidly than usual when combined with morphia. The authors think that the absence of feeling lasts longer with chloroform and morphia, and that the doses of the former may be greatly reduced, much risk being thus avoided. They also consider that the morphia should be used some time before the chloroform is begun, as the injected solution is not very quickly absorbed. Many will probably side with us when we say that it looks rather hazardous to combine such powerful narcotics. Further experiments and the report of the committee appointed to examine the matter will throw some light on the subject; but too much caution cannot be used.

A MODE OF DEMONSTRATING MINUTE PORTIONS OF SUGAR IN THE URINE.

Prof. Seegen states (*Centralblatt*, No. 5) that even Trommer's test is not very satisfactory where small quantities of sugar only are present in the urine, and that it does not detect such amounts so readily in urine as in watery solutions, since the whitish cloud that is formed when sugar is really present is indistinguishable from that produced by excess of uric acid. He finds that a great increase of sensitiveness can be produced by filtering the urine through blood-charcoal two or three times successively. By this means not only is the color removed, but the uric acid is almost entirely retained by the charcoal, and then the reduction of the copper by Fehling's test may be very clearly seen. No quantitative estimate of the amount of sugar present can be made after the urine has thus been filtered through charcoal, as a certain portion is retained in the filter.

THE MUSCULAR SYSTEM OF THE SMALL BRONCHI.

On this subject Professor Rindfleisch, in the *Centralblatt* of Feb. 3rd, makes the following remarks:—1. The smallest bronchi have a special and distinct layer of transversely disposed muscular fibres, which, in the places where the former debouch into infundibula, are specialised into a sphincter; they are capable of considerable dilatation, and have under their epithelial layer a finely meshed network of capillaries, like those of the lungs. 2. The circular fibrous tracts of the smaller bronchi send loop-like processes into the mouths of the infundibula, which penetrate to the fundus of the same. At from two to four points, tracts of smooth muscular fibres surround the infundibula in an annular form; these muscular rings lie mostly in the inwardly directed portions of the alveolar septa. 3. All these muscles are, in the so-called brown consolidation of the lung, in a condition of hyperplasia; those who have seen them in morbid specimens will be able to recognise them, though they are very delicate, in normal lungs.

STATISTICS OF STONE IN THE BLADDER.

Dr. Eve has published at Philadelphia a synopsis and analysis of 100 cases of lithotomy and lithotrity operated upon during a practice of thirty years. From some comments in the *Gaz. Hebd.*, we find that, as to children, seventeen of them were under five years, and twenty-eight between five and fourteen years. Of these forty-five cases of lithotomy only three proved fatal. Of the fifty-five cases above fourteen years, eight died—namely, 11 per cent. Eighty-seven were operated on by the bilateral method, and eight of these patients died. As to the race of those who were operated upon it is interesting to notice that six only were negroes, six mulattoes, and eighty-eight were white people. The great number of successful bilateral operations is very striking.

THE HISTORY OF CHOLERA.

The *Gazette Hebd.* has just brought to a close a series of no less than seven long articles on this subject, written by M. Tholozan, physician to the Shah of Persia. This essay will be read and consulted with much benefit by all those who take an interest in this disease. The author says at the end of his last article:—"In forty-one years, viz., from 1830 to 1871, five great epidemics of cholera have swept over Europe; two came from Asia, and two from western countries. A great many local outbreaks, both secondary and tertiary, and some regional epidemics were also observed. The only authenticated respites were from 1837 to 1847, a period of about ten years; and from 1861 to 1865, a period of four years. Thus we have only fourteen years non-epidemical out of forty-one years; the total of the epidemical years being thus as many as twenty-seven.

ANATOMICAL CHANGES IN THE SPINAL MARROW AFTER AMPUTATION OF A LIMB OR SECTION OF ITS NERVES.

M. Vulpian says, in a paper addressed to the Academy of Sciences of Paris: "After the amputation of a limb the dimensions of the moiety of the cord supplying the limb are considerably lessened in all directions. The posterior cornu and corresponding fasciculus suffer most, as shown by Dr. Dickinson, and are, in fact, the seat of simple atrophy; but there is no interstitial myelitis nor fatty degeneration. This happens at all ages, but more rapidly with the young." By experiments upon animals M. Vulpian convinced himself (by simply dividing nerves without amputating) that such division is the real cause of the medullary atrophy above described.

A RAPIDLY FATAL CASE OF CHOLERA IN PARIS.

Professor Bouillaud mentioned at a late meeting of the Academy of Medicine of Paris (March 5th, 1872), that he observed, ten days before, a rapidly fatal case of Asiatic cholera at the Hôtel Dieu. All the worst symptoms were present: haggard countenance, cyanosis, cold extremities, cramps, sickness, rice-water stools, suppression of urine, loss of voice, and very low temperature (48° in the axilla, and 50° in the rectum). Death took place the same day of the attack, and on examining the intestinal mucous membrane, it was found covered with a vesicular eruption. There was here neither epidemic nor contagious cause; the man resided in Paris, and M. Bouillaud asks, supported by this fact, whether there is really any difference between Asiatic and European cholera.

LIEBREICH'S CHLORIDE OF ÆTHYL.

This substance was used instead of chloroform in Langenbeck's wards in 1870, and with the best results. It gave rise to no vomiting; sleep was induced easily, but was less profound than with chloroform. Of course it was expected that the latter would soon be superseded, and that the chloride would prove quite innocuous; but a death under its influence, which lately occurred, proved that absolute safety in the use of anæsthetics has not as yet been obtained.

PERFECT OCCLUSION.

M. Cousin states, in the *Bull. de Thér.* (Jan. 15th, 1872), that to obtain such occlusion a thin sheet of gutta-percha should be used. Cut it twice the size of the surface to be occluded, and, with a camel's-hair brush, apply along the margins, to the width of about half-an-inch, some chloroform, and place the sheet immediately on the skin. Energetic adhesion at once takes place, and all the irregularities of the skin are moulded on the gutta-percha.

SCHIFF ON NUTRITION.

At the conclusion of his lectures on this subject, the learned professor says:—"Nutrition is a complicated function; we cannot as yet appreciate the importance of the factors which take a share in the general end of nutrition, and much less to decide upon the particular origin of the disturbance of nutrition observed in inflammation." It is plain that the field of inquiry open before young physiologists and pathologists is sufficiently extensive.

News Items, Medical Facts, &c.

Bromide of Potassium.—It has been said that the prolonged use of bromide of potassium was injurious to the economy in many ways—notably that it caused loss of memory, diminished the appetite, reduced the weight of the body, and even predisposed to phthisis. With a view of satisfying themselves on these points, the following observations were made by the medical officers of the Sussex County Lunatic Asylum, Haywards Heath. Ten patients, four females and six males, were ordered the bromide, in doses varying from twenty to forty grains, three times a day. Their weights were taken weekly, and the effects carefully noted. It would appear from these observations that, when the bromide is given in doses not exceeding one drachm daily, it almost invariably increases the weight of the body; and that even where this dose is doubled the weight need not necessarily diminish, but rather that the chances are in favor of its increasing. The appetite was never impaired, and the functions of the secretory and excretory systems were not perceptibly affected in any instance. As regards the alleged failure of memory, no opinion could of course be formed from observations derived from insane patients; but the medical officers from whom we are quoting state that they have not, in their experience of the remedy, had any evidence in support of such a view.

Effects of Meteorological Facts on Insanity.—The Rev. T. E. Crallan, M.A., chaplain to the Sussex County Lunatic Asylum, has been attempting during the last four years to trace any possible relations that may exist between meteorological facts and the mental and physical conditions of the insane. He states that the accessions of epileptic fits have, as a very general rule, been preceded or accompanied by considerable alteration in atmospheric pressure, or solar radiation, or both; and he is led to the inference that it is not the moon but the change in the weather which directly affects epileptic patients. So far as his observation goes, he concludes that any marked change of atmospheric pressure, solar radiation, or both, either in the same or contrary directions, is almost certain to be followed by an increased number of fits among the epileptics, or by a development of mania or melancholia. Sometimes all three forms of disease are augmented at once, sometimes only one; and it is deserving of notice that very often the maniacal and melancholic patients seem to be affected in opposite ways, the latter being well when the former are excited, and the converse.

PRINTED AND PUBLISHED BY

Wm. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 8. NEW YORK, AUGUST, 1872.

PRACTICAL PHYSIOLOGY.

Lectures
ON
EXPERIMENTAL PHYSIOLOGY.

By Wm. Rutherford, M.D., F.R.S.E.,

Professor of Physiology in King's College.

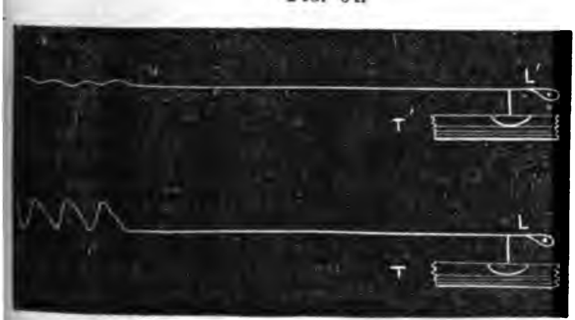
LECTURE XIII.

CIRCULATION—(continued.)

The Pulse.

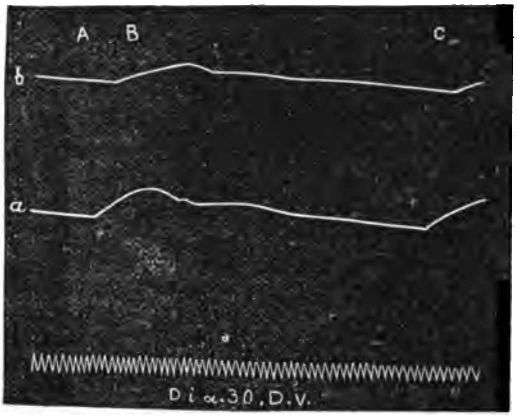
Gentlemen,—At our last demostration we studied the blood-pressure more particularly, and we also directed our attention to various phenomena connected with the pulse. You now know, from the experiments performed by us, that the pulse is essentially a temporary distension of a blood-vessel produced by a quantity of fluid being suddenly projected into it from the heart. The artery is always filled with blood. If its wall were rigid, the heart could not project fluid into the vessel faster than it flows out: this fact you have demonstrated experimentally. But the blood-vessels being elastic, the mass of blood projected into them at intervals causes them to distend in order to accomodate the increased amount of fluid. This distension disappears as the blood escapes from the arteries. Let us rest a light lever (Fig. 64, L) on an elastic tube, through which we will pump some water. We bring the free end of the lever against a cylinder covered with smoked paper, so that the motions of the lever may be recorded. As we imitate the heart's action, and propel fluid through the tube in an interrupted manner, the lever rises with every distension and falls with every collapse of the tube. If you place your finger on the tube and feel its pulse, you find that the lever is only rendering more evident to the eye what may be perceived by the sense of touch. If we close the exit aperture of the tube and drive fluid into the tube, the lever rises, but does not fall again, because of course the tube cannot collapse; but the moment the fluid is permitted to flow out the lever falls. You see, then, that the ascending limb of the pulse-curve indicates distension, while the descending limb indicates collapse of the tube. Let us put a second lever on the tube (L', Fig. 64). The first lever (L) rests on a part of the tube (T) which is close to the pump. We now place the second lever on the tube at a much greater distance from the pump. A piece of tubing about six feet in length intervenes between the two levers. Now we shall find out whether or not the pulse-wave is the same at every part throughout the length of the tube; and we shall also be able to measure the velocity of the pulse-wave. As its progress is rapid, we record time on the cylinder by means of a pitch-fork. We carefully arrange the points of the two levers and the writing point of the pitch-fork, so that all the three, when at rest and horizontal, will be perfectly vertical—that is to say, they all touch a vertical line which we draw upon the paper. We throw the fork into vibration, the cylinder into revolution, and we send pulse-waves at intervals through the tube. Let us examine the tracings. (Fig. 65.) You see at a glance that the pulse-curve (*a*) produced by the lever next the pump begins sooner than that (*b*) produced by the lever at a greater distance from the pump. It is very easy to measure the time represented by the interval between their origins. We have only to intersect the diapason-trace and the two pulse-traces by vertical lines (A and B), and count the pitch-fork vibrations enclosed by these two vertical

Fig. 64.



ines. In this case there are three and a half double vibrations (the two limbs of the letter v represent a double vibration); thirty double vibrations are equal to one second; so 30 D V : 1 sec. : : 3·5 D V : 0·1166 sec. In order to find out the velocity we must now measure the length of the tube between the two levers. In this case it is 79 inches. The wave has therefore taken 0·1166 second to travel along 79 inches of this tube. 0·1166 sec. : 79 in. : : 1 sec. : 677·7 in., or 56·4 ft. The velocity of the wave therefore is in this case 56·4 ft. per second. But you must all have noticed that when we produce waves in water—e. g., by an oar—we can make the waves go with different velocities according to the amount of force with which we start the wave. Here, in like manner, the velocity of the wave varies with the initial force with which it is projected from the heart; or, more precisely, the velocity of the pulse-wave bears a direct ratio to the force and velocity with which the pump is compressed. Of this fact you can very readily convince yourselves by experiment. That such waves take a very appreciable time to travel is a fact which might have been anticipated by anyone. Here, however, is a figure (66) given by Marey which has misled many regarding this important point. I show it you in order that I may warn you against the snare from which even some physiological instructors seem to be unable to extricate themselves. (See Dalton's Physiology and others.) The curves in this figure are represented by Marey on squared paper in order that their relative points of origin and relative heights may be readily perceived. The curves are produced by three levers at different distances along an elastic tube. The arrangement of the whole was similar to that observed in the experiment which you have just witnessed. Curve A was produced by the lever nearest the pump, curve C by the lever furthest away, while B was caused by a lever in the median position. The left ends of the waves are their origins. You see that the unhappy diagram makes all the waves begin at the same moment, as is indicated by the waves all starting from the same vertical line. How Marey ever published a figure so extraordinary I cannot imagine. But in justice to him I tell you that nine years ago he retracted his error. His retraction, however, seems to have escaped many readers.

Fig. 65.



Dia. 30, D. V. 30 double vibrations of a diapason per second. *a*, Pulse-curve produced by the lever near pump; *b*, that produced by the lever far away from it.

Fig. 66.

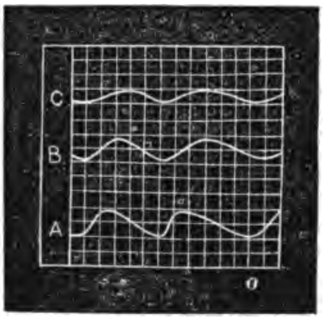
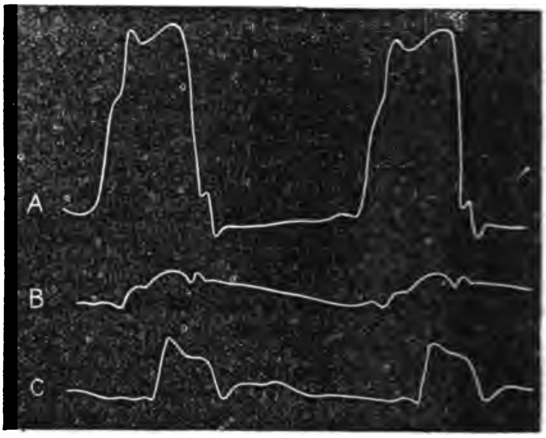


Fig. 67.

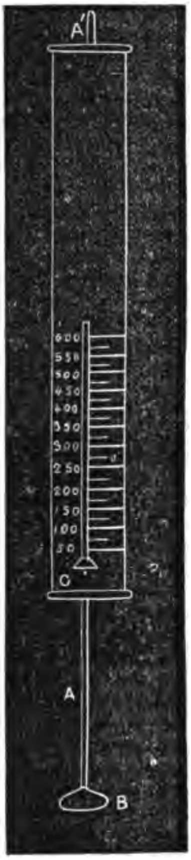


Here is an instructive figure (67), given by Chauveau and Marey, representing the transmission of the pulse-wave in the arteries of the horse. The upper line (A) indicates the motion of the left ventricle; the middle line (B) represents the pulse-waves in the aorta; while the lowest line (C) shows the pulse-waves in the femoral artery. All the tracings were recorded simultaneously. The relative duration of the various events and the time at which they occur are indicated by this valuable drawing. You see at a glance that the pulse-wave begins to rise in the femoral distinctly after it has begun in the aorta. (Read the tracing from left to right.) The pulse-wave appears in the distal arteries later than in those near the heart. It may be observed that at different parts in the length of even a simple elastic tube the shape of the pulse-wave is not the same. Near the pump the ascending limb of the wave is much more nearly vertical than it is several feet further along the tube. (See Figs. 64, 65, 66.) The crest of the wave may be said to be "retarded" as the wave passes along the tube. What is the explanation of this? Simply that the force of the wave diminishes as it travels along the tube, and that in consequence of this diminished force the distension of the tube does not take place in the distal part of the vessel with a velocity so great as in the part of the vessel near the pump. The greater the velocity of distension the steeper, of course, is the ascending limb of the wave. When we feel the pulse with the finger, it is the crest of the wave, in other words, the greatest degree of distension of the artery, which produces the greatest sensation. The pulse at the wrist is commonly said to be one-tenth of a second behind that in the carotid. This statement is made from a comparison of the crests of the waves. I would caution you against the error of supposing that the velocity of the pulse-wave and of the blood-current mean the same thing. You have often seen waves produced by the wind or some other agent upon a stream of running water. You must have noticed that when the waves pass down the stream they go much faster than the current. This fact is rendered evident by leaves or other light things floating on the stream. They are carried along by the stream, and have a velocity almost exactly the same as the particles of water. Yet the waves easily overtake and outstrip these things. The pulse-waves and the blood-stream constitute an analogous case. The particles of blood travel through the carotid at a velocity of 10·5 inches per second (Vierordt); the pulse-wave travels at the rate of 28·5 feet per second (Weber); probably, however, the velocity of the pulse-wave is greater even than this.

The pulse-wave, as it passes along a simple elastic tube of even uniform calibre, diminishes as the distance from the pump increases. (See Figs. 64, 65, 66.) Why is this? The height of the wave must obviously much depend upon the distensibility of the tube; but the tube we have used in our experiment is equally distensible throughout its whole length. Hence the difference in the height of the waves is not to be explained by any difference in the distensibility. Perhaps you might jump to the conclusion that the pulse-wave diminishes as it travels along the tube merely because the force of the pump becomes spent: to wit—it becomes transformed into heat by friction; the mechanical motion becomes in part converted into thermal motion. But this is not the whole truth. There is a stopcock at the free end of our elastic tube. During the formation of the waves which we have just been studying the stopcock has been quite open, the fluid has been flowing freely from the tube, and there has been little resistance. Now see the effect of turning the stopcock so as to diminish the aperture of exit, and thereby increase resistance. As we increase the resistance to the exit of the fluid, the wave produced in the tube near the stopcock increases in height, and the difference in height between it and that produced in the tube near the pump grows less and less as we increase the resistance. In a tube of uniform calibre and uniform extensibility the height of the wave is the resultant of two forces—(*a*) the propelling force and (*b*) the resisting force. Both the propelling and the resisting forces are greatest in the tube at the pump; hence the wave is highest there. When, however, we have to compare the pulse-waves produced in tubes of different degrees of distensibility, we have to take into account the effect of this factor upon the height of the wave. The wall of the aorta is not nearly so distensible as that of the femoral artery; and, in consequence of this, the pulse-wave in the aorta is not so high as that in the femoral, although the former is nearer the heart. (Fig. 67.)

[The part of the lecture dealing with the further consideration of pulse tracings and of the sphygmograph is omitted, owing to lack of space, and because so much has lately been written upon these subjects. The best form of sphygmograph seems now to be Marey's, modified by Mack and Béhier. Mack has made the spring of Marey's instrument transmit its motions directly to the writing lever; and Béhier has added a simple piece of apparatus for ascertaining the amount of pressure exerted by the spring upon the artery. Dr. Sanderson has converted one of Marey's myographs into a tambour sphygmograph. He has described this instrument in his lectures (*Medical Times and Gazette*, April 29th, 1871). I have represented it as adapted to my scheme of the circulation (Fig. 70, s). Although useful for taking long tracings, it is not so well adapted for ordinary purposes as Marey's sphygmograph. Much yet requires to be done ere the value of the sphygmograph can be fully understood. It is a dangerous instrument in the hands of the unwary. The labor attending its use, and the mistakes to which it can so easily lead, seem to be the cause of its general neglect. Nevertheless, it is a useful instrument for showing pulse-irregularity, and for indicating more precisely than the finger the pressure that can be borne by the pulse. It has proved serviceable in ascertaining changes wrought in the circulation by the use of therapeutic agents.]

Fig. 68.



Dr. Handfield Jones has invented what appears to be really a good instrument, and one that is likely to in some measure take the place of the sphygmograph for estimating the strength of the pulse. It (Fig. 68) resembles a spiral spring letter-weigher. In place of the weighing disc, there is an ivory knob (B) fixed at the end of a steel rod (A, A′). This rod is fastened to a spiral spring. There is a steel pin (C) connected with the rod, which pushes up a little index that points to the figure of the scale, and thus shows the amount of pressure in grammes which is applied to the ivory knob (B). In using this very simple instrument you ask the patient to place his hand in a vertical position. You feel the radial pulse close to the carpus with the forefinger of your left hand. You then take the instrument in your right hand and press its knob (B) over the radial, on the cardiac side of the point at which you are feeling the pulse. You carefully increase the pressure of the knob upon the vessel just until you cease to feel any pulsation with the finger which you have upon the pulse. You stop, and then read off the figure on the scale to which the index points. This indicates the pressure-weight in grammes which is necessary to stop the transmission of the pulse. The pressure required varies with the power of the heart. The instrument gives a more accurate indication than the finger can of the strength of the pulse.

FIG. 69.

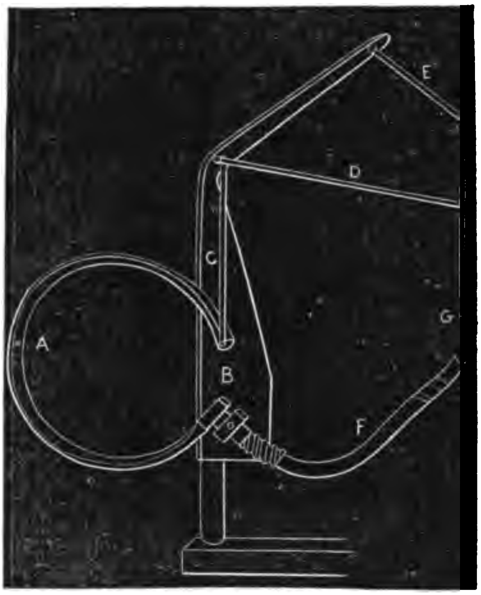
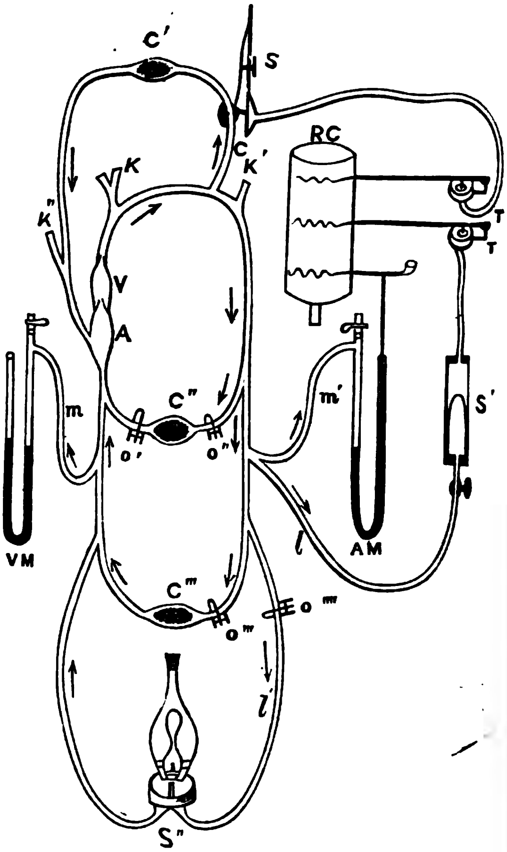


FIG. 70.



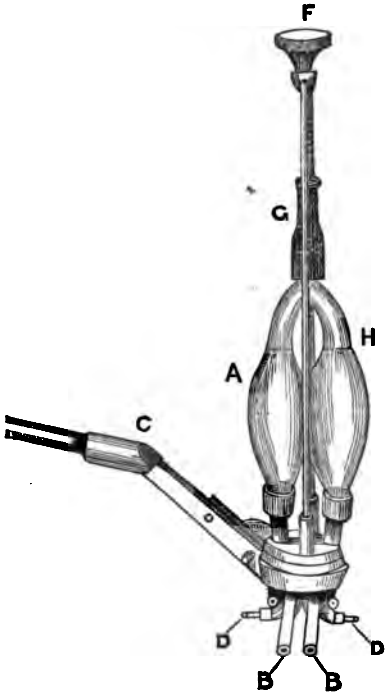
Rutherford's schema of the circulation, showing how it may be adapted for studying the use of various instruments—such as the sphygmograph (S) (Sanderson's tambour sphygmograph here-shown), the sphygmoscope (S'), the stromuhr or hæmometer (S''), and the hæmodynamometer or kymograph (A M). T T', Two polygraphs. R C, Cylinder. A, Auricle of heart. V, Ventricle. C, Carotid. C' C'' C''', Bags containing sponge representing capillaries of head, abdomen, and feet. O' O'' O''' O'''', Clamps for compressing the vessels. A M, V M, Arterial and venous manometers or hæmodynamometers. *m* and *m'*, Tubes connecting these with an artery and with a vein respectively. *l*, Tube for sphygmoscope. Arrows indicate the direction in which the fluid is projected when the pump is compressed. (A full description of this schema has been given in THE LANCET, May No., 1872. The figure is reproduced here with the sphygmograph, sphygmoscope, stromuhr or hæmometer, revolving cylinder, and pen in connexion with the arterial manometer.)

In experiments on the lower animals we sometimes want the pulse-curve. In that case we do not use the sphygmograph because it is difficult to apply it and keep it applied. In them the pulse-wave can be far more easily and accurately obtained by means of instruments placed in communication with the interior of the bloodvessel. We have seen that the mercurial hæmodynamometer gives the pulse-waves. But this instrument, though well adapted for measuring the blood-pressure, is not suited for giving us any information regarding the pulse further than its number merely. A column of a heavy fluid like mercury is not suited for the transmission of delicate movements. Fick's spring manometer or spring kymograph (Fig. 69, and Marey's sphygmoscope, Fig. 70, S') are excellent instruments for registering the pulse-motions. Fick's instrument consists of a hollow spring (A) filled with alcohol—a light mobile fluid. One end of the spring is open, and communicates with a tube (F) filled with a saturated solution of sodium bicarbonate. The free end of this tube is brought into communication with an artery by means of a nozzle. As the pulse-wave flies into the interior of the spring it causes it to partially unbend. Its free end (above B) rises and moves a system of levers (D, E) and vertical bars (C, G), one of which bears a pen (G) for recording the motion. Fick's instrument gives the pulse-curve admirably; but it is somewhat inconvenient to use. The blood-pressure can be ascertained by means of it, but certainly with considerable trouble. The instrument, is therefore little used. Marey's sphygmoscope (S', Fig. 70) consists of a glass tube, communicating at either end with a short leaden tube. The glass tube contains a cone of vulcanised india-rubber, so fixed that its cavity is in connexion with that of one of the leaden tubes. This tube, together with the elastic bag, are filled with a saturated solution of sodium bicarbonate. It is brought into connexion with the cavity of an artery by means of a glass nozzle and a piece of elastic tubing, all filled with soda solution. The tube at the opposite extremity of the sphygmoscope is joined to a polygraph, so that the pulse-motions may be recorded. This instrument is an excellent one, capable of giving the pulse-curve extremely well, and, moreover, it is easily used. It should be employed along with the mercurial kymograph, as I have represented in the following figure of my scheme. The mercurial kymograph registers the pressure, while the sphygmoscope gives the pulse-curve. The simplest way to use it is just to join its tube to that of the mercurial kymograph.

The Hæmometer.

I give this name to an instrument (Fig. 71) invented by Ludwig and Dogiel, and called by them the stromuhr. The main object of the instrument is to measure the quantity of blood that flows through a vessel in a given time,—just as a gas-meter measures the quantity of gas that flows through a tube in a given time. If the calibre of the vessel and the quantity of blood which flows through it in a given time be ascertained, the velocity of the stream can be readily calculated. This instrument is therefore also a tachometer—that is, a measurer of velocity. It is the only hæmometer, and it is the best tachometer, as yet invented. The instrument you see consists of an inverted Y-shaped glass tube (G, A, H). The two oblique limbs of the Y are blown into an elliptical shape. They communicate inferiorly with two metal tubes inserted into a metal disc perforated in two places to receive them. The metal disc is articulated to a similar disc placed below it, in such a manner that the one can rotate upon the other round a vertical axis to the extent of half a circle. The lower disc has, like the upper one, two holes. Into these are insterted two bent tubes, and to each tube may be fixed a nozzle (D and D′), which is inserted into the bloodvessel. These tubes of the lower disc communicate with other two (B and B′), to which a hæmodynamometer may be attached, for the purpose of ascertaining the blood-pressure. The lower disc is immovably fixed by a pair of screw forceps and a metal rod (C), which may be supported on the head rod of Czermak's rabbit-holder (Fig. 22, THE LANCET, Sept. No., 1871). Let us use the instrument, and you will now readily understand it. There are nozzles (D and D′) of different sizes, so that we can select a nozzle of the same size as the bloodvessel into which we are going to insert the instrument. We expose the carotid of a chloralised rabbit, measure its breadth with a pair of compasses, and then select a nozzle as near the size as may be. We fasten these nozzles to the instrument; with a syringe we inject into the upper limb (G) of the glass bulbs defibrinated blood heated to 100° F., and let it fill the tubes at the lower part of the instrument. When it flows out by these tubes we hold their nozzles, and let the blood accumulate until it fills perfectly both tubes as high as the commencement of the glass bulbs. Now we rotate the upper part of the instrument through an angle of 30° or 40°, and leave it in this position. This cuts off the holes in the upper from those in the lower disc, and thereby prevent the escape of the blood. To simplify matters, we will dispense with the use of the hæmodynamometer for the present; so we close the openings of its tubes (B B′) with wooden plugs. We next isolate the carotid, and clamp it with bullnose forceps at the upper and lower part of the neck, cut the artery across between them, and insert into either end the nozzles of the instrument. Let us put D into the cardiac end, and D′ into the cranial end of the artery. We keep the clamps on the artery, for we do not at present want the blood to flow. We are not ready. We now fill the bulb A with defibrinated blood up to the line at its top, and lastly fill the bulb H, and all that remains of the cavity of the tube, with olive oil. We put a cork in the india-rubber tubing that surrounds the upper part of the glass tube G, and secure it by a ligature carefully, because the pressure of the blood will be apt to expel it, seeing that it has been so well oiled. Last of all, we put a brass handle (F) over the instrument, and fix it on two pins of the upper disc. This will enable us to rotate the upper disc readily and safely. Now you see the bulb A is full of blood, and the bulb H full of oil. We unclamp the carotid both above and below. The blood will flow in by D, and out by D′, when I rotate the upper disc, so as to bring its holes into communication with those of the lower disc. I now place bulb H in connexion with tube D. You see the blood flows steadily into it. It rises in bulb H, expelling the oil from it over into bulb A, and driving the defibrinated blood out of bulb A into the cranial part of the carotid. The moment the blood reached the line at the upper end of H, we whirl round the upper disc, so that A is now brought into connexion with tube D, and H into connexion with tube D′. Now the stream runs into A, driving the oil from it, and the blood from H. This being completed, we whirl the upper disc round again, so as to reverse the operation, and so on. The two bulbs have an equal capacity; we only require to know this capacity, and the time which the blood takes to fill the bulb, in order to ascertain how much blood flows through the carotid in a given time. How shall we best measure the time which the blood takes to fill the bulb? We take a registering apparatus with continuous paper (Fig. 29, Feb. No., 1871), and record time upon it with a metronome apparatus (to be afterwards described), and we cause the hæmometer to

FIG. 71.

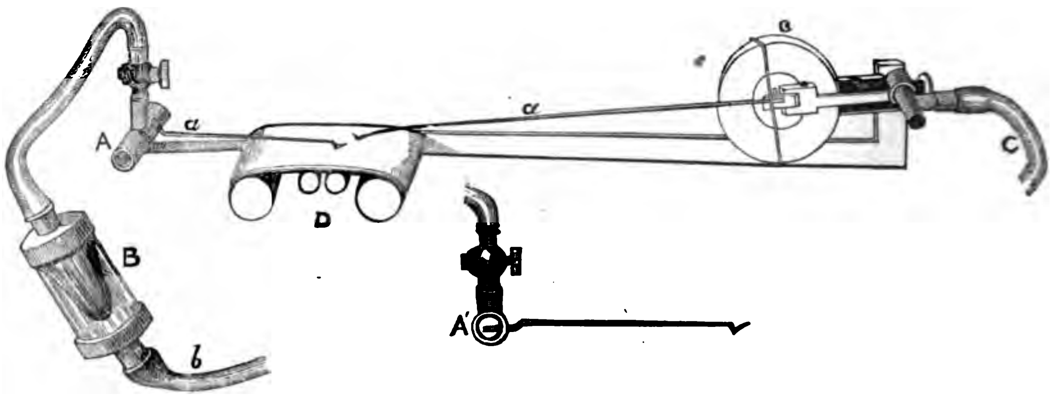


Ludwig's hæmometer, or stromuhr.

trace a horizontal line upon the paper, and to make a vertical stroke at each turn of the upper disc. (This is done by bringing it into connexion with an electro-magnetic apparatus. The description is omitted, however, until the figure of the metronome apparatus alluded to above is published.)

The amount of blood going to most parts varies from time to time, the variation chiefly depending upon the contraction of the small arteries of the part—e. g., when food is introduced into the stomach its bloodvessels dilate and permit of the passage of much more blood through the viscus. Also in the kidney the amount of blood which passes through it varies much. The hæmometer enables us to measure the quantity of blood and its variations in such cases. On this account the instrument is a very valuable one, and will doubtless be much employed for ascertaining the action of drugs, &c., upon the circulation in various parts.

Fig. 72.



Chauveau and Lortet's tachometer or hæmo-dromometer (A and A'). *a*, Its pen. D. Cylinders with paper for registration. B, Sphygmoscope. *b*, Its tube, supposed to be joined to *c*. *c*, Polygraph. *a'*, Its writing-pen.

Velocity of the Blood.

Various instruments have been invented with a view to ascertain the velocity of the blood, and the changes which it undergoes in the arteries and veins. It is worth while that I mention Chauveau and Lortet's tachometer (A, A', Fig. 72), on account of the principle alluded to in its construction. It consists of a metal tube (A) having a hole in its side. The hole is closed by a piece of vulcanised india-rubber, in which a thin plate of aluminium (*a*) is inserted. The inner end of this plate projects across the interior of the vessel, while its outer end is prolonged as a thin rod, bent at its end into the form of a pen, which records the motions of the bar upon a revolving cylinder. An artery is divided and the metal tube fixed into the vessel. As the blood-stream traverses the tube it causes the metal tongue to deviate from its rectangular position, the amount of the deviation bearing a direct ratio to the velocity of the stream. The deviations of the tachometer-pen may be compared with the pressure by connecting a sphygmoscope (B) with the tachometer-tube. The sphygmoscope is connected with a polygraph (C) which records its motions upon the same cylinder (D). This instrument can only be used in a large animal, such as a horse, and therefore it is not one which is nearly so serviceable as the hæmometer above described.

Ludwig's hæmometer enables us to ascertain not only the quantity of blood which flows through a vessel in a given time, but also the velocity. It is the best instrument for showing deviations in velocity which extend over a few seconds. Unlike Chauveau and Lortet's instrument, it cannot show deviations which last for only one or two seconds. But Lortet's instrument can only be used with an animal such as a horse, and its results are even then not always reliable, while the hæmometer can be used with dogs, rabbits, &c.

[The foregoing observations on velocity are shortened owing to the length of this lecture.]

◆

Clinical Lecture

ON THE

TREATMENT OF "PERSISTENT" INFLAMMATION

BY THE

LOCAL APPLICATION OF SOLUTIONS OF THE OLEATES OF MERCURY AND MORPHIA.

Delivered at University College Hospital, March, 16th, 1872.

By John Marshall, F.R.S.,

Professor of Surgery in University College, London.

Gentlemen,—I wish to speak to you to-day about certain new combinations of two very old remedies which have been much used in surgery—combinations presenting many advantages over the forms of those remedies ordinarily in use, and possessing a value of which I have now had ample experience, having frequently tested their efficacy during the last three years. They consist essentially of solutions of oxide of mercury in oleic acid—being, in reality, the oleate of mercury dissolved in oleic acid; but to these solutions is added a certain quantity of morphia, which, in its uncombined state, is readily soluble in, and doubtless unites with, the oleic acid. I have employed these mixed oleates of mercury and morphia chiefly in the treatment of what I would term prolonged or "persistent" inflammation, which is at once obstinate and difficult to control.

Inflammation, as you know, is a local disease, involving the occurrence of certain changes in the part affected. It is characterised by a state of irritation or morbid excitement of the sensory nerves, by a diminished functional activity of the vasimotor nerves and of the walls of the bloodvessels, by the supervention of exudation and the emigration of white blood-corpuscles, and sometimes by the formation of new organised products, derived by processes of evolution from the pre-existing anatomical elements. When these several conditions occur in a healthy person, when their causes are transitory, and the surrounding circumstances favorable, the nervous irritation and vascular disturbance subside, the products of exudation, emigration, and new formation are absorbed, and the local disease is straightway cured.

But, not unfrequently, owing to various causes, the conditions of inflammation, whether they extend or remain limited in area, become prolonged or "persistent" in a given locality. I prefer the term "persistent," to distinguish this state, to either of the more specialised terms "subacute" or "chronic"; for such state may be, and often is, more acute than subacute in its type, and it may, and often does, cause more constitutional disturbance, though less consequential degeneration of the tissues involved, than is produced by a truly chronic inflammation.

This prolongation or "persistence" of local inflammation may be due to various causes, local or general. Thus the parts affected may not be kept sufficiently at rest, as when a joint is exercised too soon after injury or inflammation. Or the interruption to the cure may be due to the continued performance of some natural function—as, for example, in the case of the tonsils, the glottis, certain orifices of the body, the mammary gland, or uterus,—the condition induced, in such cases, being not so much chronic as "persistent" and severe. So, also, if the general health be disordered—if a gouty, rheumatic, or syphilitic, an anæmic or plethoric condition should exist,—a local inflammation may not disappear, but may be prolonged or become "persistent." Again, supposing this condition to be once established, the products of the inflammatory process, either by their presence or as a consequence of the chemical changes which they themselves undergo, may excite or exhaust the functional activity of the neighboring nerves, bloodvessels, and other tissues, and may thus induce further irritation, leading, if not to the extension, at least to the prolongation of the inflammatory process. Inflammation of this obstinate type may, of course, terminate in suppuration, ulceration, or gangrene; or it may, by some interference with the nutrition of the affected tissues, impair or destroy the elasticity or strength of parts which serve mechanical uses in the body, or deeply influence their special vital functions.

Now, in the treatment of all inflammation, three chief ends must be kept in view—namely, the relief of the local nervous irritation, the stimulation of the vasi-motor nerves and the bloodvessels, and the promotion of the absorption of the inflammatory educts and products. The mode by which this absorptive process is actually accomplished is so far normal that these inflammatory products and educts undergo a series of changes—namely, softening, degeneration, and solution—quite similar to those through which certain healthy tissues pass when they are no longer needed in the economy; as, for instance, the tissues of the uterus and mammary gland after parturition and lactation are completed. In the quasi-normal as in the normal process, the veins and lymphatics are the agents of absorption. Finally, when the morbid constituents or products are removed, the nervous irritation and the vascular disturbance are allayed, and the part regains its ordinary or healthy condition. In many cases, time, rest from mechanical use or functional activity, and freedom from disturbing influences, are sufficient to ensure this natural process of recovery. In a certain number of instances it will happen that a purgative, a diaphoretic, a stimulant, a tonic, or a sedative, will indirectly promote absorption; and thus salines, alcohol, quinine, iron, and morphia, may really act as absorbent remedies, and so especially will food and pure air.

But in the management of the prolonged or persistent inflammation above referred to, further local measures are often indispensable; and, for the purposes of practical illustration, let us suppose we have to deal with persistent inflammation of a *joint*. Here, as of primary importance in the local treatment, must be mentioned immobility and rest, as secured by means of proper splints or other apparatus, accompanied by the use of a sling for the upper limb, and by the maintenance of the recumbent posture for the lower limb. Next may be considered the application of uniform and judicious pressure, by means of cotton-wool confined by suitable bandages. Heat and cold have also their peculiar advantages in particular cases. Lastly, certain medicated applications have frequently to be resorted to. Whether these are employed in the form of lotions, fomentations, liniments, ointments, or plasters, they usually contain one or more of the following substances as their active ingredients—viz., alcohol, ammonia, camphor, turpentine, cantharides, iodine, or mercury, with or without chloroform, opium, morphia, or belladonna. Each of these most efficacious remedies is, no doubt, adapted to particular stages of persistent articular inflammation; but every surgeon meets with numerous instances of such inflamed joints which, in spite of care and good treatment, continue obstinately uncured for weeks or months. With some, blisters, and with others, iodine, is the favorite remedy in such cases. Scott's ointment and the mercurial liniment are likewise often employed with great advantage, in both of which applications mercury is the most active ingredient. But mercurial ointment, the basis of these preparations, is merely a mechanical mixture of minute globules of mercury, or of solid particles of the black oxide, with some unctuous substance; and I have long thought that, if we could employ a *solution of mercury in some oleaginous or unctuous medium*, we should obtain more immediate, rapid, and satisfactory results from the well-known therapeutical powers of this ancient remedy. The other mercurial ointments now in use, made with the nitric oxide, the subchloride, the ammonio-chloride, or the red iodide, are also essentially mechanical mixtures, and all, as well as the nitrate ointment, are too irritating to be used as topical absorbents. In seeking for my object I first dissolved some of the perchloride of mercury in a small quantity of ether, and added to it about four times the amount of oleic acid; but I found that this combination freely used on the skin produced much irritation, unless it was employed in too dilute a form to be of service as an absorbent. In Gmelin's chemistry there is a short account of certain metallic oleates formed by double decomposition; but with this as a guide, I failed to obtain any satisfactory oleate of mercury. My friend, Mr. Frank Clowes, to whom I then referred the chemical question, soon discovered that, although the ordinary sublimed scales of red oxide of mercury were with difficulty dissolved in oleic acid, the oxide, precipitated by caustic potash or soda from a solution of the metal in nitric acid (which is a yellow impalpable powder) is, when recently made and well dried, readily soluble in oleic acid, especially when aided by a temperature of about 300° Fahr. At my request Messrs. Hopkin and Williams have since studied the subject pharmaceutically, and have succeeded in preparing oleate of mercury, and certain solutions of that salt in oleic acid. The strength of the preparations made by them is indicated by the percentage of the oxide of mercury which they contain. The 5 per cent. solution is a perfectly clear pale-yellow liquid, resembling olive oil, but thinner; the 10 per cent. solution is also fluid and perfectly clear, but as dark as linseed oil; whilst the 20 per cent. preparation is an opaque yellowish unctuous substance, closely resembling in appearance resin ointment, melting very readily at the temperature of the body, and forming a kind of transparent, viscid, colorless varnish when applied to the skin. The chief care to be observed in the manufacture of these solutions is not to hurry the process, and not to employ a high temperature, or the mercury will be immediately reduced.

Unlike the mercurial ointment so long in vogue, which is a crude, gross, unscientific mixture, very dirty and very wastful, because so small a proportion of its mechanically admixed mercury is but slowly absorbed, these solutions of oleate of mercury are cleanly and economical in use; and as the diffusibility or penetrating power of oleic acid is much greater than that of ordinary oils or fats, and as each one-thousandth part of even a minim of these new preparations contains its proper modicum of mercury, they are absorbed by the skin with remarkable facility and manifest their remedial effects with great promptitude. They should not be rubbed in like ordinary liniments or embrocations, but should be *merely applied with a brush, or be spread lightly over the part with one finger;* otherwise they may cause cutaneous irritation, or even produce a few pustules on the skin, especially in certain persons. This result may, however, be obviated by the addition of a small quantity of olive oil, or purified lard, according as an oleaginous or an unctuous preparation is required. Any of these forms may be scented by the addition of essential oils.

In employing these mercurial solutions for combating persistent inflammation of joints, I soon found that the addition of morphia was of very great advantage. For this purpose the simple alkaloid must be used, as neither the hydrochlorate, the acetate, nor the meconate is soluble in oleic acid. For every drachm of the solution of oleate of mercury in oleic acid one grain of morphia may be added. Being, as well as the mercury, completely dissolved, it quite as rapidly penetrates the skin, comes quickly into contact with the extremities of the nerves, and thus, even within a few minutes, acts upon them at their most sensitive points, and speedily produces a soothing effect.

The oleates of mercury and morphia, thus united in one preparation, represent, as it were, a liniment, ointment, or plaster of mercury and opium; but they are far more elegant, economical, and efficacious. As a rule, according to the size of the part affected, from ten to thirty drops are sufficient for one application. This should be repeated twice daily for four or five days, then at night only for four or five other days, and afterwards every other day, until a cure is obtained. The morphia immediately begins to relieve pain, allays the nervous irritation and consequent vascular turgescence, and thus arrests the progress or "persistence" of the inflammatory process; whilst the mercury probably promotes the death and degeneration of the morbid products, and so facilitates their subsequent removal by absorption. Unless used in excessive quantity, the oleate of mercury does not salivate, or produce any marked constitutional disorder.

As some of you will remember, I recently tested, in the hospital, the value of these combined oleates in the treatment of a case of "persistent" inflammation of the knee-joint. This affection, I need hardly say, is often difficult to cure, on account of the large size of the articulating surfaces, ligaments, and synovial capsule, and also on account of the great weight supported at the joint, and the frequent disturbance of the parts in the unavoidable movements of the limb and body. Frequently, indeed, this disease leads to irreparable injury to the knee. In the case in question, occurring in a man aged forty-four, the disease had already lasted upwards of eighteen months. From effusion into and about the joint the knee was greatly swollen and disfigured; there was not only synovitis, but, in addition, inflammation and thickening of the fibrous capsule and the ligaments of the joint. He complained of great and constant pain, of a creaking feeling or noise when the knee was moved, and of occasional starting pains at night, probably indicative of threatened or actual softening of cartilage. The skin over the knee was œdematous, and deeply stained with iodine, the remedy which had chiefly been relied upon before his admission under my care. After this stain had been removed, from twenty to thirty drops of the solution of oleate of mercury and morphia (5 per cent. of the former with one grain of the latter to the drachm) were applied to the knee night and morning for about ten days, and then at longer intervals. A piece of linen was kept around the joint; over this was put a smooth, thick layer of cotton-wool, and then the limb was rather firmly bandaged from the foot to just above the knee. During this treatment, the patient, who was of course confined to his bed, rapidly improved in all respects; and at the expiration of four weeks he left the hospital, able to walk by the aid of lateral leather splints, and very far advanced towards a cure. From the first day of their application the effect of the remedies was decided and prompt. I could adduce many other examples equally satisfactory. I select the two following:—

1. A gentleman, aged thirty-one, suffered, about eighteen years ago, for some weeks from an attack of fever, followed by consecutive abscesses in the left hip-joint, which led to stiffness, shortening, and wasting of the corresponding limb. On his recovery, the right leg was necessarily subject to over-use, which, together with occasional blows received at foot-ball and otherwise, brought about repeated attacks of capsule-synovitis in the right knee. These at length resulted in chronic thickening of the joint. During the last three or four years, owing chiefly, it is said, to the increasing weight of the body, renewed attacks of inflammation more or less acute led to a state of "persistent" inflammatory disease of the joint, which became much enlarged, deformed, somewhat flexed, and almost immovable. There was pain on the slightest motion, especially on twisting the leg, and such extreme tenderness that the patient shrank from even an attempt at examination of the knee. There was scarcely any power in the limb; and there were nocturnal startings on falling asleep. Finally, the joint at one part seemed on the point of suppurating, as indicated by increased local heat, redness, and œdema, and by the occurrence of slight shiverings. In consultation with Dr. Sheldon, I ordered absolute rest in bed, the use of two long well-fitted leather splints, and the employment of the combined oleates of mercury and morphia, as above indicated. The first application afforded manifest relief to the local pain. This and the exaggerated tenderness soon subsided, absorption was established, and the size of the joint became so reduced that the splints speedily required renewal; and in four weeks there was complete freedom from pain in and about the knee, and the patient was soon able to walk on crutches with comparative ease. Thus in a month, a joint, already very seriously implicated and threatened with suppuration, was practically saved. I can confidently say that I know of no other treatment which would have afforded such satisfactory and speedy relief. I doubt whether Scott's plan would have succeeded in controlling the disease present in this knee; certainly it would not have done so with such promptitude.

2. The case now to be related was equally demonstrative of the efficacy of these combined oleates. A young lady at school in London, having had a series of inflammatory attacks affecting the left wrist, each of which had been subdued by appropriate treatment, came to me with great exaggeration of her worst previous symptoms, owing to a fall she had met with a month or so before her visit. Her wrist was hot, painful, and much swollen; there was distinct fluctuation on the back of the joint, and the hand was slightly displaced forwards at the radio-carpal articulation; but the joint was so painful that no very minute examination could be attempted. At the expiration of a week after the commencement of the use of the combined oleates, a marked improvement was manifest. After four weeks, the disease was sufficiently abated to admit of the application of a close-fitting leather splint; but the ultimate result must be uncertain, as I detected a very distinct grating sensation in the joint, and more or less fluctuation still remained opposite to the lower end of the ulna.

I will now advert as briefly as possible to the use of these same preparations of mercury and morphia in the local treatment of "persistent" inflammation affecting other parts or organs of the body. Indeed, their applicability and utility appear to me to be almost coextensive with the occurrence of such inflammations themselves, provided only that the seat of the disease be in, or sufficiently near to, the skin.

I may first mention that not only in persistent articular inflammation, but also in simple synovitis, these remedies rapidly relieve the tenderness and pain, and promote the absorption of the fluid effused into a joint. They are also of decided benefit in the rheumatic, the arthritic, and the mixed forms of joint disease; but in these they do not, of course, supersede the necessity for general treatment. In inflammation of the mammary gland occurring during or after lactation, or altogether independently of that secreting process, their efficacy is unequivocal; for I have seen, not only the induration left after previous abscesses speedily disappear under their use, but a tendency to recurrent suppuration in the site of old abscesses, and the threatened formation of new ones, entirely controlled and arrested. I have also seen a threatened abscess in the perineum from inflammation of one of Cowper's glands, and likewise the troublesome indurations left after ordinary perineal abscess, rapidly disappear on the use of these preparations. In obstinate and painful tonsillitis, in epididymitis, in periostitis, and in inflammation with imminent or actual suppuration in or around lymphatic glands, I have similarly employed them with decided advantage. In hydrocele they have not appeared to be useful. I have used equal parts of the 20 per cent. ointment and purified lard applied outside the eyelid, with success, in hordeolum and in palpebral conjunctivitis.

In many cutaneous affections the oleate of mercury solutions, without morphia, form elegant and powerful remedies. It was in a case of obstinate sycosis menti that I first used, and with excellent results, an etherial solution of the perchloride of mercury mixed with oleic acid; but I now much prefer, as equally efficacious and far less irritating, the five per cent. solution of oleate of mercury in oleic acid, with the addition of an eighth part of ether. This, when applied to the skin with a camel-hair pencil, is a most diffluent and penetrating remedy. It enters the hair-follicles and the sebaceous glands, penetrates the hairs themselves, and carries everywhere with it its powerful metallic constituent. Besides sycosis, it will cure chloasma and the various forms of tinea; it is useful in porrigo and in pruritus ani et pudendi; but I have not found it serviceable in non-specific psoriasis or in eczema. The solution of oleate of mercury destroys pediculi immediately; and, owing to its singular power of permeation, simultaneously kills the ova—a result not always certain when ointments containing undissolved mercury are used.

Again, in many of those syphilitic affections for the cure of which mercury is applicable, the oleate-of-mercury preparations offer some advantages. Thus, in congenital syphilis, a piece of the 20 per cent. ointment, about the size of a pea or bean, placed in the child's axillæ, night and morning for five or six days, rapidly and easily, and without any sign of uncleanliness, produces constitutional effects. Even in the adult this mode of introducing mercury into the system, either for the cure of syphilis or other disease, may often be preferable to, and less troublesome than, the bath, and it certainly gets rid of the objections to the ordinary mode of inunction. As a topical remedy for certain local manifestations of syphilis, such as the non-ulcerated forms of syphiloderma, especially when these disfigure the head, face, neck, or hands, the 10 per cent. solution is a most valuable adjunct to other treatment, the spots rapidly disappearing under its use. This or the 20 per cent. preparation, diluted with equal parts of purified lard, may also be applied to non-ulcerated syphilitic indurations and condylomata, but it gives pain if applied to surfaces much excoriated or ulcerated, to moist warts, or to mucous membranes. In syphilitic iritis, and also in non-specific forms of that disease, this diluted oleate ointment smeared over, not within, the eyelids, evidently promotes the absorption of the effused lymph. Lastly, in some of the remoter kinds of syphilitic affections, which iodide of potassium will usually cure, such as very hard nodes and certain forms of syphilitic testicle, the external application of the oleate of mercury is very valuable. I have seen a case of enlarged testicle and epididymis, the syphilitic origin of which had not been suspected, and for which no mercurial course had been prescribed, but which during a period of six years had been, from time to time, relieved by enormous doses of iodide of potassium, speedily and decidedly benefited by the inuction of the 20 per cent. mercurial oleate.

In reference to other uses of the combined oleates of mercury and morphia, I may remark that I cannot doubt their value in the treatment of "persistent" inflammation of certain internal parts and organs—as, for example, of obstinate pleurisy, pneumonia, pericarditis, and endocarditis; for they would here also allay pain and nervous irritation, would thus contribute towards the arrest of progressive disease, and would likewise promote the process of absorption. Moreover, I may state that a solution of morphia in oleic acid (one or two grains to the drachm), without mercury, is an excellent typical remedy in neuralgia, and in that exquisitely painful affection, herpes zoster, care being taken not to produce cutaneous irrition by friction. I have also used, endermically, with advantage, a solution of atropia in oleic acid, and have had prepared for me the oleates of zinc and copper. Each of these preparations will probably come to have its uses, to which, however, I have now only time thus generally to refer. Oleic acid is likewise a ready solvent of cantharidin and croton oil. It is itself aperient, and permeates fæces more readily than olive oil. Indeed, I fully anticipate that this acid, as well as its compounds with mercury and with morphia, besides other preparations made by its aid, will eventually be admitted into the Pharmacopœia. An ointment of the oleate of mercury would almost supersede the old-fashioned blue ointment, whilst the solutions of the oleates might replace the liniment of mercury. The remaining mercurial ointments of the Pharmacopœia, of which the nitrate may contain a little oleate or some allied salt, will, however, still have their special uses.

Clinical Lecture

ON THE

USE OF HYDRATE OF CHLORAL IN CERTAIN DISEASES OF THE CHEST.

Delivered at the Liverpool Royal Infirmary,

By A. T. H. Waters, M.D., F.R.C.P.,

Physician to the Infirmary.

Gentlemen,—I wish to call your attention to-day to the use of hydrate of chloral in diseases of the chest; and I will first consider its value in reference to its hypnotic properties. A want was long felt for some agent which would procure sleep without causing the injurious effects of opium, and there can be no doubt that chloral supplies this want in a remarkable degree.

The first cases that I shall refer to in which I have found chloral very valuable are cases of bronchitis, and especially bronchitis complicated with emphysema of the lungs, where, in consequence of the profuse secretion and diminished expectorating power, life is threatened, and indeed is often destroyed, from apnœa. There can be no doubt of the injurious effects of opium, speaking generally, in such cases, if given in sufficient doses to procure sleep. It tends to load still further the bronchial tubes, it checks expectoration, and increases the liability to death from apnœa. Various other forms of hypnotics may be tried; but when a decided effect is wanted, I have little confidence in their value. Not so, however, with chloral; and I will illustrate its effects by referring to one or two cases, which must serve as types of those in which you may use it with great advantage.

On the 15th of March, 1870, J. B——, a sailor, twenty-six years of age, was admitted into the Northern Hospital, under my care, suffering from all the symptoms of severe general capillary bronchitis, with emphysema. The pulse was quick, the dyspnœa urgent, and the patient had that leaden hue so characteristic of general emphysema when attended with bronchitis. Moreover, there was a good deal of spasm of the bronchial tubes. Carbonate of ammonia, sulphuric ether, and lobelia were ordered, together with six ounces of brandy daily. The man complained that he had not slept for some days before admission, and for four days after admission he was sleepless. On the 19th of Marth—the fifth day of treatment—the bronchitic symptoms were somewhat improved, but from want of sleep the patient was getting exhausted. In such a case as this—"suffocative catarrh," with profuse secretion and very imperfect aëration of the blood—opium in its various forms is inadmissible: first, because it will rarely, if ever, produce sleep; and, secondly, because, if it do produce sleep, it will probably increase the pulmonary symptoms, and endanger the life of the patient. Speaking for myself, I never in such cases sanction its administration, unless in very exceptional circumstances. But to return to our patient. I ordered him on the 19th of March twenty grains of chloral, to be taken every night. On the first night he slept for six hours; on the second, for about the same time; on the third, he slept almost uninterruptedly from 10.30 P.M. to 6.30 A.M.; and subsequently he slept well. The chloral was continued for twenty-four nights. It never failed to produce sleep, and it never caused any unpleasant symptoms. During this time the patient took a mixture of iron and sulphuric ether, with six ounces of brandy daily. He steadily improved, and was discharged from the hospital on the 3rd of May.

[Another case of a similar character to the last, but occurring in private practice, was referred to, in which, previous to the exhibition of chloral, morphia had been given. The morphia produced a good deal of excitement, and was not followed by sleep, but by an increase of the pulmonary symptoms. The chloral—twenty-five grains—produced sleep shortly after it was given, and the patient woke the next morning much improved; it was continued every night with the same general measures as were previously adopted. The patient steadily improved, and was soon out of danger.]

I have referred in detail to these cases because they are types of a class, and I wish to impress the main features of them on your memory. They are highly instructive. In both instances sleepless nights had existed for some time. In both there was severe dyspnœa, with profuse bronchial secretion and greatly impaired aëration of the blood. Both were unsuited for the exhibition of opium; in one case it had been tried and had signally failed. In both cases the chloral was given without any modification in the general treatment, and in both it produced sleep of a refreshing kind, and thus, in my opinion, materially hastened the recovery of the patients. Moreover, it did not fur the tongue, impair the appetite, nor confine the bowels; nor did it lose its effects, for the dose was not increased throughout; indeed, in the last case it was, I believe, reduced to fifteen grains towards the close of my attendance.

I might refer you to numerous instances of a similar character to those which I have detailed, but it is quite unnecessary. The important practical point I wish to illustrate is that chloral is specially applicable for the purpose of producing sleep in cases of bronchitis, and that it may be safely given in those cases in which opiates act very prejudicially.

Whilst on the subject of bronchitis, I may mention that I have given chloral, in many cases, in small doses for the purpose of allaying cough, and I have found it very useful. I give it in four or five-grain doses, with syrup of tulo or tincture of orange-peel, and in this manner I have found it effectual in checking irritating cough, depending on chronic or subacute bronchitis, and thus in producing sleep.

I have found chloral useful in procuring sleep in cases of asthma, both of the purely spasmodic kind, and when complicated with emphysema of the lungs. It has apparently prevented the accession of spasm and dyspnœa during the night, and thus enabled the patients to sleep well. I have seen good effects from it in spasmodic cough, as in the following instance:—

I was consulted in May, 1870, by a gentleman who told me that he had suffered for about fifteen years from severe attacks of dyspnœa and cough. He was much emaciated and was seized with a violent fit of coughing when he came into my room, and he told me that every night he awoke with an attack of a similar character. My examination led me to conclude that this was a case of spasmodic asthma with much bronchial irritation, but there was slight dulness at the apices of both lungs. I prescribed twenty grains of chloral to be taken every night, and a mixture, containing five grains in each dose, to be taken whenever the cough became troublesome. Three days afterwards the patient reported to me that he had passed much better nights, and had not been disturbed by the cough; that in fact he had slept from 10 P.M. to about 5 or 6 A.M. This case was under my observation for some time, and the chloral continued to give relief to the dyspnœa and cough.

I now pass on to speak of the use of chloral in phthisis. I have administered it in many cases of this disease, both in the advanced and early stages, with the view either of procuring sleep (in doses of fifteen to twenty grains), or of allaying cough (in doses of four or five grains). In many of the cases I have found it useful, but in some it has failed. In these latter, however, with the exception of giving rise to slight temporary unpleasant symptoms which I shall allude to hereafter, I have failed to notice any injurious result from its use. It is very valuable, and really becomes curative, in producing sleep in certain cases of phthisis, where sufficient sleep is not obtained and where morphia or opium in any form is not tolerated. You will meet with instances of the kind where patients do not progress, apparently from the simple reason that they do not get enough sleep. If you give an opiate the tongue becomes furred, the bowels get confined, the appetite fails, and in fact the sleep is purchased at too dear a rate. In such cases you may safely give the chloral, and I have been able to date the commencement of a decided improvement from the period of its exhibition, from the simple fact that it caused sleep without interfering with any of the organic functions.

I have given chloral in cases of pleuritic effusion with the view of relieving the distressing cough which is sometimes present; and I have found it at times useful, but at other times it has failed to do any good—a circumstance which might be fairly anticipated. I have tried it to relieve cough in acute pneumonia and in pericarditis, but I have seen no good results from its use. Opium is far more valuable as an anodyne and hypnotic in these latter diseases.

And now I will call your attention to the use of chloral in cases of heart-disease. The following will illustrate its value:—

P. C——, fifty-nine years of age, was admitted into the Northern Hospital, under my care, on the 9th of June, 1870. He said he had been ill for about eighteen months. He had a somewhat sallow aspect, and a faintly-marked upper segment of the so-called "arcus senilis." From the commencement of his illness he had been from time to time attacked with dyspnœa and a sensation of impending suffocation; but there had been no decided angina. His attacks had become more frequent and more severe, and he had therefore sought admission into the hospital. He complained of being unable to sleep, or even to lie down, and said he passed his nights sitting up in bed. On examination, I found all the characteristics of aortic disease, with regurgitation, together with hypertrophy of the heart. It was clear, therefore, that the patient was suffering from cardiac asthma; and the sleeplessness which often exists in such cases, and was so strongly marked in this one, is not a sleeplessness which can be beneficially treated by opium. Stimulants given at night sometimes act well, but they frequently afford only very slight and temporary relief.

I prescribed for this man twenty grains of chloral, to be taken every night. The first night he took it he went to sleep about an hour after it was given and slept for four hours. He soon went to sleep again, and slept till morning. He continued the chloral every night with the most satisfactory results. He took during the day a mixture of iron and sulphuric ether, with a small quantity of whisky.

For some weeks he remained free from any decided attack of dyspnœa. He felt at times as if an attack were coming on, but it always passed off; and whereas he used, before he took the choral, to have a fit of dyspnœa every morning between 5 and 6 o'clock, he now did not suffer at all. He continued well and free from any attack till the beginning of July, when he had diarrhœa, and his breathing became somewhat worse; but there was no asthma till the 12th, when he had some slight attacks during the night. All medicines were omitted for a few days. On the 18th he had a slight attack; and on the morning of the 20th a very bad one. The chloral was accordingly resumed, when he again slept well, and was free from attacks —with the exception of one on the 5th of August —up to the time of his discharge, August 13th.

The following is a case of much interest:—I prescribed in April, 1870, twenty grains of chloral every night for a gentleman who suffered from the following symptoms. About Christmas, 1869, his breathing became bad, and for twelve weeks before I saw him he had been unable to lie down, and had always rested in a chair at night. He said that usually about twelve o'clock at night he went to sleep for about an hour, when he woke up with a start and did not sleep again. He was seventy-two years old, and very feeble. There was œdema of the legs, a feeble action of the heart, a slight roughness with the first sound, but no distinct murmur, and no increased cardiac dulness. The pulse was 100. I considered the case to be one of fatty degeneration of the heart. I prescribed iron, and directed that twenty grains of chloral should be given every night. As the patient came from a distance I gave instructions that the effect of the chloral should be carefully watched by his medical attendant. I saw the gentleman again in about a fortnight. There was not much change in the general symptoms, but he reported that since he had taken the chloral he had passed somewhat better nights. I recommended that the chloral should be continued in thirty-grain doses. I did not see the patient again, but about ten weeks afterwards I had the following report of him from one of his relations:—From May 10th he had taken thirty grains of chloral every night. This produced about three hours' sleep regularly. He still slept in his chair. On one occasion he omitted to take his draught, thinking he could do without it. He passed a very restless and sleepless night, and did not repeat the experiment. This gentleman died some months after the date I have just alluded to. He continued to find great relief from the chloral, and there can, I think, be no doubt that his life was both prolonged and rendered more comfortable by its use.

I have related these cases because they are types of a class. I have now used chloral in many of a more or less similar character, and, speaking generally, I have rarely been disappointed with its effects. In one of the cases related there was valvular disease of the heart, in the other I could detect no decided murmur, but the heart was feeble, and had, I doubt not, undergone fatty degeneration. The relief afforded to the asthmatic symptoms in the first case was very marked and not transitory, and in the second case the sleep which attended the exhibition of the drug rendered a life previously wretched tolerable, and tended to check the downward progress of the disease. I think there is no other known remedy which could have been given in either of these cases with so much safety or success as chloral.

In cases of heart disease where there is albuminuria, I have not seen good results from the exhibition of chloral, and I always hesitate to give it where the kidneys are diseased.

I will now refer briefly to some of the unpleasant symptoms which I have found following the exhibition of chloral. I gave twenty grains one night to a gentleman about fifty-five years of age, who was suffering from weak and, probably, fatty heart, œdema of the legs, subacute bronchitis, with emphysema of the lungs and slight paralysis agitans, the latter of which had existed for several years. His nights had been almost sleepless and very restless, and there had been slight delirium. He was in the habit of taking small doses of Battley (fifteen minims), and sleep usually followed; but if the dose were taken persistently, his other symptoms invariably increased. I saw him the day after I had ordered him the chloral; he had passed a bad night, had not slept, and said the medicine "drove him wild." I urged him to give it another trial, and I doubled the dose, making it forty grains. I was told the next day that he had passed a still worse night than before, and that early in the morning he was found wandering about the house in a very excited state. I therefore stopped the chloral; in fact, the patient flatly refused to take any more. I may state that he had been an inveterate smoker, and was taking, under my directions, a moderate amount of alcoholic stimulants, which amount, I have reason to believe, was exceeded. I may also remark that he afterwards derived great benefit from the use of bromide of potassium, which, although it did not always produce sleep, rarely failed to give a certain amount of rest.

Now in reference to this and other cases of a similar character which I have met with, it appears to me that there is a risk in giving chloral where there is chronic alcoholism; and, further, that when the patient is in the habit of smoking much, as well as drinking freely, the probability of excitement following the use of chloral is greatly increased. In another case in which I gave chloral it produced symptoms somewhat like those of the last patient referred to. A man was in the hospital suffering from advanced phthisis and I prescribed chloral for him at night to procure sleep. It answered tolerably well for a time, producing a fair night's rest; but after he had taken it for about ten days, he was seen getting out of bed during the night, and he fell down in an unconscious state. He was at once lifted into bed. In the morning he had no recollection of what had happened. I prescribed twenty grains for a woman who was admitted into the hospital suffering from phthisis. Ten minutes after she had taken the draught she called out for the nurse, said she felt "queer," and began to cry. She afterwards went to sleep. When she awoke in the morning she was quite stupid as if drunk, and even at 11.30 A.M. slight symptoms of drunkenness remained.

I am disposed to think that chloral, when taken habitually for a long time, has a depressing effect on the heart—that it, in fact, enfeebles the circulation, and this should be borne in mind in prescribing it.

Chloral sometimes produces vomiting, but, if certain precautions are taken, this will rarely happen. It should be well diluted or it will cause a burning sensation in the throat. I usually prescribe it with tincture of orange-peel or syrup of tolu, and an aromatic water. It ought to be taken by the patient when he is in bed, and he should at once compose himself for sleep; walking about, even crossing a room, after it is taken, will often lead to its being vomited.

Clinical Lecture ON LITHOTOMY.

By Eben. Watson, A.M., M.D.,

Professor of Physiology in Anderson's University, and Surgeon and Clinical Lecturer on Surgery to the Royal Infirmary, Glasgow.

Gentlemen,—During the last few weeks I have, in your presence, twice performed lithotomy after the method proposed by Dr. Andrew Buchanan, of this city, and therefore it forms the most natural subject for our consideration this morning. I wish especially to direct your attention to this operation because it is very slightly, if at all, noticed in text-books on surgery, and, in my opinion, it is not by any means sufficiently valued by the profession. I believe, indeed, it is easier, both for surgeon and patient, than any other method of extracting stone from the bladder, and that its results will compare favorably with those of any other operation of the kind. The two cases which are the occasion of this lecture ended, as you know, quite successfully, though in one of them the man was fifty years old. They make up the number of times I have performed this operation to forty-seven, and of these only two have died, or 1 in 23½; and I may briefly state here that in neither of the two cases in which the patient died could it be said that death was the direct result of the operation. In one it was caused by pyæmia; and, on inspection of the body, I discovered an abscess at the fundus of the bladder, which I thought of older date than the operation. In the other case erysipelas attacked the neighborhood of the wound, and extensive suppuration and sloughing in the pelvis ensued before death. This patient was about fifty years of age; the former was a child. But even if we count both as deaths from lithotomy, the result is still favorable, even when compared with the apparently less formidable operation of lithotrity. Thus, if we look at the somewhat contradictory statistics of the latter operation, we find that, while Civiale states that he only lost 1 in 42 cases, Malgaigne and other Parisian hospital surgeons give their mortality at 1 in 4 to 1 in 8; Sir Benjamin Brodie lost 9 in 113 cases, or about 1 in 12½; Crichton, 1 in 5; Sir Henry Thompson, 1 in 14½; while the mortality in my cases of lithotomy has been only 1 in 23½.

Then if the ease of the patient after lithotomy be compared with the anxiety which he must endure after lithotrity and during its frequent repetitions, as well as his distress from the fragments sticking in the urethra and often causing retention of urine, one cannot but regard the cutting operation with greater favor than the one by crushing. When the patient is under chloroform during the performance of lithotomy his sufferings are in the great majority of cases ended by the operation almost as completely as the pain of toothache is terminated by a skilful extraction; and I have known a patient after lithotomy sleep for several days and nights with but short intermissions, just from his entire freedom from an agony which had disturbed his rest for months before. Yet I confess that this preference which I have for lithotomy in the majority of cases is founded on my conviction of the safety of Dr. Andrew Buchanan's operation when rightly performed, and I shall therefore mention to you somewhat minutely the steps of that operation, with some of the lessons regarding them which experience has taught me.

The patient having been laid on a convenient operating table and brought under the influence of chloroform, the rectangular staff should be introduced into the bladder and the stone felt with it. The staff to be selected for use in any case is the largest that can be readily introduced into the urethra. It is of consequence that the rectangular part of the staff should be quite passed into the bladder, and the handle brought down between the patient's thighs, to be held there for the time by an assistant. The limbs are then to be tied up as usual, but I prefer passing the bandages round the thighs to the practice of tying the feet and hands together, both because under chloroform it is needless to fasten the hands, as the patient cannot move them, and because if they are left free there is no interference with the movements of the chest in respiration.

The operator now sits down before the perineum, and he should take care that the hips are projected over the end of the table, and that the body of the patient is lying quite straight before him. He now places the staff as he wishes it to be held during the operation, and of course on this depend the parts cut and the success of the whole procedure. The index finger of the left hand should be introduced into the rectum and the prostate should be felt, while at the same time the operator should take the handle of the staff in his right hand. He should then raise the long limb of the staff at right angles to the table and to the axis of the patient's body, and should place the angle of the staff just anterior to the prostate gland, and therefore in the membranous part of the urethra. Keeping the long limb of the staff in this position, he should now depress the instrument towards the rectum, until, in fact, the angle is nearly on a level with the junction of the skin and mucous membrane of the anus. One other little manœuvre is still required—viz., to turn the handle of the staff a little to the right, for by this means the short limb of the staff is inclined to the left side of the perineum.

The good of this is, that when the point of the knife enters the groove the operator feels more sensibly the fact of its being there by the opposition which the inclination of the staff occasions to its passage to the bladder. This will be perceived at once on trial. If the staff is held right before one, he hardly feels the sliding of the knife along its groove; but if it be held at a slight angle to him, he feels it all the way as he pushes it along to the stop at the end of the short limb. The staff is now committed to the charge of the assistant, who is to hold it exactly as it is given to him during the whole procedure.

It will be observed that in this position the long limb of the staff is nearly parallel with the triangular ligament, and that its angle is not made to project in the perineum, though this was at first supposed to be necessary. In truth, it is not at all desirable to make it project, because the doing so disturbs the parts and inclines the point of the staff dangerously upon the rectum. It ought also to be remarked that the angle of the staff is fairly below the central point of the perineum, and that, as the incision is altogether below it, the deep perineal fascia and the transverse muscle and artery of the perineum are uninjured in this operation, while they are all divided in the operation of Cheselden. I think this the essential feature of Dr. Andrew Buchanan's operation and the one on which its success depends.

The operator retains the index finger of his left hand in the rectum, and puts the thumb of the same hand on the long limb of the staff a little above its angle, of course having the soft parts between them, and thus he makes sure of where the angle of the staff is, for it must correspond to the angle made by the thumb and finger thus placed. The depth at which it lies is very various in different perineums, but it is seldom very great. I know that it has been stated at two lines, but it is often much more, as in fat children and in tall and corpulent adults. However, it is never so great but that one who is practised in the operation can easily plunge his knife directly into the groove of the staff. To do so one must hold the knife above the hand, and raise it so that its blade shall be parallel with the short limb of the staff. Most of the difficulties which I have known to be encountered at this part of the operation have arisen from the operator holding the knife as if dissecting, with his hand above its handle, and also from his introducing it at an angle to, or not quite parallel with, the short limb of the staff. I insist that the knife must be held above the hand, and boldly raised to a level with the groove along which it is to be passed. It may then be made to pierce the perineum and to enter the groove. There is usually no need of a preliminary incision. A little practice will enable anyone who is accustomed to use the knife to perform this step of the operation at once; but he must be sure that the knife is really in the groove and not above or below it, or, as sometimes happens, across the staff, for then he is almost certain to do serious mischief. When the knife is fairly in the groove the operator feels a degree of resistance to its passage into the bladder, owing to the slanting manner in which, as I have already said, the staff should be held; and this gives him confidence in running it on till it is arrested by the end of the groove. The knife will then be found to have penetrated through the skin and the anterior layer of the superficial fascia, the upper fibres of the sphincter ani and those of the levator ani, and, passing beneath the triangular ligament, to have entered the membranous portion of the urethra just anterior to the prostate, which gland it has also notched in passing into the bladder.

The surgeon now causes his knife to leave the groove of the staff, and, keeping his right wrist stiff, he draws his blade outwards and downwards to the side of the anus, thus cutting only a few more of the muscular fibres above mentioned, with a few usually small bloodvessels, and enlarging the incision of the prostate. He then places the index finger of the left hand in the wound, and passes it along the staff into the bladder. He will then very often feel and fix the stone with the tip of his finger, and may quite safely desire the staff to be withdrawn. If, however, the subject is bulky, it is better to retain it in the bladder till the scoop or the forceps have been introduced.

In the majority of my operations I have been able to fix the stone at once with my finger, and then I introduce a scoop—which is, you see, somewhat more than usually turned down at the point—over the back of my finger, and carry it round both finger and stone so as to seize the latter as in a forceps. It is thus very easily and neatly extracted; but if the forceps is employed, there is less likelihood of immediately grasping the stone. In fact, it is often strangely difficult to do so; and we are left without any good rules by which we may render this part of the operation at all so precise and certain as the others. If the stone is difficult to find, you should not, however, as is often advised seek for it with your forceps closed, but rather with them slightly open; for thus both limbs of the instrument act as sounds for its detection. I need hardly say that this part of the proceeding must be performed with great delicacy, lest the bladder be injured, and also that the stone when detected may not be moved by a rough touch of the forceps. Of course such difficulty is most likely to be found when the stone is small and in a large adult bladder; and I may here mention a manœuvre practised by the late Dr. Lawrie in this hospital in these cases. It is to pass the forceps into the bladder so that the joint will be at the wound in the prostate, and then to open the blades to their full extent horizontally in the bladder. This stretches the bladder on the blades, and very likely causes the small calculus to roll between them into the middle of the viscus. If the forceps, with its blades still extended, be now made to sweep round half the bladder—i. e., till they and the handles are perpendicular instead of being horizontal—and then closed, it will generally be grasped at once. If not, it may be turned round the other half in a similar manner; for it is strange how the bladder will sometimes contract around and retain a small stone in its folds. I have on one occasion in the living subject proved the usefulness and the success of this manipulation, which I have not seen described in books, and I would have you bear it in recollection.

I have now only to say regarding this operation that I never use a tube, but only insert a slip of oiled lint into the wound to prevent its edges from adhering, and making the first passage of the urine difficult and painful. This lint should be withdrawn in three or four hours after the patient has been laid in bed; and if any pain or uneasiness is complained of, an opiate suppository or a laudanum injection may be given with advantage.

Original Papers.

ON THE DIFFERENTIAL DIAGNOSIS AND TREATMENT OF BRONCHOCELE.[*]

By MORELL MACKENZIE, M.D. Lond.,

Physician to the Hospital for Diseases of the Throat, Assistant-Physician to the London Hospital.

IT has recently been remarked, with great truth, by Dr. Parona,[†] of Bologna, that no advance has been made in the treatment of bronchocele in the last fifty years. Since Coindet discovered the medicinal value of iodine the therapeutics of goitre have, indeed, remained almost stationary, and it might truly be said that the general recognition of the curative powers of iodine in relation to bronchocele has actually retarded the "exploitation" of that important disease.

Whilst, however, iodine exercises such a beneficial influence over some forms of goitre, in others it is quite inoperative; and hence it becomes important to differentiate the varieties of the disease which are met with in practice. For clinical purposes bronchocele may be divided into seven varieties—namely, (1) Simple or Adenoid, (2) Fibrous, (3) Cystic, (4) Fibro-cystic, (5) Fibro-nodular, (6) Colloid, and (7) Vascular. The disease may present the characteristic features of any one of the types referred to, or many of the different varieties may be associated together in varying degrees, and in different situations of the gland. This fact will be readily understood when it is borne in mind that the various kinds of bronchocele are generally different stages of development—sometimes progressive, sometimes retrograde—of the simple hypertrophied gland. Exophthalmic goitre, being a disease of an entirely different nature, and one in which the goitre is but a comparatively unimportant manifestation of a neurosis, will not be here considered. In treating of the ordinary varieties the following points will be discussed: the appearance and general symptoms, the course and termination, the pathology, and the treatment.

I may here state that my attention was particularly directed to the subject of goitre at the commencement of the year 1868, and that since then notes have been taken of all cases that have come under my notice. I find that I have seen 211 cases in the last four years; of these, 36 cases have occurred in private, and 175 in hospital practice. Of the 211 cases, 54 have been of the simple variety, 101 fibrous, 39 cystic, 9 fibro-cystic, 6 fibro-nodular, and 2 colloid. As to sex, I have had 27 male patients and 184 females. Amongst the males, 6 were of simple character, 9 fibrous, and 12 cystic; amongst the females, 48 were simple, 92 fibrous, 27 cystic, 9 fibro-cystic, 6 fibro-nodular, and 2 colloid.

Simple or adenoid bronchocele may vary greatly in size, but it generally retains the form of the thyroid gland. Sometimes the hypertrophy is so slight as only to give a graceful fulness to the neck; whilst, on the other hand, the enlargement is sometimes so considerable as to produce a decided deformity. Sometimes the swelling affects only a portion of the gland, one lobe, or the isthmus; but far more often the whole of the gland is more or less involved. So much is this the case that the diffusion of the swelling may almost be said to be one of the characteristic features of this variety of goitre. The other marked peculiarity of adenoid bronchocele is its extreme softness. The margin of the hypertrophied gland can generally be felt; but sometimes so soft and elastic is the swelling to the touch that it has more the feeling of a layer of adipose tissue. This form of bronchocele seldom gives rise to more than slight inconvenience, but occasionally it appears to be the cause of globus hystericus. It has long been noticed that tumours of the thyroid gland follow the movements of the trachea, but this circumstance is of little diagnostic value; for other tumours of the neck, when connected with the windpipe, also follow its movements; and, on the other hand, in large bronchoceles, the trachea is often entirely concealed, and there is no movement of the diseased gland.

Cases of simple goitre occurring sporadically often disappear spontaneously. The endemic cases, however, rarely improve except under treatment, or upon removal from the local causes of the disease. Most cases of simple goitre, if untreated, sooner or later show a disposition to undergo some process of evolution. The most common change is an increase in the fibrous stroma of the gland. This increase of fibrous tissue may be general or partial. In some cases cysts are developed.

The pathology of this variety is extremely simple, as the disease consists essentially of an increase of the normal gland-structure.

The internal administration of iodine is a specific for the endemic variety; and though it likewise often cures sporadic cases, it is frequently unavailing. Simple hypertrophy occurring in weak, anæmic girls is most effectively treated by iron and proper hygienic measures. Cases not yielding to internal treatment may almost invariably be cured by counter-irritation; or the internal and external treatment may be combined. It matters little what agent is used for counter-irritation, so long as considerable blistering is effected. The liquor epispasticus of the British Pharmacopœia is the remedy which I generally employ. It will be found convenient to apply the blistering fluid about twice a week on alternate sides of the throat. The biniodide of mercury ointment, which has been found so useful in India, has also proved efficacious in the hands of some practitioners in this country, notably in those of Dr. Frodsham. I have, however, in my practice, found it less reliable and more troublesome than the simple blistering fluid. No doubt the absence here of tropical sun, the direct rays of which are employed in India after the application of the ointment, accounts for the difference in the effects of this remedy in the two countries. Tincture of iodine, when applied externally, may act as a counter-irritatant, or it may have a specific action. Electrolysis, as introduced into this country by Dr. Althaus, has also proved of great service in some cases. The ointments of iodide of potassium and iodide of lead, when employed in these cases without constitutional treatment, are of little use. In the case of a patient under my care, who was treated by daily external application of iodide of lead, the circumference of the neck actually increased an inch and a half in six weeks, instead of diminishing.

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[*] This paper was read before the Hunterian Society on Jan. 22nd, 1872, and was illustrated by detailed reports of twenty cases and a large number of photographs showing the appearance of patients before and after treatment. In publishing the paper in its present form, it has, however, been thought desirable to give only a few cases typical of each of the more important varieties. The woodcuts are fac-simile tracings of the photographs.

[†] Contribuzione alla Terapia del Gozzo. Del Dott. Francesco Parona (dalla *Rivista Clin.*). Bologna, 1871.

Of the 54 cases of simple goitre of which I have had notes taken, 47 have undergone treatment. In 35 the enlargement entirely disappeared, 5 were lost sight of after a few visits, and in 7 no alteration took place, or so little as not to be appreciated.

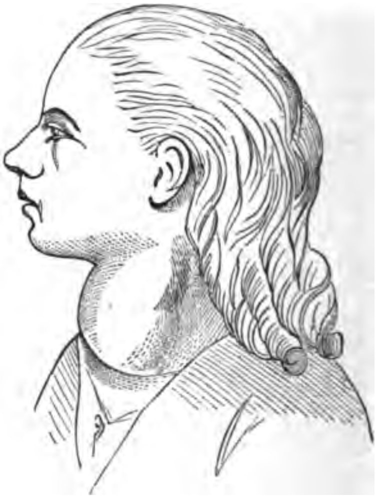
Fibrous bronchocele not only varies in size, but also in position, and somewhat in form. I have seen this variety as small as a walnut, and as large as an infant's head. It may attack either lobe, the isthmus, or the whole gland; but it is comparatively rarely that the isthmus is alone affected. Even when affecting the whole gland, the different parts of the thyroid body which have undergone fibroid hypertrophy are distinctly apparent. To the touch, fibrous goitre is smooth, hard, and unyielding, though occasionally there is a slight amount of elasticity. It does not, as a rule, cause any disturbance of function; but in some cases it presses on the trachea, and produces dyspnœa. There is, indeed, a special variety of fibrous goitre, in which the lateral lobes grasp, as it were, the trachea, constituting what has been well called "suffocative goitre." In these cases, also, the œsophagus is often compressed. Fibrous goitre, when once established, never disappears spontaneously, though it may occasionally develop into the fibro-cystic, or fibro-nodular varieties. The tumour sometimes remains stationary for many years; but it may at any time take on an active disposition. Fibrous goitre, from its tendency to compress vital parts, is more dangerous to life than the other varieties. Pathologically, it consists in an excessive development of the areolar tissue, and a relative diminution of the vesicular structure.

The treatment which I have found most successful in fibrous goitre is the introduction of a seton, passed transversely through the whole substance of the gland. The seton may be of twine or silk, but twine is preferable, as it more quickly produces suppuration. From six to twelve threads are used, according to the size of the tumour. If local anæsthesia is employed, no pain is felt. This plan of treatment, which was revived by Dr. Quadri, of Naples, was also pursued with success by the late Mr. Hey. The more recent pathological views concerning blood-poisoning have caused an undue prejudice against the introduction of setons and issues, but I have never met with a single instance of septicæmia following either of these methods; and when it is recollected how constantly setons were used by our immediate professional predecessors, in the treatment of chronic cerebral diseases, and how rarely untoward results were ever observed, it can scarcely be doubted that fashion has tended to deprive us of a most valuable remedial agent. In certain cases, where the diseased structure is so deeply situated that it is impossible to pass a seton through it, Maisonneuve's caustic darts* may be employed with advantage.

All external methods of treatment, such as counter-irritation, or applications of tincture of iodine or iodine ointments, as well as the internal administration of drugs, have proved quite unavailing in my hands in the treatment of fibrous goitre. And here I may observe, that during the latter part of last year I tried on a considerable scale the treatment of Professor Lücke, of Berne—namely, the injection of a solution of iodine into the substance of the gland. The result appeared to me decidedly unsatisfactory. In some cases little or no effect was produced, whilst in others the fibroid hypertrophy seemed to shrink into a denser, though more circumscribed, tumour, so that in fact the goitre became fibro-nodular in character. In this way, though some diminution took place in the size of the tumour, there still remained considerable deformity. Of the 101 cases of the fibrous variety that have come under my notice, in 31 the tumour was so small, and the inconvenience so slight, that treatment was not recommended; and 9 patients, who were told that the disease was not dangerous to life, declined treatment. Of the remaining 61, in 42 the result was completely successful, in 11 there was considerable improvement, in 5 no change took place, and 3 patients ceased to attend before the result could be ascertained.

CASE 1. *Fibrous goitre of eighteen months' duration; suffocative symptoms for thirteen weeks; treatment by seton; cure.*—Emma G——, aged fourteen, was sent by Dr. Tatham to me at the Hospital for Diseases of the Throat, April 5th, 1869, on account of considerable dyspnœa, which occurred in such paroxysms that suffocation had frequently been apprehended. The symptoms had existed only thirteen weeks, but the parents had observed that the neck was larger than that of most children for the last year and a half. There was general fibrous hypertrophy of the thyroid gland, and the neck measured 14½ inches. The swelling was very hard, and the wings of the thyroid body seemed to embrace the trachea (Fig. 1). The child was immediately admitted, and on the next day a seton was passed through the tumour. The dyspnœa was relieved in a day or two, but as the swelling showed little disposition to diminish, the seton was not removed for three months. On May 19th,

FIG. 1.



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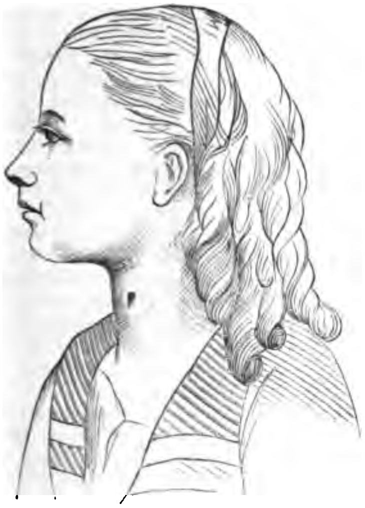
* These darts consist of one part of chloride of zinc with one or two of wheat flour.

the girl was made an out-patient, having greatly improved. On July 12th the size of the neck measured two inches less than on admission (Fig. 2), and all symptoms of dyspnœa and suffocation had disappeared. The child had gained flesh, and was in every respect quite well. She was therefore discharged "cured."

FIG. 2.



CASE 2. *Suffocative fibrous goitre; six months' duration; treatment by seton; cure.*—I was requested by Dr. Jackson, of Highbury, to see Master H—— on January 11th, 1871. The patient, aged eleven, was suffering from an enlargement of the neck, causing slight dyspnœa, and dysphagia, especially when taking liquids. These symptoms had only existed since the previous summer. The neck measured 14¼ in., and the tumour appeared to pass deeply into the tissues of the neck on each side of the windpipe. It may be incidently observed that the family history of this case shows a very remarkable predisposition to the disease. Neither the father nor mother had suffered from any enlargement of the thyroid gland, but a female relative living near Tunbridge had a large goitre. An elder brother had died just a year previously at the age of nineteen from constriction of the trachea caused by a goitre. Tracheotomy had been attempted by an eminent surgeon, but it had been impossible to reach the trachea on account of the intimate connexion of that canal with the diseased gland. Another brother of my young patient, aged twenty-three at the time of my first visit, was the subject of a simple goitre, the circumference of his neck being 16½ in. A sister aged eight years and a half had for two years suffered from soft bronchocele, which was disappearing under the use of iodine, employed externally and internally. To return to my own patient: the tumour was diagnosed to be fibrous in structure, and I advised that it should be treated by seton. Dr. Jackson concurring with me, a seton was introduced January 13th. Suppuration rapidly took place. On the 17th a small abscess which had formed in the centre of the neck was opened. On the 20th, one week after its introduction, the seton was removed, and in one month the goitre was entirely cured. It may be mentioned that even the day after the seton was inserted both the dysphagia and dyspnœa were almost entirely relieved. I saw this young gentleman on January 11th of the present year. He is perfectly well, and the neck measures only 12½ in.

Cystic bronchocele varies greatly in size. It is sometimes quite small, but I have seen tumours large enough to contain a pint of fluid. Sometimes the tumour is so large as to hang down in front of the chest; and, more rarely still, it passes down between the trachea and sternum. Such a case I have at present under treatment, in conjunction with Dr. Elliott of Dulwich. Cystic tumours are invariably of globular or ovoid form. The isthmus is the most frequent seat of these formations, but any portion of the gland may be attacked. The cysts are sometimes very movable, while at others they are perfectly fixed. Sometimes the sense of fluctuation is very apparent; sometimes it is barely perceptible; whilst not unfrequently it cannot be distinguished at all. This varying degree of fluctuation depends principally on the density of the walls (which may vary in thickness from two or three lines to an inch), and in a lesser degree on the density of the fluid. I have never found these cysts diaphanous, even when examined with a strong oxyhydrogen light, a circumstance which is readily explained by the nature of the contents of the cyst. As development of the growths generally takes place towards the surface, there is seldom any dysphagia or dyspnœa; but cases occasionally occur in which, the cyst being situated on one side of the neck, the difficulty of breathing is extreme. This variety of the goitre not unfrequently causes displacement of the air-passages. The tendency of cystic tumours is to continue to develop, but sometimes they are obliterated by the deposition of layers of fibrine on the inner surface of the cysts, or by the deposit of calcareous matter in their walls. It is stated by many authors that new formations grow from the internal walls of the cyst, and in this way more or less fill up the cavity. It is probable, however, as Virchow has pointed out, that these apparent new formations are, in fact, remains of the original normal structure which has been accidentally encapsulated in the cyst. The morbid specimen which I now show well illustrates this condition. There is general degeneration of structure of the right lobe; the vesicles are enlarged, and a few contain colloid matter; others are empty. The normal corpuscles are almost absent; vessels of areolar structure (hyaline) are seen to be undergoing fatty degeneration and breaking down. Scattered throughout the mass are large masses of cholesterine, compound granular corpuscles, and altered blood. The left lobe is also diseased, and the cysts apparently dilated.

Cystic goitre, although frequently causing great inconvenience from its weight, seldom endangers life. In the first illustrative case which I shall relate there was complete orthopnœa for several nights previous to the operation.

The pathology of these tumours is involved in some obscurity, but the essentially vesicular structure of the gland-tissue points to the probable development of the cysts from the original vesicles. These vesicles or closed follicles enlarge, the walls of adjacent follicles become broken down, minute hæmorrhagic extravasations take place, and so the cyst grows. The contents of the cyst consist of the metamorphosed elements of the original vesicles, together with the altered products of blood-extravasation. According to Beck and Ecker, cysts sometimes originate from simple extravasation of blood and from the subsequent changes which take place. Virchow, on the other hand, considers that very commonly cystic goitres are due to the breaking down and liquefaction of a previous colloid growth.

The treatment of these cases which I consider best combines safety with success is the conversion of the cyst into a chronic abscess. It must be borne in mind that the great preponderance of these tumours are blood-cysts, and even in the case of those cysts which contain a simple serous or albuminous fluid, the withdrawal of the contents is almost invariably followed by rupture of the capillaries in the gland-structure so commonly contained in some part of the cyst. In carrying out the treatment it is therefore most important that the disposition to hæmorrhage should be checked, and it is fortunate that the treatment which most effectually arrests the hæmorrhage also most rapidly produces suppuration.

The method of procedure is as follows:—First, empty the cyst. When practicable, it is well to make the puncture as near as possible to the median line, and to select the most dependent portion of the tumour for introduction of the instrument. As soon as the trocar is felt to pierce the cyst wall it should be withdrawn, and the canula passed further in by means of a blunt-pointed key. The fluid having been withdrawn through the canula, a solution of perchloride of iron (two drachms of the salt to an ounce of water) is injected through the canula by means of a syringe. The plug is re-inserted, and the canula secured in position by a strip of plaster. The injection of iron is repeated at intervals of two or three days until suppuration is established. When this point is reached the tube is withdrawn, poultices are applied, and the case treated as a chronic abscess. Where the tumour consists of more than one cyst it may be necessary to make a second or third puncture; but it frequently happens that other cysts can be opened through the cyst originally punctured, and in this way some scars may be avoided.

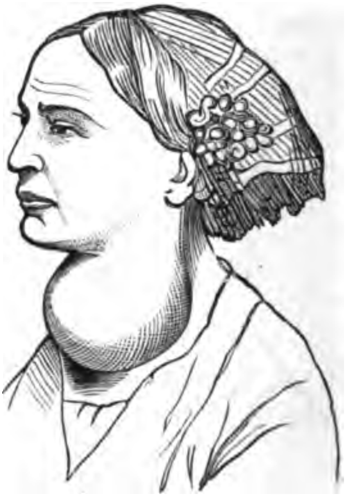
In the class of goitre now under consideration a great variety of treatment has at different times been employed. Simple puncture is one of the oldest methods; but under this plan death has occasionally taken place, two fatal cases having been collected by Gurlt. Setons also have been used, but with only indifferent success. The method of establishing suppuration by means of canulæ and pledgets of lint has also been employed, but such treatment is most tedious. The injection of iodine has been successful in many cases. In two cases treated by Langenbeck the cure was effected in twelve days. In one case under the same surgeon, however, death took place in three weeks, and several fatal cases have occurred in this country. Simple free incision has been largely employed; but in a case under the care of Porta death took place in fifty-five hours, and in a case in the practice of Michaux death took place in nine days. A cure has also been effected by excision of a portion of the sac; but this treatment has not been without fatal results, one death having occurred in the practice of Hecker in four days; another, under Dr. Fleury, in five days, with purulent cellulitis of the neck; in one other case, which occurred to Schwörer, the patient "died cured" in seventy-two days with typhous symptoms. Extirpation of the cyst, which is evidently only suitable for small and movable tumours, has been performed on many occasions, but one death occurred shortly after this operation, to Dieffenbach. The application of powerful escharotics has also been employed, and by this means cases have been cured. One fatal case is, however, recorded by Bonnet, in which death took place after an almost complete cure, repeated hæmorrhages having occurred. One successful case treated by subcutaneous discision under Dr. Bühring has been reported by Gurlt; and the same author has found two cases treated by ligature, in one of which death took place after three days.*

Of my 39 cases of cystic goitre, 38 underwent treatment. All of these were completely cured. In three cases there was hectic with diarrhœa and temporary emaciation, but these patients completely recovered. After puncture and injection the temperature seldom rises about 101° F. In one case (No. 10) the temperature began to rise on the evening of the third day, when it was 98·8°, till the fifth and sixth day, during which it was 103°. It then gradually fell. In the case of a patient now under treatment in the hospital, in which one cyst communicates with another, the opening in the inner cyst gets occasionally occluded, so that the pus is retained, and this circumstance is always attended with a rise of temperature from 98° or 98·2° to 101°.

One patient, who was not known to be pregnant at the time of operation, aborted, and was ill in consequence. She made a good recovery, and the goitre was quite cured.

CASE 3. *Cystic goitre of five years duration; treatment as before; cure.*—Esther J——, aged fifty-three, was first seen at the Hospital for Diseases of the Throat on Feb. 26th, 1869. She applied on account of an enormous goitre, which had existed for five years. It had not till lately been more than an inconvenience, but during the last three months it had rapidly increased, and she had

FIG. 3.



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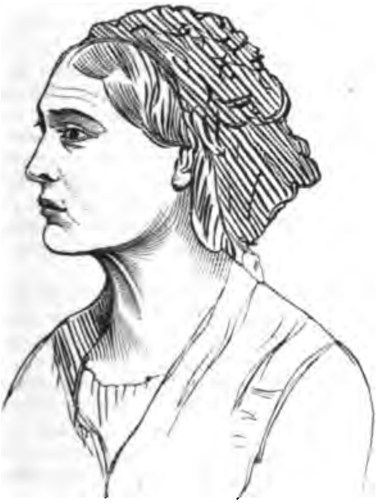
* Those who are interested in following up these various modes of treatment are referred to Gurlt's most able and interesting work "Ueber die Cystengeschwulste des Halses."

become much weaker in her general health. She, however, declined operative treatment until Jan. 13th, 1870, when she applied again on account of continued increase in the size of the tumour. She was then admitted as an in-patient, and her portrait (Fig. 3) was taken. The cyst was not punctured and injected till Feb. 1st, and, on account of the great size of the tumour, treatment was pursued gradually. Pus did not form for more than three weeks, at which time the patient suffered for a few days from hectic and diarrhœa accompanied by a slight rise of temperature. The tube was removed on the 15th of March, forty-three days after its first introduction, and Fig. 4, taken March 30th, well illustrates the complete success of the treatment.

FIG. 4.



CASE 4. *Cystic goitre, treated by tapping and injection; cure.*—Charles W——, aged forty, was admitted into the Hospital for Diseases of the Throat June 26th, 1871, suffering from a cyst of the right lobe of the thyroid gland (Fig. 5). The cyst was punctured June 29th, and iron injected. Pus quickly formed, the canula was removed in three weeks, and the patient was discharged July 27th, the portrait (Fig. 6) having previously been taken.

FIG. 5.

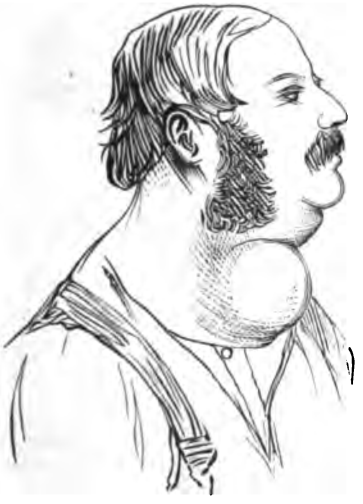
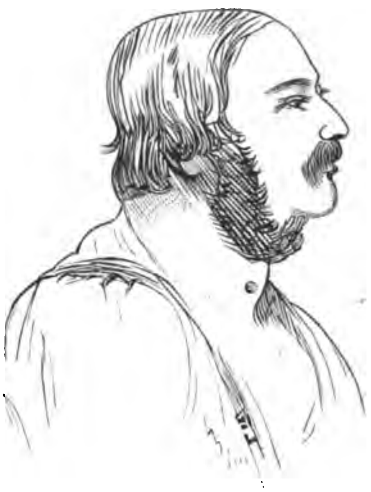


FIG. 6.



Fibro-cystic goitre is, after all, only a combination of the fibrous and cystic forms of the disease, the difference in character being dependent on the relative proportion of these two varieties of goitre.

It is only necessary to remark that the cysts are often multiple, and the fibrous tissue is very frequently developed behind the cyst; so that if a very careful examination be not made, the practitioner may think that he has only a cystic goitre to deal with, and when he has tapped the cyst he may discover the solid structure.

The treatment consists in a judicious combination of the methods recommended for the treatment of fibrous and cystic bronchocele; but it is advisable to treat the cysts first. I have notes of nine cases of fibro-cystic goitre. In seven of these treatment has been adopted; four were completely cured, in two there was considerable benefit, and one discontinued attendance.

CASE 5. *Fibro-cystic goitre; twenty years' duration; treatment by tapping, drainage-tubes, &c.; very great benefit.*—Elizabeth M——, aged fifty, was admitted into the Hospital for Diseases of the Throat on September 28th, 1870, on account of a very large swelling in the neck, which had first commenced twenty years previously. On examination, a large multilocular fibro-cystic tumour was seen to project from the front part of the neck (Fig. 7). The tumour, though lobulated, was of pyramidal shape, the apex being directed forwards and slightly downwards. The neck measured 20½ inches in its widest circumference, and the most projecting point of the tumour was 4 inches in front of the hyoid bone. On Oct. 4th the two principal cysts, one in the right side and one in the centre, were tapped and injected with iron. On Nov. 6th, the tumour having become considerably reduced by the obliteration of the cysts, a drainage-tube was introduced from left to right through the entire mass. Considerable suppuration followed, and the tube was removed on Nov. 30th. Great hardening still remaining on the right side, two caustic darts were introduced on Dec. 14th. This was followed by fresh and abundant suppuration, and a further reduction in the size of the tumour. On January 23rd, 1871, she was discharged, greatly benefited, her throat measuring only 16 inches—i. e., 4½ inches less than when admitted. As she lived at a distance from London, the portrait (Fig. 8) was taken before she left; but on visiting the hospital three months later, the appearance was considerably better than is shown in the picture, the neck being reduced a further inch in circumference, and the scars being much smaller.

Fig. 7.

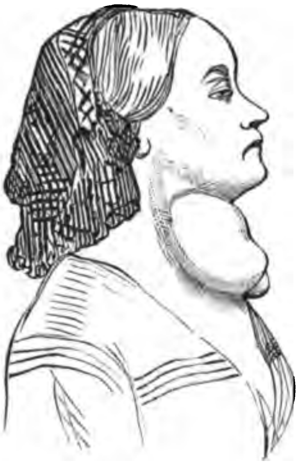
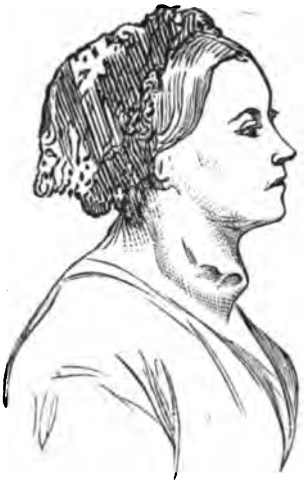


Fig. 8.



Fibro-nodular bronchocele is a subdivision of fibrous goitre, in which fibrous tissue, developed in scattered nodules, varying in size from that of a pea to an orange but generally not larger than a walnut, has undergone further change of a cirrhotic nature. These nodules are always of the densest consistence, and frequently they undergo calcareous transformation. Six cases have come under my notice, two having been sent to me by medical practitioners on the supposition that the tumours were scirrhous. The disease is probably a retrograde metamorphosis of a previous pathological condition; and although it seldom undergoes any improvement, it also rarely gives rise to any serious inconvenience. This is the more fortunate as treatment has seldom any beneficial effect.

Colloid bronchocele.—Though more or less colloid change is exceedingly common in the thyroid body, even in cases where the gland is apparently of normal size and to the naked eye perfectly healthy, colloid goitre is comparatively rare. It is remarkable on account of the large size it attains, and, as a rule, for the manner in which it effects coalescence of the different lobes, so as to form one solid tumour. It seldom causes pressure on the windpipe or gullet, and is only inconvenient on account of its very large dimensions. This form of disease is known to depend on a metamorphosis of the albuminoid constituents of the cells into the jelly-like material called "colloid"; and the large quantity of albumen contained in the cells of the thyroid body accounts for the relative frequency of colloid degeneration in this organ as compared with other parts of the body. I have no doubt that in many of the fibrous and fibro-cystic cases which I have treated there has been more or less colloid change; but I have only noted two cases as belonging strictly to that variety. The first of these cases was treated by electrolysis. This was in the year 1868. The battery employed was not at all equal to those now used, and the result was almost negative. The second case was that of a young child; and in this case a seton was inserted, but it did not produce any reaction. With my increased experience, I think that both these cases might have been successfully treated by irritating setons or caustic darts.

No case of *vascular bronchocele* having presented itself in the 211 cases on which the present paper is based, this variety will not be here considered.

I have reserved till the end consideration of the advisability of surgical interference in cases of goitre. Some eminent surgeons are of opinion that no treatment should be adopted unless there be danger to life, or at least serious disturbance of function; and even in such cases only recommend palliative measures, such as the simple puncture of cysts, or the attempt, often unsuccessful, of tracheotomy. Billroth says that operations should not be performed for "cosmetic" effect; but seeing that so many operations are daily performed with a view of removing defects of personal appearance far less objectionable than that of bronchocele, Billroth's dogma can scarcely meet with general acceptance, especially when it is further remembered that at any time the disease may become dangerous to life.

The rule which I have adopted is, *not to recommend* patients to undergo treatment unless there is serious embarrassment of function, or continuous increase of the tumour; but as none of the various methods of treatment employed by me have hitherto been attended with fatal results in my practice, I have always been willing to carry out treatment when the patient has desired it on account of disfigurement.

If I have not referred to various other measures, such as ligature of the thyroid arteries, division of the sterno-cleido-mastoid muscle, and removal of the entire thyroid gland,* it is because I have not yet met with a case in which I have deemed such measures necessary.

Weymouth-street, W., April, 1872.

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NOTES

ON THE

GENERAL PRINCIPLES OF CUTANEOUS THERAPEUTICS.

By Tilbury Fox, M.D., F.R.C.P.,

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The progress which dermatologists are making towards the foundation of a more rational and at the same time more successful system of cutaneous therapeutics is to my mind, on the whole, satisfactory. This progress is, no doubt, in great degree a result of pathological revelation, which has led to a decreasing faith on the part of practitioners in arsenic as *the* remedy for every cutaneous trouble; whilst experimental observation shows that the general principles upon which skin diseases should be based are, in the main, indentical with those which govern the management of all other diseases. No doubt, in points of detail, there is much that is special in the treatment of diseases of the skin. Yet it cannot be denied that there is still too much clinging on the part of the physician to the use of arsenic as the panacea for all skin affections, and that this ingrained belief in the superlative efficacy of the drug has directly led to a general ignoring of the value of other remedies, and thus to a retardation of all experimental inquiry into their relative properties. Notwithstanding this drawback, not only has the number of incurable skin diseases been much diminished of late years, but the rapidity of cure in the case of individual diseases has most certainly increased. Skin diseases are daily being more and more shorn of that intractableness which at one time was their chief characteristic; and I do not hesitate to say that, when pathology shall have made more progress, and the principles of general therapeutics have been adopted in regard to diseases of the skin, they will become amongst the most manageable of all diseases.

But in order that this condition of things may be reached it will be requisite that the treatment of diseases of the skin shall be regarded as much from the physician's as the surgeon's point of view. This is not the case at present. To my mind the connexion between functional or organic disturbance of important internal organs—e.g., the stomach, the liver, the kidney—as well as of the nervous system and the sympathetic, is exceedingly close; but it is practically disregarded, and I think in this direction we specially need to push inquiries.

I now propose to give a general sketch of the treatment of the different kinds of diseases of the skin, and the reasons which should induce us to employ various remedies in particular groups or kinds of cutaneous mischief, and in relation to particular circumstances. I think this sketch may be not a little useful. Ordinarily, in dealing with the general treatment of skin diseases, it is usual to name particular remedies in detail, and to state their relative virtues; but in so doing observers lose sight of the principles upon which they are employed. Now for practical purposes, as regards therapeutics, I think diseases of the skin may be conveniently grouped under three heads:—

1. Those which are purely local.
2. Those which, though mainly local in their origin, are yet *influenced* or modified by different conditions of the general nutrition, or, if the expression be preferred, by constitutional conditions. These diseases, in fact, require mainly local remedies, but demand the use of such as are *general* as *auxiliaries* to cure.
3. Those which arise primarily or directly from disturbance of the general nutrition or system. Here general are the most important, *local* measures being employed as secondary *aids* to cure.

Now in the first category may be placed warts, the simpler erythemata, and inflammations excited by cold, irritants of all kinds—e.g., erythema, simple eczema, herpes, cancer, keloid, fibroma, nævus, atrophia cutis, scleroderma, ichthyosis, and certain parasitic diseases.

In the second category may be placed the simpler forms of inflammation in badly nourished or debilitated subjects: eczema in its severer forms, lichen, impetigo, ecthyma in some of its forms, pemphigus, acne, psoriasis probably.

Under the third head fall the eruptions of the acute specific diseases: urticaria ab ingestis; medicinal rashes; all diathetic diseases—e.g., strumous disease, syphilis, leprosy; cachexias of special kinds—e.g., lead poisoning, malarial poisoning, &c.; chromatogenous (pigmentary) diseases; neurotic diseases—e.g., prurigo; and diseases connected specially with disorder of the sympathetic nervous system.

Before entering into further detail, it will be as well to remark here, that in order that treatment should be properly conducted in any case of disease of the skin, the natural history and course of the disease presented for medication should be known and regarded. The object of the physician should be to cut short the natural course of the disease by all means if he can without ill effect, but, if not, to aid in conducting it through its natural course, if that be towards resolution. If the disease be a simple erythema induced by cold or the like, non interference, rest, or protection of the part, may be all that is required. ***What not to do*** is quite as important as what to do in diseases of the skin, and both are best learnt in their individual appropriateness to particular cases by a knowledge of the natural history of cutaneous diseases—a point mostly neglected. The means adapted to directly check the progress of disease are chiefly local. The diseases whose course we may ***cut short*** by these direct local means are particularly—(1) local diseases induced by causes acting from without upon the skin—e.g., simple eczema and traumatic erythemata; (2) certain maladies in which it is only necessary that the destruction of new tissue may be accomplished—e.g., warts, lupus, nævi, cancer, rodent ulcer; or (3) disease the cause of which is local and can be rendered inoperative by local means, as in parasitic diseases.

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* This operation has lately been performed with great brilliancy and success by Professor Greene, of New York, and those who are interested in the subject are referred to his very complete article in the "American Journal of Medical Sciences" for January, 1871.

In the majority of cases of diseases of the skin, the cause is acting from within outwards, and we have less chance of rendering that cause inactive by the direct use of appropriate medicaments. It is only by altering the whole character of the nutrition that we can effect a cure. In some cases, as in urticaria ab ingestis, a sharply-acting emetic relieves the patient of his trouble at once—i.e., cuts short his disease; whilst local measures are adopted not with the view of cutting short, but rather (*a*) in the first place of *moderating* changes in the skin—e.g., hyperæmia, pus-formation, squamation, and the like; then (*b*) *protecting* the diseased part; and (*c*) subsequently of rousing the circulation and absorbents to a healthy action.

It will therefore be readily understood that a knowledge of the natural histories of skin diseases is most necessary to a correct handling of drugs, both internal and external, as against these diseases. My own belief is, that the early stimulation of most skin diseases does an infinity of harm, and I think this erroneous use of remedies arises from the want of a correct knowledge of the natural course of skin eruptions. We know in the case of pneumonia or pericarditis the course the disease should run towards resolution and cure, and the physician is able to give his medicines not only with a firm faith in their doing no harm, but with every prospect of their aiding nature. We do not act with the same certainly in cutaneous medicine, but dermatologists neglect to make themselves acquainted with the natural causes of the diseases they treat.

I will now proceed to some more particular remarks touching the three groups into which I have divided diseases of the skin.

Class I.

In the first place as regards diseases that may be considered local. Here we have (1) local inflammation excited by irritants in persons of good health. All we need do is to recognise the fact that the skin will soon recover from the effect of the irritant, if we do not increase the irritation already set up, and protect the part a while. We frequently do too much in these cases of erythemata. We use our astringents too freely and too early, forgetting that the ordinary stimulant action externally of the air, heat, cold, friction, and the like, upon the hypersensitive, because hyperæmic, surface is vastly increased. The exclusion of the air by powders, and the protection of the part by a layer of cotton-wool, if very simple and apparently a do-nothing affair, is in reality the best treatment; whilst it is beneficial in direct proportion to its speedy and complete adoption. The active hyperæmia having subsided, the vessels may have lost tone, and it is then alone that astringents and stimulants are advisable. This is a principle of treatment of no little importance. Its reasonableness is obvious and experimentally vindicated in practice. (2) Local degenerative changes—e.g., cancer; hypertrophous growths, as keloid. The object we have in view is to destroy by caustics, or to remove by the knife or the ligature; of course, the removal should be complete, free, and speedy. It is scarcely necessary to dilate on these points, which emphatically apply to rodent ulcer and its congener epithelioma. An exception is found in the case of keloid, which seems almost certain to return in the cicatrix. Pendulous tumours call for the ligature, as in molluscum. Whether the knife or caustic should be used, or both conjointly, must be, of course, decided by the circumstances of particular instances; but this depends mainly upon the extent of disease. If, as in rodent ulcer and nævi, the growth be small, caustic will of course suffice, but it should be freely used, and so as to attack some slight portion of the healthy tissue to make sure that *all* the morbid tissue is destroyed. If the growth be extensive, the knife should remove it freely, but by all means should a layer of caustic be subsequently used to secure the death of any stray particles or masses of the foreign growth. But even in the cases of persons in good health, of whom we are speaking, I hold that if the growth, and especially the edges, be very hyperæmic, it is better to attempt by simple astringents and exclusion of air to allay the hyperæmia before attempting to destroy the local disease; because I hold that diseases have a tendency to return—and likewise oftentimes to spread in cases where removal unfortunately happens to be incomplete—in direct proportion to the degree of hyperæmia of the boundary line between the healthy and diseased tissues. There is still a third class of local diseases that call for local treatment mainly—viz., parasitic diseases. The mode of using parasiticides in cases of animal parasitic disease is simple. It is easy to do too much even here, and to apply the remedies to the wrong place. Sulphur in scabies is frequently used too freely, and for too long a time, so that long after all acari are killed the parasiticide is applied to increase the accompanying follicular irritation, and to set up urticaria, eczema, and the like. The effect of remedies should be carefully watched after a few applications. Three days' use of *weak* sulphur ointment, or a single sulphur-bath, if followed by cessation of itching, has probably in most cases practically cured the disease, which only needs the application of the parasiticide to a spot here and there, where ova hatch out, to rid the patient of his malady. In phtheiriasis the lice and their ova are in the clothes, which require to be baked; not in the body, which only needs a good washing or two, and not parasiticide treatment, which only keeps the lice from attacking it, and does not destroy them. With regard to vegetable parasitic disease, something special must be said, and the difficulties of treatment are great oftentimes. There are innumerable parasiticides for the destruction of vegetable fungi. We are scarcely now concerned with these individually, but rather the mode and reasons of their use. Wherever hair is present in any amount, as in the hairy scalp, the fungus finds its way deeply into the follicle, and a difficulty is experienced in bringing parasiticides into contact with its elements; but on the non-hairy parts of the skin there is little trouble in the matter. It will be apparent, therefore, that in the hairy parts early treatment by local measures is of the first importance: the difficulty is one of access to the fungus deeply imbedded in the follicles. If water solutions are prescribed, it is always best to have the greasy matter of the skin removed with soap and water before applying these lotions, because many skins, being very greasy, repel the watery lotions more or less effectually. If unguents are used, they should not be merely smeared on, but well rubbed in so that they may reach the parasites. When it is thought desirable to blister, it should be remembered that the effect may not be felt at the deepest part of the hair follicle in hairy parts, where the most active fungous growth may exist. Rapid and extensive serous effusion may take place in the more superficial layers, and shut off all access, as it were, from the deepest part of the follicles. I am by no means in favor of blistering in chronic cases, though I think it of great service where the disease is pretty recent—that is, where the fungus has not invaded the parts very deeply and extensively, and the depth of tissue blistered will include the area invaded by the fungus. In old cases I prefer the continuous contact of the parasiticide in a form that does not vesicate. There is one more point to mention. It is the desirability of getting away bodily all diseased hairs, which are loaded with fungus elements. This is done by keeping the hair cut close, by shaving, and by epilation, or extraction of the hairs with forceps. As a general rule the first procedure is adopted; the second makes the head sore and too sensitive to remedies. The latter takes a great deal of time to carry out, and generally the hairs, as in the common ringworm of the head, are so brittle that they break away and the follicular portion is left behind in the follicle. Keeping the hair cut very close is the most available thing for general adoption. When, it may be asked, do we cease to apply our parasiticides? or rather, how do we know that all the fungus has been destroyed? By microscopic examination, is the answer, as we shall see in treating specially of parasitic diseases. I will only say here that where the root-sheaths are re-forming properly, and there are no fungus elements to be detected, we may conclude the remedies have had due effect.

Class II.

So much for the general principles of treatment in purely local diseases. I now come to speak of what I have termed the mixed class of cutaneous affections, the components of which are very numerous—i.e., those which are essentially local, but are influenced by the state of the system at large. These demand, for their relief, a judicious combination of general and local treatment. In many instances, a disease excited by a local cause—i. e., intertrigo, eczema simplex—occurs in a badly nourished subject; and the mal-nutrition of, or mal-elimination in, the patient prevents the progress towards cure, which should, if regard be had to the local mischief itself, speedily happen. There are one or two special points in the therapeutics of these cases. I may say generally that in these diseases the proper treatment is that which we adopt for pyrexial diseases in other parts of the body in the early stages, and is subsequently such as is of a tonic nature, and finally that which is calculated to remove chronic inflammatory thickening or the like. But there is something more than this to be considered. It is not enough to adopt a treatment recognised as suited to a particular disease. There are a number of influences that modify disease in different subjects, and that require to be carefully attended to. These influences are of very common operation, and arise out of the peculiarities of the patient. The dermatologist must determine not only what is best for individual cases, and how far the specific or formulated treatment for particular diseases should be added to, or modified, to suit these diseases in particular individuals. In fact, the treatment of the cases of skin diseases now under notice should be a combination of the remedies suited specially to the particular disease present, with those suited to the concomitants of the individual case. The following are the modifying influences to which attention should be given.

Firstly. It is important to recognise the great influence which scratching possesses in exaggerating certain diseases. Nothing is of more consequence than the use of special remedies calculated successfully to allay the irritation which forces the patient to scratch, and to protect and to soothe the parts when scratched.

Secondly. The influence of general debility is at work not only to retard recovery in, but to favor the development of many diseases to a more severe degree than would otherwise be the case. It is not a question of debility *producing* this or that malady, but *modifying* the same. The debility may express itself mainly in the form of anæmia, want of nerve power, &c.; and special and appropriate remedies given with a view to meet these several states will lead to an improvement in the general health, and thus indirectly favor the cure of any diseased condition about the skin for it does not need any proof from me that the nearer a man approaches in his condition the standard of health, the sooner and more certainly will he throw off any disease about him, be it in his outer or inner surface or parts. But in practice we must forget that the specific should be linked with general treatment. And the relief of the debility of persons afflicted with diseases of the skin is to be accomplished not only by the use of medicines, but by mental and bodily rest, change of air and scene, and the like. These matters are by no means sufficiently considered.

Thirdly. Every attention must be paid to dyspepsia as effecting the origin and course of skin diseases. It gives rise, of course, indirectly to debility, to torpid action of the liver, to the generation of acridities that float about in the blood, and circulate freely through the skin to disorder it; and, lastly, it increases cutaneous hyperæmia by reflex action. Dyspepsia must therefore be regarded as a powerful intensifier of all hyperæmic skin diseases.

Fourthly. The circulation of retained excreta is always a cause of intensification of hyperæmic conditions. The blood charged by excreta in passing through an eczema or a psoriasis, will irritate it, if I may so say, and thus necessarily tend to give it an inflammatory character, whilst the progress towards cure will be retarded. I believe this to be one of the most important of all points to be attended to in treating diseases of the skin, and the principle is of almost universal application. It is a matter of common sense (but matters of common sense are apt to be very much neglected) that a poisoned blood-current—poisoned, i. e. "charged" with bile or nitrogenous matter which we know must disorder the tissue nutrition and vascular tension—will not permit the cure to be accomplished so easily as if the blood were uncharged with the same elements; and the removal of the latter will help the cure of disease, and that oftentimes in a marvellous manner. Now, whence the excreta to charge the blood? From (*a*) defective assimilation, as in dyspeptics, torpid liver action, and from excessive waste; or (*b*) from defective excretion by the skin, the kidneys, and the liver. The skin has to get rid of a certain amount of effete products; and if the skin acts sluggishly or scarcely at all, this may impurify to some degree the blood-current. So, again, deficient kidney action tells its own tale. The non-excretion of bile products in the usual way, and their passage into the blood-current to circulate within it, is another cause of skin mischief, as in some cases of erythema, urticaria, and probably purpura. The severest cases of general eczema I ever saw was excited by the circulation of bile through the skin in connexion with a most marked attack of jaundice. Of course in such a case the influence of the retention of the non-eliminated products is readily appreciated, but in other cases readily overlooked. There are minor degrees of blood-impurification by bile, urea, and the like. It seems to me that the treatment of diseases of the skin is essentially the physician's work and not the surgeon's for the very reason of the connexion between skin maladies and functional and structural changes in internal organs.

Fifthly. Deficient renal action, particularly in reference to the quantity of fluid discharged, is another matter of prime moment in regard to skin diseases. I am convinced that we do not, when the skin is disordered sufficiently make use of the kidneys to relieve the skin of its work. There are three conditions in connexion with diseases of the skin in which the kidneys should be freely stimulated to active excretion of fluid: (1) Where, in consequence of organic disease of the heart or other cause, and in connexion with a sluggish circulation, there is a distinct tendency to fluid accumulation in the tissues. (2) In all cases of hyperæmic skin disease, and in the early stages of inflammatory diseases, where the blood tends to accumulate in the skin in connexion with pyrexia. And (3) in diseases of the legs, where there is the slightest tendency to local or general œdema. In inflammatory diseases of the leg diuretics greatly counteract the injurious effects of mere gravitation, and they tend to relieve the engorged tissues. Diuretics are useful under all these circumstances, and emphatically so where the quantity of urine is already deficient. But I hold that in diseases of the skin generally a deficiency of urine calls for free stimulation of the kidneys to relieve the disordered skin of its work as much as possible. Of course where there is free circulation with the blood of retained excreta the necessity for free kidney action is increased tenfold. The liberal administration of diuretics in a case of eczema of the legs in a gouty subject will often act like magic when all other remedies fail.

Sixthly. Gout and rheumatism have their special influence upon skin diseases. I might have referred to these modifying influences under the last head, for it is the excess of uric acid in the one and the presence of lactic or an allied acid in the other case, and the circulation of blood charged with these products through the skin, that cause special mischief in skin diseases, *and give an inflammatory or irritable aspect to them.* The older practitioners, in attributing to gout and rheumatism the causation of disease which we now deem independent of them, were even nearer the truth than are some modern observers who hold excessive belief in the local origin of disease. If gout does not *produce*, it often *modifies* skin mischief, as above stated; and, after all, our forefathers were only at fault in regard to the use of terms. It is not an uncommon thing to have this or that eczema or psoriasis or lichen called gouty eczema, gouty psoriasis, gouty lichen. Practitioners have found that by treating patients effected by these forms of disease for gout or rheumatism, as the case may be, the maladies have often speedily vanished when other measures have failed. They have imagined that the gouty blood has caused the disease. It would be more correct to say it modified, it irritated, the disease, and so prevented the cure from taking place. With this explanation there can be no difficulty in seeing that gouty and rheumatic dispositions exaggerate hyperæmic conditions in skin diseases, and give an inflammatory aspect to them very frequently. The removal of gouty influences must aid the cure of skin diseases.

Seventhly. The tuberculous and the strumous diatheses also, it must be remembered, considerably modify certain skin diseases. The tendency in the phthisical and phthisically inclined and in the strumous is to a particular perversion of cell-growth in certain of the tissues, and notably the fibro-cellular: I refer to the tendency to the production of pus. Now it will be readily understood that this must have much influence upon local tissue-change in skin disease. Given the occurrence of a local inflammation in a non-strumous and a strumous subject, there will be difference in the two instances accounted for by the operation of the pus-producing tendency in the one and not in the other. It must be so. The tendency to the morbid cell growth, which may not have shown itself in action before, will be likely to do so when nutrition is disturbed by local inflammation, disordered innervation, &c., as the case may be. The strumous tendency takes occasion by the perversion of local nutrition to give evidence of its presence and to complicate matters. Of this fact experience assures us. Take the case of an eczema: if it occurs in a strumous subject, it is accompanied by free pus-production. Acne in a non-strumous and acne in a strumous subject are different. In the latter case, the fibro-cellular textures of the wall and about the gland are implicated and inflamed, often presenting the appearance of the livid-red and indolent tubercular swelling of a strumous tubercle, whilst the acne spots suppurate freely and leave behind scars, indicative of the tendency to ulceration, which is another feature of the strumous habit. This is a simple but it is an important point in therapeutics, and, because simple, often disregarded as of no moment. In psoriasis, in strumous subjects, the tendency to the production of pus is seen, and in this statement I am confirmed in my original opinion by my friend Dr. R. W. Taylor of New York, an excellent observer and worker. Therefore in regard to phthisical and strumous subjects, whilst one treats the local disease, be it eczema, erythema, lupus, acne, or psoriasis, it is of great use to neutralise, by the conjuntion of anti-strumous remedies, the bias of the general nutrition towards free pus-production, which will of course express itself in proportion as the diathesis is marked and the local derangement of nutrition severe.

Eightly. An old syphilitic taint is to be carefully detected and dealt with in reference to skin diseases. This should be recognised as a distinct and special cause of chronicity in certain non-syphilitic eruptions. The tendency of fully-developed syphilis in the human subject is to produce a new growth in the fibrous textures, or to give rise to a modification of the normal cell elements of these parts. The altered or new tissue we call "granulation tissue." Now, when syphilis modifies ordinary skin eruptions, it does so by impressing upon them more or less of the characteristic tissue change it ordinarily produces, and in doing so may antagonise to some extent diseased processes peculiar to the disease which it modifies. Eczema may illustrate what I mean. The characteristic of eczema is the occurrence of serous catarrh in the papillary layer of the skin as the primary condition; but if eczema occur in a syphilised subject, and the syphilis be active, then the tendency of the syphilis towards the formation of the granulation tissue may show itself, and the "serous effusion" may in part give place to the formation of new tissue—i. e., the two processes may be more or less intermingled, or rather, I should say, the result is a compromise. This is exactly what we should be led to expect from an examination of the naked-eye characters of eczema in a syphilitic subject. There is less discharge and more thickening than usual, and than we should be led to anticipate. So in psoriasis there is more thickening, less scaliness, and less vascularity, because the syphilitic tissue invades and presses upon the vessels in the normal tissue. Hence the effect of syphilis on ordinary skin diseases is to render them less typical, in regard to naked-eye characters and to their course, because the additional element of syphilis retards the cure, whilst there is the superaddition of the element of "thickening" or "deposit" (granulation tissue). Whenever I meet with a case of *unaccountable* chronicity I suspect that a syphilitic taint is in the back ground; and, for my own part, I believe that syphilis is a modifier of disease in local inflammations to a greater extent than has yet been expressed in any writings. Syphilis does not give evidence of its presence, as regards skin affections, until some strain is put upon the nutrition of the tissues, and then an explosion takes place. A strain is put upon the nutrition of the skin when a local inflammation occurs, its normal resistant power is lessened, and thereby syphilis, taking occasion by the weakness, shows itself in action as a modifier of the inflammatory process as described. A very familiar illustration of the fact that syphilitic complications or special development of latent syphilis in the system show themselves under such circumstances, is afforded by the outbreak of tubercular syphilis around a wound—about the shin, for example. What would be a simple wound in a healthy subject takes on unhealthy ulceration or syphilitic pustules, or tubercles develop about the seat of injury in subjects in whom syphilis is latent, but which had given no sign of its existence to the physician before. In like manner skin affections seem to excite latent syphilis at times into activity, and are thereby modified by it in the way I have indicated. I know of no more important point than this in cutaneous therapeutics. If we treat the syphilic taint we shall much more speedily and certainly cure the disease which is present.

"Chronic inflammatory thickening" is a matter upon which a few words must be said. In some cases this condition is found to be developed out of proportion to the degree of antecedent hyperæmia both as regards its severity and its duration. My own belief is that in such cases it is to be explained by the existence of the strumous diathesis in the individual concerned, which often at once accounts for the infiltration of the fibro-cellular tissue with a low type of lymph, or the formation in it of a new tissue of a less perfect kind. At other times an old syphilitic taint may best explain the occurrence of thickening. At all events a mercurial course sometimes does wonders, as we all know. Of course long-continued hyperæmia in a person free from the two diathetic tendencies named will lead to "chronic inflammatory thickening." I am now particularly referring to cases in which thickening and induration either rapidly show themselves, or in such a manner as to strike us that it is due to some special tendency of the textures to hypertrophy rather than to the hyperæmia itself, which perhaps has been but slightly marked.

Ninthly. There is a class of phenomena connected with reflex action which it is of some moment to pay attention to in treating skin diseases. He who would deal with the matter scientifically must never concentrate his attention solely upon the mere local changes in these diseases, but must especially be on the alert to discover, in disturbance of the nervous system, the frequent reason of the intensification of hyperæmic conditions in the skin. I have seen not infrequently a redness of the skin produced by mental excitement or sudden fright in "nervous subjects"; and there can be very few who have not known an eczema rubrum made immediately very much worse by the same influence. Depression and mental excitement play great parts in aggravating hyperæmic conditions of the skin. That uterine mischief occasionally intensifies acne rosacea, that dyspepsia frequently makes acne much worse, and the like, are facts belonging to the same category.

Tenthly. The hygienic surroundings of patients must always be carefully taken into account in the treatment of diseases of the skin, in reference especially to the class of cases particularly under consideration now. It is in this respect that the treatment of hospital and private patients of the better class differs so much. The confined living-rooms, the breathing of impure air, the defective diet, and the neglect of cleanliness, are influences that aggravate skin diseases in the poor as compared with the rich. It is cleanliness and feeding up, with fresh air, that the poor want, in many cases of disease which would be relieved by purgatives and alkalies and nervine tonics amongst the better classes.

Now the several modifying influences referred to under the foregoing heads may be present in different combinations, and it is the full recognition of this fact that leads to success in the treatment of skin diseases.

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ON THE

RHYTHM OF THE PRESYSTOLIC MURMUR.

By **George W. Balfour**, M.D.,

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I have read with great interest Dr. Barclay's recent communication in The Lancet on the subject of the rhythm of the so-called presystolic murmur, and have to thank him for his kindly criticism of my published cases, which I hope to turn to account on a future occasion; though, indeed, part of that criticism was founded on a misprint, while also I could scarcely expect my views to be understood by any one holding opinions such as those Dr. Barclay has just promulgated.

Apart from the name, which we all acknowledge to be a misnomer, and only use for convenience sake, I believe I am right in stating that Dr. Barclay's objections to the presently most generally accepted rhythm of the presystolic murmur—expressed in the synonym auriculo-systolic—are based upon the following reasons:—First, that from the absence of valves at the entrance of the pulmonary veins into the auricle (I speak of course mainly of the left heart, but my remarks are obviously applicable to both hearts), and from the comparative feeble muscularity of the auricle itself, it is incapable of acting with such force as to produce so loud and rough a murmur as the presystolic always is; and, second, that in regarding this murmur as auriculo-systolic in rhythm because it has been found to run up to the apex beat, to be synchronous with the carotid pulse, and to precede the first sound when that is present, due account has not been taken of the circumstance that following the close of the auricular systole there is a period of ventricular action, consisting mainly of tension, but partly also of active contraction, preceding the apex beat, the latter phenomenon being more truly the conclusion rather than the commencement of the ventricular systole, and that it is to this early and hitherto, he believes, unnoticed period of the ventricular systole, and not to that of the auricular systole, that the rhythm of the murmur termed presystolic actually belongs. I believe this to be a truthful statement of Dr. Barclay's views, but if I am wrong I trust to him to correct me, and in my subsequent statements I shall not criticise his paper as a whole, but shall merely point out certain facts in regard to the physiology of the circulation which not only invalidate these views, but which also seem to render the older and more generally received idea of the auriculo-systolic rhythm of this murmur more probably the correct one.

In the pulmonary circulation, which takes place, as it were, within a closed sac, the contained fluid acts precisely as the water in a Bramah press, producing an equal pressure in every direction. The result of this is that so long as the pulmonary semilunar valves remain sound the left auricle is able to exert its whole force in producing onward propulsion; even in the most unfavorable case possible of auricular regurgitation none of this propulsive power is lost so long as the *point d'appui* of the pulmonary valves remains intact. The absence of valves, therefore, at the auricular inlets of the pulmonary veins in no way invalidates the assumption of any possible increase of the auricular force so long as the pulmonary valves remain sound, and how extremely rarely these are found incompetent scarcely any pathologist is in a position to state; I myself have never seen such a case. This fact therefore completely invalidates Dr. Barclay's first assumption. But I go further, and, while confessing that regurgitation through these venous inlets is, under certain circumstances, a possible occurrence, affording, like mitral regurgitation in aortic disease and tricuspid regurgitation in certain other conditions, a temporary relief which postpones the final result, I deny that the absence of valves necessitates this form of regurgitation in the normal condition. Throughout the whole circulation venous peristalsis plays a most important part, in the pulmonary no less than in the systemic circulation; and as the veins approach their inlets into the auricle their muscular coat becomes denser and more powerful, while the muscular fibres of the auricle are so arranged as to close completely the venous openings as the peristaltic wave passes from the veins to the auricles. Were it otherwise, were the venous openings always patulous, permanent congestion of the lungs would be the normal state of matters, pulmonary hæmorrhage would be the rule and not the exception, and accentuation of the pulmonary second sound, instead of being an early and infallible sign of pulmonary congestion, would be a matter of every-day occurrence. The teachings of anatomy, therefore, confirmed by those of physiology, forbid the assumption of patulence of the auriculo-venous inlets as a normal condition, render it even improbable as a constant concomitant of valvular disease, and teach us that even where it does occur it cannot interfere, except under the rarest conditions, with the fullest exercise of the auricular power.

As to the ventricular movements, it may be said that, as the peristalsis of the auricle commences in the veins, so that of the ventricle takes its origin from the auricle. The vermicular action of the auricle forces the blood into the ventricle to its complete distension; during this process the segments of the mitral valves are gradually floated up to apposition, and then the auricular peristalsis passes instantly into the ventricular systole, the first act of which is the throwing spirally upwards of two columns of blood, which carry the segments of the mitral valve still upwards in a spiral direction till they reach the level of the auriculo-ventricular opening, where the same spiral movement screws them home, as this action has been aptly termed (Bell Pettigrew), and then instantly follows the lifting of the aortic semilunar valves, and the outflow of the blood through the aortic orifice. During the further process of ventricular contraction, by which its cavity is completely emptied, the segments of the mitral valve are gradually untwisted and pulled downwards by the action of the musculi papillares aided by the chordæ tendineæ, the delicate margins of the valves being prevented from falling inwards on the walls of the ventricle by the continuance of the spiral columns of blood ascending from the ventricles, while their opening is aided by the descent of blood from the auricles. At the termination of the ventricular systole the auricle is already distended, and by the retraction of the segments of the valve a dependent spiral cone is formed opening into the ventricle, by which the blood in the auricle finds ready access into the ventricular cavity whenever the cessation of the systole restores the passive receptivity of the ventricle, the termination of this most important act of the circulation thus initiating its commencement *de novo*. Such is the series of actions occurring during the systole of a normal ventricle. It is obvious that in a diseased one our conception of them must be considerably modified by the condition of the valve. In the most common form of valve in mitral stenosis—the diaphragmatic—it is apparent that the ascending spiral columns of blood, which are the earliest result of the ventricular systole, and which must act more or less inwards from the sides of the distended ventricle, being designed to screw home the segments of the valve, can have little effect in promoting regurgitation through a central button-hole slit, and equally little in producing the more or less forcible succussion of the tense diaphragm, forming the somewhat impure first sound synchronous with the apex beat, which is always present unless replaced by a soft blowing murmur, which it not unfrequently is. On the other hand, when the valve is funnel-shaped, these spiral currents acting sideways must aid in producing a more rapid, forcible, and complete closure of the valve; hence the louder and purer character of the first sound always present in such cases, and the almost universal absence of any blowing murmur taking its place.

A due consideration therefore of the physiological mechanism of the closure of the mitral valve, including its modifications in disease, seems to me very greatly to invalidate Dr. Barclay's idea of the dependence of the presystolic murmur on ventricular action in its early stage. And this view appears still more problematical when we reflect that the early, inwardly directed, spiral currents of commencing ventricular action are supposed to produce a murmur so loud and rough as the presystolic always is, while the further and more energetic ventricular action, when, by contraction of the cavity, these spiral currents are brought more directly within the range of the permanently patent opening in the mital valve, and thus rendered more truly regurgitant (upward) in character, should only be accompanied by a murmur always soft and blowing in character. From Dr. Barclay's point of view it is also inconceivable why the loudest and roughest portion of the murmur should be that which is least stable and most variable in character, while the softer and more blowing portion should be, if once recognised, the least liable to change, and the one which is most likely to recur even if both should temporarily disappear.

Further, we know that murmurs are either directly conducted to the surface of the chest at the point of origin or conveyed to another and more distant one by the blood current, and are heard most distinctly at the one or the other point according to its proximity to the surface of the chest, and according also to certain other conditions as to the conduction of sound which in their entirety are as yet unknown. Thus the murmur of aortic constriction is usually heard loudest at the aortic cartilage (convection), but occasionally it is most distinct just over its point of origin, at midsternum opposite the lower edge of the third costal cartilage (conduction), but it is never heard more distinctly below this. The only exception to these well-known modes of conduction of murmurs is to be found in the case of the mitral regurgitant, which is usually best heard over the area of the apex beat, because there the left ventricle impinges most directly upon the walls of the chest, and the sonorous vibrations are more readily conducted through its denser tissue than through the soft, porous lung-tissue which overlies its point of origin. Not unfrequently, however, as Naunyn was the first to observe, the murmur of mitral regurgitation is (from convection) heard most loudly over the auricle above the third rib, usually in the pulmonary region, where the appendix auriculi lies; and, if Dr. Barclay's view of the truly regurgitant character of the presystolic murmur were correct, it would be inconceivable why it should never be heard in this situation, and why it should, when present, be always audible over the left apex, and invariably inaudible above the fourth rib—circumstances which are susceptible of ready explanation on the principles both of convection and conduction, if we accept the auriculo-systolic rhythm of this murmur. Moreover, in many cases of presystolic murmur, the auricle may be readily felt acting with great force; and in these cases the murmur and thrill are both to be heard and felt coincident with the auricular action and preceding the apex beat; while in the soft blowing and truly regurgitant murmur, the murmur and thrill, where present, are both coincident with the apex beat. Further, in certain rare cases, the presystolic murmur is heard separated by a short yet perfectly appreciable interval of silence from the first sound or soft blowing murmur, if present—a fact which is wholly inexplicable if we suppose both to depend upon ventriculo-systolic action.

In regard to the phenomena observed in the case which Dr. Barclay has evidently so accurately narrated, I too have occasionally heard a similar double murmur occupying the long pause—i.e., the period between the second and first sounds. Sometimes both murmurs were audible at the apex; sometimes (and this has happened in the same case at different times) the soft blowing murmur was audible only over the auricle, and the rough one at the apex. The reverse of this has never happened; these two murmurs either ran into one another, or were separated one from the other by a short but appreciable interval, capable of being increased by slowing the heart's action. I explain this phenomenon, however, differently from him, and in this fashion: at the end of the normal systole the ventricle suddenly relaxes, the apex descending to its normal position in the cavity of the chest, its descent being necessarily accompanied *pari passu* by the flow of blood from the auricle into the ventricular cavity, attended, as Hope says, by a slight movement mainly of the auricular appendix; then follows the long pause, the first portion of which is occupied by the gradual distension of the auricle, probably by a slight further distension of the ventricle, and the latter part by the auricular systole, which is immediately followed by the ventricular systole. But the completion of the ventricular diastole is of necessity delayed by any constriction of the auriculo-ventricular opening which obstructs the flow of blood into the ventricle, just as the diastole of an India-rubber bag syringe is delayed by the partial obstruction of its nozzle; and, if, from roughening of the upper surface of the mitral valve, roughening of its orifice, or any other unknown cause, the diastolic passage of the blood into the ventricle should be rendered sonorous, we have presented to our ear the phenomenon of a murmur which is always soft, blowing, or musical, never harsh or rough; which runs quite up to the rough murmur of the auricular systole, or is separated from it by a more or less appreciable interval—representing the long pause—according to the amount of stenosis present. The more trifling the constriction present therefore, the shorter the murmur, the longer the pause; and, *vice versâ*, the greater the constriction present, the longer the murmur and the shorter the pause, which latter, if the constriction be very great, may be altogether absent. To this statement I have only to add that I have found the criteria narrated, where present, perfectly sufficient in making the diagnosis of a greater or less degree of stenosis.

In regard to the cause why direct murmurs should always be so rough, even when produced by the comparatively feeble muscular action of the auricle, and indirect—regurgitant—murmurs always soft and blowing, even when produced by the much more powerful action of the ventricle, the only reason which seems to me at all probable is, that in the one case the muscular force—as developed in blood-currents—directly converges upon the orifice through which the blood is forced, while in the other this is not the case. The softness of the diastolic auriculo-ventricular murmur—which is direct in its course though not in its character—is obviously due to the absence of any muscular force directly converging upon its point of origin at the moment of its occurrence; the only muscular action synonymous with it being that of the terminal portions of the pulmonary veins, which is so feeble as to be utterly lost as to sound-producing power before it reaches the auriculo-ventricular opening.

In conclusion, I may add that, however we may explain the mode of origin of the presystolic murmur, its diagnostic significance remains wholly unaffected. We all acknowledge its pathognomonic character as distinctive of mitral stenosis; as practical physicians, therefore, we can afford to wait with perfect equanimity the arrival of that *magna veritas* which, as Dr. Barclay has not less tritely than truly remarked, *prevalebit*, but which in cardiac physiology is somewhat difficult of attainment.

Alva-street, Edinburgh, April, 1872.

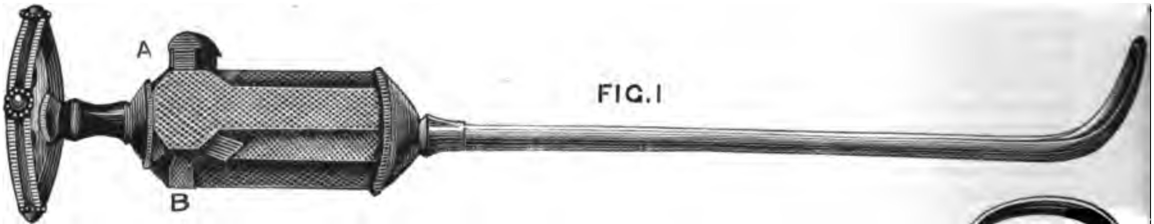

FIG. I

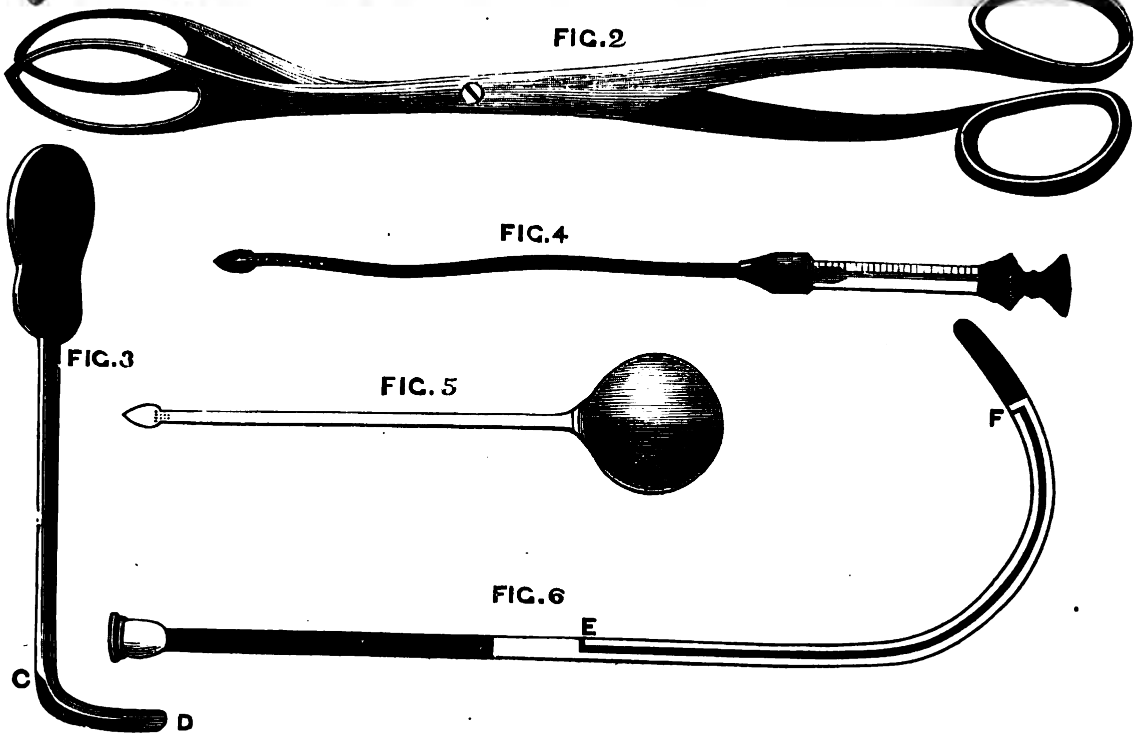

FIG. 2

FIG. 3

FIG. 4

FIG. 5

FIG. 6

SOME IMPROVED SURGICAL INSTRUMENTS.

By W. F. Teevan, B.A., F.R.C.S.,

Surgeon to the West London Hospital; Surgeon to St. Peter's Hospital, and Late Lecturer on Anatomy at the Westminster Hospital.

Fig. 1 represents a lithotrite of different construction to any in use.* Having tried variously shaped lithotrites, I found that the small square handle of Civiale's instrument possessed certain advantages which were, I considered, capable of being still further utilised. Accordingly I had my lithotrite made with a heavy octagonal handle. The sliding movement is converted into the screw action by the simple pressure of the thumb on the vertical slide A, which causes it to descend and project as much at B as it did at A. If pressure be applied at B the slide will ascend and the blades be no longer fixed. The advantages possessed by a lithotrite of the construction I have described are—1. The flat surfaces of the handle enable the fingers to rest steadily and lightly on them, and thus permit the greatest delicacy and accuracy in manipulation, whilst the angularity of the octagonal handle imparts a firmness to the grasp unattainable in other instruments. 2. The heaviness of the handle enables the lithotrite to glide into the bladder more easily than one possessing a lighter handle; it simply requires, not pressure, but only guidance. 3. Vertical pressure on the slide is the one only action which causes the minimum amount of movement of the lithotrite, and is consequently the least disagreeable to the patient.

Fig. 2 represents the forceps I use for extracting a stone in the adult. It is an open polypus forceps of very slight construction, with fenestrated blades serrated on the inside. It will never let a stone slip, and its circumference, plus the stone, only equals that of the latter. In a prolonged operation the ordinary lithotomy forceps is very liable to let the stone slip if the surgeon desires to change hands or give it to an assistant whilst he enlarges the wound. Not so with the forceps I have described, for if the grasp on the handles be relaxed it will still firmly hold the calculus, as its blades are flexible to a considerable extent. I would here remark that the best way of using the forceps is that advocated by Mr. Coulson as follows:—When the forceps has been introduced into the bladder the blades ought to be quickly and forcibly expanded, and then slowly closed, when the stone will be found to have fallen into the grasp of the instrument. Any attempt to grope about the bladder for the stone, or to nibble at it with the forceps, when felt, will only tend to prolong the operation. I have never found Mr. Coulson's manœuvre fail. In children I do not use a forceps, but extract the calculus with the left forefinger.

Fig. 3 represents my rectangular staff. It differs considerably from Dr. Buchanan's well-known instrument. The handle is twice as large as is usually made, the shaft is shorter, and the horizontal part scarcely forms with the shaft a right angle. The groove is peculiar, inasmuch as it is continued upwards at C for half an inch on the vertical part, whilst the other end of the groove terminates half an inch from D in an acute angle. Lastly, the groove is not so much lateralised as in Dr. Buchanan's staff, but occupies much the same position as in the ordinary curved staff. The advantages of my staff are that the large handle allows a firm grasp, so that the surgeon can drive his knife boldly along the groove without the instrument quivering under the pressure, as is often seen. In Dr. Buchanan's instrument it is not always easy to get the knife into the groove, for, if it strike the cylindrical vertical shaft, it will glide off; but as in my instrument the groove is continued upwards for half an inch, the surgeon will find no difficulty in getting into it at the first essay. The most important point connected with a staff is the termination of the groove near D. If it end in an obtuse angle, or even a right angle, there is always the danger of the knife travelling on, and piercing the posterior wall of the bladder, as happened twice to a London hospital surgeon with fatal results from peritonitis. Now with my staff such a misforture is not possible, as the point of the knife becomes locked in the acute angle, and so arrested in its progress.

Fig. 4 is a glass syringe with an elastic nozzle, six inches long, graduated, and surmounted with a cone, for applying injections to the deeper part of the urethra. It is very undesirable to use a metallic nozzle, as it is not only painful but injurious. The cone enables us to know with certainty when we have arrived at the veru montanum, or diseased spot, as measurement cannot always be relied on, for the penile urethra is continually altering its length. The syringe is capable of holding twenty minims of fluid, but it can never be necessary to apply more than five minims, as that quantity will paint a large surface of the urethra. No solution of nitrate of silver ought to be used stronger than five grains to the ounce. I have seen disastrous results ensue from the application of powerful injections to the deeper part of the urethra.

Fig. 5 represents a syringe for patients to use. It consists of an india-rubber ball with a bone nozzle three inches long, tipped with a cone. It is applicable for gonorrhœa and gleet, and is durable and inexpensive.

Fig. 6 is the section of an elastic prostatic catheter, whose curve is preserved by a steel spring six inches long, extending from E to F along the concavity of the interior of the instrument, and preserved from rust by being inserted between the coats of the catheter. This instrument does away with the trouble of "setting," and is peculiarly adapted for those patients who cannot dispense with the daily use of a catheter. If a soft catheter cannot be used without a stiff stylet to preserve its curve, it loses its special advantages over a metal one. Now, the catheter I have described, although perfectly pliant and capable of extension, will still preserve its curve whilst entering the bladder, as the spring causes the instrument to hug the upper wall of the urethra, and avoid the third lobe of the prostate by tilting the point upwards and forwards.

All the above instruments were made for me by Messrs. Mayer and Meltzer, with the exception of the elastic nozzle (Fig. 4), which I obtained from M. Lasserre.

Portman-square, April, 1872.

ON INCONTINENCE OF URINE IN CHILDREN.

By J. Warrington Haward, F.R.C.S.,

Assistant-Surgeon to the Hospital for Sick Children.

Among the minor ailments of children, one of the most common and troublesome is incontinence of urine—an affection which, besides the misery and ridicule that it often brings upon those subject to it, not infrequently, in an indirect manner, gravely interferes with their health. True incontinence of urine in adults usually indicates serious disease of the nervous centres, and has quite a different meaning and importance to that met with, as a rule, in childhood; wherefore I shall confine these remarks to the affection as it occurs in children and young persons. Although it is in these of much less serious import than the same symptom occurring in adult life, and depends, generally, upon much more remediable and transient causes, yet even here its effects are often very distressing. There are several cases on record, and I have myself seen two, in which the urethra has been cut through by a string tied tightly round the penis by boys anxious thus to prevent the escape of urine during sleep. A troublesome eczema, and even considerable ulcers, may be produced by the constant irritation of the urine upon the skin: a condition which, when combined with any disease requiring confinement to bed, often leads to serious bed-sores. For this reason, it is always desirable, when any surgical operation is to be undertaken on a patient with this affection, to first cure, if possible, the incontinence. I have lately had a good example of this in a boy who came under my care for the treatment of talipes, which had failed owing to the occurrence of sloughs wherever any pressure was made on the foot, and the boy had besides two large and deep ulcers over both hips. I found that he was constantly wet from the involuntary escape of urine—a condition that I have observed is often associated with talipes, but which in his case was due to congenital phimosis. I at once circumcised him, and thereby cured the incontinence, and at the same time the tendency to sloughing; the sores on the hips began at once to heal, and the talipes was easily remedied. Besides these physical evils, incontinence of urine often brings upon the unhappy child a painful amount of persecution, depriving it of many of its natural pleasures, and perhaps materially interfering with its education.

The treatment of this affection, then, is not without interest, and its success will depend very much upon the accurate recognition of its causes; for these are manifold and various. And first, the cases may be broadly divided into two chief classes: (*a*) those depending upon deficient nervous or muscular action; (*b*) those which have for their cause an excess of this action. The normal retention and passing of urine depend upon the proper balancing of the expulsive and retentive forces—that is to say, of the muscular walls and of the sphincter of the bladder. And this balance may be disturbed either by a want of sphincter power or an excessive expulsive power. Now the first of these—the want of sphincter power—is, I believe, in children, much the less frequent of the two causes, and is usually associated with delicate health, spinal curvature, talipes, or other paralytic affections; it is also the more troublesome to cure. The second and more common cause, the excess of expulsive power, occurs, on the other hand, in perfectly healthy children, and may depend upon the condition of the urine, or of the bladder, or upon some neighboring or eccentric irritation, and is of the two much the more easily remedied. There is no doubt also a certain, but I believe a small, number of cases which depend upon mere indolence and bad habit; and these must be treated by careful management, and, if need be, punishment; which, I may remark, should not depend upon the temper or caprice of a nurse, but should be carried out in an intelligent manner.

In the cases due to want of sphincter power there is usually incontinence by day as well as by night; the water escapes very frequently and in small quantities, and the child is constantly wet; they are mostly associated with general nervous and muscular debility, and the expulsive power of the bladder is also weakened. These cases will be benefited by tonics, and especially by strychnia. I have seen excellent results from this remedy, but it must be persevered with and given in sufficient doses, and not relinquished if it does not in the first week or two produce much benefit; for it must be remembered that this class of cases always requires prolonged treatment. I have also seen benefit from the liquid extract of ergot, but this is less to be depended upon than strychnia; and several children in which all other means had failed, I have cured at once by the application of solid nitrate of silver to the neck of the bladder, or the orifice of the urethra. These were all girls. The majority of cases, however, depend upon the second cause—an excess of nervous or muscular action. And in these the balance of expulsive and sphincter power is not, I believe, altered by the weakening of the sphincter, but by a spasmodic expulsive action which the sphincter is incapable of resisting. Thus, during the day, by voluntarily adding to the contraction of the sphincter, the effects of this expulsive spasm may be to some extent resisted, and the water is retained; moreover, as in some other spasmodic affections, these involuntary contractions occur less frequently in the day than during sleep, when the voluntary controlling powers are in abeyance. Again, the causes of this kind of incontinence are of an irritant nature, and such as to excite spasm rather than to weaken the sphincter; a view which is besides corroborated by the fact that the most effectual remedies are those which remove irritation and allay nervous and muscular excitement. It is remarkable, too, that, like chorea and epilepsy, the incontinence often ceases during an acute illness, but returns with convalescence. In boys, I think, by far the most common cause of this muscular spasm is the existence of congenital phimosis, which, from its retaining the secretion round the glans, the barrier which it offers to proper cleanliness, and the adhesions which it frequently leads to between the preputial mucous membrane and the glans, is a constant source of irritation. I have seen so many children mercilessly dosed with iron and other physic, when the incontinence was at once cured by circumcision, that I cannot avoid laying some stress upon the apparently obvious necessity of examining the urinary organs of all children suffering from incontinence. And I would point out that in many of these boys a very moderate degree of phimosis will be sufficient to cause incontinence; there is often an excessive length of prepuce with but little contraction of the orifice; such cases are almost certainly cured by circumcision. It must be remembered, however, that a bad habit often persists after its original cause is removed, and therefore it is sometimes necessary to take some pains to break these children of the habit of passing water during the night for the first week or two after the operation; as, for instance, by waking them a little later every night; measures which, though ineffectual before the operation, will after it soon be successful. Many of these cases, however, are at once cured by the operation without any further trouble. Another frequent source of irritation is the presence of ascarides in the rectum, and thus the incontinence may sometimes be cured by a compound scammony powder, followed by iron. Fissure of the anus, though not common in children, may also cause incontinence of urine, and it is remarkable sometimes how much of the inconvenience is referred to the bladder rather than to the real seat of disease. Of course this would be treated by the appropriate operation. Excessive ascidity of the urine is another cause which, it will be remembered, depends usually upon disordered digestion, and must be treated accordingly. Alkaline urine is occasionally present in these cases, and, when not associated with cystitis, is generally found in hysterical girls. In these, phosphoric acid, chalybeates, and valerian will be the most appropriate remedies.

Incontinence of urine is sometimes the only symptom of stone in the bladder, especially if the stone be a very large one, so that sounding must not be omitted in our investigation into the causes of this affection. But there remain certain cases, and of these not a few, in which we can discover no cause whatever for the affection; and these are they which are, as a rule, to be cured by belladonna. To prevent disappointment it is needful to take care that the preparation of the drug is a good one, for specimens of belladonna differ much in potency. The extract is, I think, the best form of giving it, and should be administered in doses commencing at one-sixth or a quarter of a grain, and, if necessary, increased till some dryness of the fauces or other physiological effects are produced; if then there is no improvement it is not likely to be beneficial. It matters little whether it is given during the day or only at night. Chloral has been highly spoken of as a remedy for this affection; but, though I have tried it pretty extensively, I have not found it nearly so efficient as belladonna. Neither have I found blisters, which are recommended to be applied to the sacrum or umbilicus, of much use. Bromide of potassium and cantharides are also by some recommended, but have generally failed in my hands. I have recently been testing the efficacy of all these remedies, and my opinion of them is founded upon that experience. Nevertheless, they will some of them doubtless succeed when others fail; but of the cases for which no cause can be found, there are, I think, very few indeed which are not to be cured by belladonna.

Queen Anne-street, W., May, 1872.

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CASE OF SYPHILITIC DISEASE OF THE BRAIN:

COMPLETE PARALYSIS OF RIGHT ARM; PARTIAL PARALYSIS OF RIGHT LEG; APHASIA.

By J. Lockhart Clarke, M.D., F.R.S.

H. H——, a builder, aged forty-seven, contracted syphilis nineteen years ago. Two years later he had a large syphilitic ulcer on the right leg. Ever since he suffered at various intervals from periostitis of the legs, sternum, shoulders, and especially of the right brow, which was frequently swollen and tender to the touch. These symptoms were soon removed by iodide of potassium, in doses of from five to eight grains; and, as he was somewhat cachectic and weak, I prescribed at the same time some citrate of iron and quinine.

During the last two years he had great anxiety of mind arising from losses in business, and frequently indulged to excess in the use of spirits.

On Feb. 14th, 1863, he had sudden but only partial loss of power in the right arm and leg, and fell to the ground. On protruding his tongue it pointed to the right, while the face was drawn to the opposite side. The attack was preceded for some hours by occasional inability to complete the pronunciation of a word or to articulate all its syllables. The right hand and wrist were much swollen. There was partial ptosis of the left eyelid, and a dull, stupid expression of the countenance.

On the following day, when I saw him with his local medical attendant, he had taken some ammonia, some calomel, and an aperient. He was ordered to have his feet in a hot mustard-bath, to have a blister behind the left ear, and to take ten grains of iodide of potassium every four hours.

Feb. 16th.—Somewhat improved; can move the right arm a little better; can walk across the room alone, but drags the right leg.

17th.—Much improved; can lift his right arm and shake hands; wrist and hand less swollen; tongue projected less to the right side. Ptosis of left eyelid has almost disappeared; power of articulation improved.

18th.—Continues to improve. Can lift his right arm briskly; swelling of wrist and hand nearly gone; ptosis entirely gone; face perfectly straight; walks perfectly with right leg; only some hesitation in speech.

20th. — Continued improvement; has walked round the garden.

Two days after he went to Brighton apparently well, and in good spirits. While there he took citrate of iron and quinine, with smaller doses of iodide of potassium.

In the first week of March he returned to London to appear in a law court, where he was subjected to great mental excitement. On the 13th, a month after he was first taken ill, he had another attack of paralysis. The right arm was completely paralysed; the right leg was not much affected. The face was only slightly drawn to the left, but was more so when he attempted to speak or laugh. The tongue when protruded pointed to the right. He was quite conscious, but had an idiotic or imbecile expression of countenance, and his eyes were never directed to any one spot, but had the appearance of those of a drunken man. The conjunctivæ were finely injected, but not particularly red. The pupils were of moderate size. There was partial ptosis of the left eyelid. The skin of the face and body had a yellowish tint. Pulse moderate, but fuller and stronger on the affected side.

These symptoms were preceded for a few days by occasional pain and sensation of pressure on the vertex of the head. He was ordered eight grains of iodide of potassium, and five grains of bicarbonate of potash, in decoction of sarsaparilla, every four hours; and a pill composed of one grain of subchloride of mercury and four grains of extract of hyoscyamus, every six hours.

March 14th.—Expression of eyes and countenance improved; slight ptosis continues; right arm quite useless; when spoken to or questioned upon any subject, answers nothing but "No;" has passed three copious and offensive evacuations; is unable to protrude the tongue. Ordered a blister to the nape of the neck.

16th.—Expression of countenance and eyes decidedly improved. After much persuasion to speak, he articulated "Yes," and, with some difficulty, "I can't." Right arm still useless.

17th.—Continues to improve; articulates better; can protrude the tongue a little, and just move the right arm as it lies in bed; skin still tinged with yellow; has passed three copious and offensive evacuations.

20th.—About the same; gums affected by the mercury, with slight flow of saliva; right arm still useless. To omit the pills, but continue with the iodide of potassium.

24th.—Decidedly improved; can articulate several words in succession, and more distinctly.

April 6th.—General health excellent, and appetite keen; walks and drives out; but right arm still powerless, and no improvement in the power of articulation.

15th.—On waking in the morning he found that he could lift his right arm, and move it about slightly.

From this time he continued to improve. The right arm became stronger every day, although the elbow-joint was contracted and stiff. The improvement in the power of articulation was slower, and, curiously enough, after getting out a few words with difficulty and hesitation, he was immediately and irresistibly impelled to utter rapidly and quite distinctly the three words, "go to bed." By the use of electricity the contraction of the elbow-joint was much reduced.

For the last six years his health has been perfect. He is stouter and much more robust than he was before his illness, and not only superintends his business, but can walk for miles without fatigue. His intellect is perfectly clear, and his memory is excellent, but he has still some difficulty of articulation, although he can say everything that he desires and keep up a continuous conversation. His articulation very much resembles that of a foreigner whose pronunciation of English words is awkward and difficult.

Such was his condition when I saw him a few days since (on March 30th, 1872), nine years after his first attack. I then ascertained that the temperature of his right side is variously reduced, the reduction decreasing from below upwards. His right foot is always very cold, and often, as he says, "icy cold." The palm of his right hand is two degrees lower than that of his left. The temperature of his right axilla was rather more than one degree lower than that of the left. The lobe of his ear and his head on the right side were slightly but sensibly colder than on the left.

Harley-street, W., April, 1872.

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THE TREATMENT OF SMALL-POX BY VACCINATION AND THE INJECTION OF LYMPH.

By R. C. Furley, L.R.C.S.

The doctrine universally held, that vaccination will be of "no use" if delayed till five days after the inhalation of the germ of variola, I think, is erroneous. For some time past I have been in the habit of vaccinating every case of small-pox that has come under my care; and the result, as I shall show, is very encouraging, and seems to indicate that vaccination is not only prophylactic but *curative*. I have found, however, that, although the ordinary process of vaccination by scratching the arm is sufficient to modify the disease in infants, it is almost inoperative in adults. After having discovered that the ordinary process was almost useless in grown-up persons, I adopted the method of injecting lymph by means of Dr. Wood's hypodermic syringe. This instrument I also found occasionally to fail, because, on passing the wire through the needle after the operation to keep it clear, I have found it eject the lymph which I had hoped had been in the blood. This, I am afraid, has been the cause of two otherwise unaccountable failures out of sixty cases. How to obviate this source of fallacy occupied my thoughts for a considerable time. At last, however, I succeeded in inventing an instrument that promises to transfer lymph directly from the tube in which it is contained into the circulation. It consists of a hollow needle with a bore sufficiently large to admit of the introduction of a vaccine tube. The process consists in passing the point of the needle charged with a tube of lymph under the skin, and blowing the lymph directly into the blood. I have purposely described this minutely, in order that, if any of my brethren do me the honor of verifying the results of my treatment, they may not commit the errors which experience has pointed out. It would occupy too much space to describe many cases; I shall therefore only give three to illustrate the effect of the treatment at the different periods of life—infancy, childhood, and manhood.

Case 1.—Baby C——, aged one month, and unvaccinated. When first seen, the papular eruption was over the face, hands, and legs. There had been three cases of small-pox in the house. One had been sent to the Small-pox Hospital, where the mother had visited him. I ascertained that this was really the case from the resident physician, Dr. Saxby. I vaccinated the child at once. Next day, when I called, the eruption had entirely disappeared, with the exception of two papules on the face, which were now more prominent. On the third day these two papules disappeared, and a fresh crop of four-and-twenty, chiefly over the head, made its appearance; and these became hard, and did not fill like ordinary vesicles. In three days these had also disappeared. The vaccination itself did not show any signs of taking till the tenth day, and was matured on the thirteenth.

Case 2.—M. R——, aged thirteen years, vaccinated in infancy, and whose sister had died of small-pox a week before. I was called in on a Saturday, and found her face considerably swollen, and the papular eruption on the hands and forearms. I at once *injected* two tubes of lymph into the arm. On Sunday the eruption had disappeared from the hands and arms, and appeared on the feet. On Monday it had spread up the legs and trunk. On Tuesday it had entirely gone away, and there was nothing to be seen but the inflamed areola at the point where I had injected.

Case 3.—J. W——, aged thirty-four, a stableman of dissipated habits, and never vaccinated. I saw him on the second day of the eruption, and injected two tubes of lymph. The case, instead of being confluent, as might have been expected, was discrete, except at the alæ of the nose. He went on very favorably, and the eruption began to desiccate on the ninth day instead of the fifteenth, eighteenth, or twentieth. There was no areola at the point of injection.

I think these cases, selected from a number of such, show that there is at least a germ of truth in my statement that vaccination is curative of small-pox. I find that the treatment is much more successful in the youthful than in the adult period of life; and I am beginning to think that this arises from adults requiring, as it were, more of the remedy. It is also more effectual the earlier the operation is performed. I have usually found that the vesicles did not attain the size they do when the disease is allowed to run its course; they did not often become pustular, and desiccated earlier.

No doubt I have had failures and deaths; the latter five in sixty cases, in three of which I did not look for recovery from the first on account of other complications, and two were unlooked for: all were females—a fact of itself of some significance. I reserve for a future communication a detailed summary of all my cases; meantime I trust I have said sufficient to encourage my brethren calmly to investigate the subject.

I should have mentioned that the instrument I employ for transferring the lymph directly into the blood was made for me by Mr. Young, cutler, North-bridge.

Lauriston-place, Edinburgh, May, 1872.

CASE OF UTERO-PERITONEAL FISTULA.*

By LAWSON TAIT, F.R.C.S.,

Surgeon to the Birmingham and Midland Hospital for Women.

HANNAH W——, aged twenty-four, was admitted as an out-patient on March 4th. She was married at eighteen, and has had five children, the last only six weeks ago. She had no assistance of any kind at her last labour; has little recollection of it save that it lasted a very long time, and that she lost a great deal of blood. She has never since been free from loss. She presented on admission a most anæmic, worn-out, and wretched appearance. Uterus large; felt easily above the pubes; cervix spongy and easily admitted the point of the finger; cavity admitted the sound nearly six inches. Ordered three grains of the citrate of quinine and iron to be taken in a mixture thrice daily, along with a pill containing a grain of ergotin.

March 8th.—The discharge has ceased; uterine cavity still six inches, but the tumour feels less externally.

April 1st.—Uterine cavity four inches, and the tumour much diminished. Ordered five grains of the iodide and twenty of the bromide of potassium, to be taken three times daily, along with five grains of the saccharated carbonate of iron in pill.

8th.—Uterine cavity three inches; but after reaching this distance the sound slipped, without the exercise of any force, through the fundus, apparently about midway between the cornua and somewhat on the anterior aspect, and could readily be felt under the abdominal parietes. The woman is so thin that the position of the exit of the sound can readily be felt. On this day I passed the sound twice, and each time found that it only required a little manœuvering to pass it at the one spot.

On the Saturday previous (13th) I had passed the sound through the fundus in presence of my colleagues, Dr. Savage and Mr. Bracey, and on the 22nd again before Mr. Creagh and Mr. Berry, satisfying them all as to the fact of the perforation, the patient never seeming in any way the worse of the operation, or to suffer any pain in its performance. She has improved in general health in a very marked degree since she has been under treatment.

On April 24th, as I wished to bring the case under the notice of the Midland Medical Society, and as it was evidently inconvenient and unadvisable to publicly exhibit such a case, I asked my friend Mr. West, the President, to verify with me in consultation the fact of the perforation of the fundus. This he kindly did, and our united representation was accepted by the Society.

Of the fact of the perforation there can be no question; it only remains to explain it. That there is a fistula I have no doubt, though it may be difficult to persuade many that such is the case. I am also under the belief that it had its origin in a rupture of the uterus during labour, remaining undiscovered until the walls of the uterus became thinned by the process of involution.

Cases of perforation of the fundus by the sound are, I am convinced, by no means so rare as one would be inclined to believe, for I have seen at least five or six, and Sir James Simpson used to speak to us of having seen many. They are very curious, and give rise to many interesting subjects for consideration. For instance, is it likely that the subject of this fistula will ever again become pregnant? I see no reason why she should not, and I believe the fistula will remain patent until she does. Her case is also one of the many lessons leading us to be less afraid of the peritoneal cavity than surgeons, from the old traditions, usually are; and if my view of its origin is correct, it is an additional instance of recovery from rupture of the uterus.

Birmingham, April 25th, 1872.

TATTOOING OR TINTING OPACITIES OF THE CORNEA AND SCLEROTIC.

By C. S. TICKHURST, L.R.C.P. Lond., M.R.C.S.E.,

Dresser to the Eye Department, Guy's Hospital.

IN the eye department of Guy's Hospital many cases have occurred of "tinting" the cornea in order to get rid of, or, more properly speaking, to hide, opacities which disfigure the look of the eye; and I purpose to make a few remarks concerning the operation, the results of which, I may add, have been most satisfactory. It is applicable to all kinds of opacities.

When "tattooing" was first begun at the hospital, one grooved needle was used; but I saw no reason why the operation should not be performed more quickly by using several needles at a time, and finding that the grooved needles when three or four were placed together, made too large a mass to work with, I took some ordinary sewing-needles (No. 5), and in a rough way fixed them on to a penholder, taking care to have the points as level as possible, and as close without quite touching. Two I fixed on one holder, three on another, and four on another; and used them according to the size of the opacity about to be tattooed. Four needles together will be found quite large enough for the largest leucoma. Having separated the eyelids with a speculum and fixed the eye with a pair of forceps, I paint some indian-ink, previously rubbed up thick on a saucer, over the opacity, and prick through it with the needles, which may be held either quite perpendicularly or in a slanting direction, wiping the superfluous ink away now and then to see how one is getting on, until the whole opacity is covered; and then, giving it a last paint over, I leave it to dry as much as possible on the cornea before taking the speculum out. Or the opacity may be pricked by degrees first, and then the ink put on, if anyone is not quite certain as to where he is pricking, although there will not be found any difficulty about that.

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* Read before the Midland Medical Society.

I think that the ordinary needles have this advantage over the grooved ones, that the tattoo looks neater. Mr. Bader has tried and approves of their use. As to the depth to which one pricks in the corneal epithelium it is only necessary just to enter it, and it requires more force to do so than one might imagine. After operations such as abscission, or where there is no cornea, only white conjunctival tissue, thus making the look of the patient very peculiar, the benefit to be derived from tattooing is great, as a good black spot simulating a pupil can be made by its means, thus altering and improving the patient's look immensely.

As regards after-treatment, no bandage is used at all; the eye may be bathed with water if uncomfortable, and the patient is directed to use his eyes freely.

April 1872.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

UNIVERSITY COLLEGE HOSPITAL.

EXCISION OF UPPER JAW; RECOVERY.

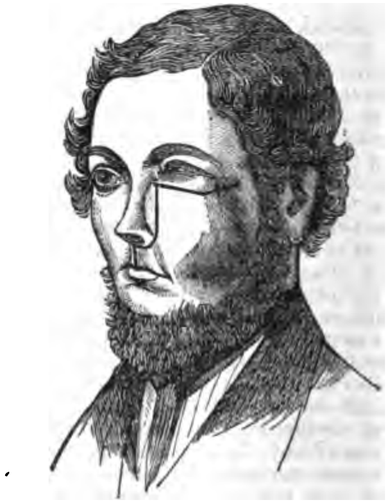
(Under the care of Mr. Erichsen.)

For the notes of the following case we are indebted to Mr. T. Barlow, ward clerk.

D. K——, a farmer, aged twenty-nine, was admitted into the hospital on Feb. 6th, 1872. Has been a strong, healthy man, and has had no serious illness. About the beginning of last August he began to have a feeling of numbness over the left cheek, radiating from a point about midway between the external angle of the mouth and the external canthus. When he gaped he had a feeling of tension as of a cord through the cheek, and also a tickling sensation along the ridge forming the inner boundary of the cheek. There has at no time been pain or throbbing. About the same time he began to notice a projection downwards from the roof of the mouth. Shortly after this a small swelling appeared on the spot above referred to. This appears to have increased upwards rather rapidly, so that in September his friends noticed that it was encroaching upon his eye by pushing upwards the lower eyelid. It also pushed up the inner canthus and pressed upon the lachrymal sac. There was epiphora. Five weeks ago an abscess formed over the lachrymal sac, from which pus and a watery fluid have been discharged up to the present time. Soon after the eye was affected the swelling began to press upon the nose, so as ultimately to prevent his breathing through the left nostril. Epistaxis, but never excessive, has occasionally occurred. As the tumour increased the upper molars became loose and were removed, as also the pre-molars, except a part of the first; from the cavities left there has at times been a little bleeding. During the last two months his friends think he has lost flesh a little.

State on admission.—An oblong tumour occupies the inner part of the left cheek. It measures two inches and three-quarters from above downwards, and two inches and a quarter from outside to inside. It has a defined edge on the outer side corresponding with a vertical line through the outer canthus. Below, it extends to within half an inch of the left angle of the mouth. On the inner side it has a defined edge where it encroaches upon the nose. Above, it presses up into the orbit, especially on the inner side. The tumour is not at all tender to tolerable pressure; it feels elastic throughout; no crackling; it is semi-fluctuating where it approaches the eye and nose. The skin is not adherent; color natural. The left nostril is closed by the pressure of the tumour. The eye is ordinarily closed, the lower lid being pushed up; when it is pulled down the patient can read without much difficulty. The pupil contracts perfectly; the conjunctiva is slightly injected; the caruncula protrudes more than on the right side. On looking into the mouth a tense oval mass is seen protruding from the roof. It is confined to the left half of the palate, and extends as far back as the last molar. Like the external mass, it does not crackle on pressure, but feels very elastic; the mucous membrane covering it is very pale. No enlarged glands in the submaxillary region.

On February 7th, the patient being under chloroform, Mr. Erichsen performed the following operation:—The left median incisor was first ex-



tracted; then two ring forceps, with elastic bands round them to keep them tightly closed, were introduced just inside the angle of the mouth. These compressed the coronary and other branches of the facial, and thus prevented hæmorrhage during the operation. An incision was made a little to the left of the middle line round the ala nasi, and along the side of the nose to just below the inner canthus, then outwards over the malar bone, nearly as far as the articulation of the lower jaw. The accompanying diagram shows the line of incision. This triangular flap was then thrown downwards and outwards. Mr. Erichsen then sawed through the hard palate from the mouth. The nasal process of the maxilla, lachrymal bone, &c., were broken through by a curved pair of bone forceps, thus opening into the bony orbit. The malar bone was then sawn through. As the mass was too soft to apply the lion forceps, Mr. Erichsen gradually extricated it with his fingers, cutting through the soft palate with a pair of long curved scissors. Two plugs of lint were stuffed into the wound, the coronary and one or two small branches tied, the ligatures cut close, and the flap was brought together by silver sutures and harelip pins at inner canthus and upper lip.

8th.—Some œdema about the orbit, and chemosis of the eye; the wound looks quiet; pulse 108; temperature 101·4°. He is taking spoon diet, four eggs and 8 oz. of brandy.

11th.—Mouth has been syringed out daily with Condy's fluid; the harelip pins removed; the line of incision has united by first intention; chemosis diminishing; temperature 99·6°; pulse 88.

17th.—Patient has been going on well. Yesterday afternoon diarrhœa came on; no cause can be assigned except that he is taking port wine and stout instead of brandy. The left half of face is swollen, and he complains of pain at the root of the nose; pulse 126; temperature 102°. Ordered an astringent mixture.

18th.—Diarrhœa less; there is a slight blush over the forehead radiating from the root of the nose; has slept well; no shivering or backache; temperature 104° in the evening; some of the wire sutures removed from the incision.

19th.—There are two or three blebs on the forehead; left eye much swollen; the right half of face much swollen and feels quite brawny; no change in color. Temperature (morning) 99·5°; pulse 96. One little aphthous spot on tongue. Temperature 104° in the evening.

20th.—Temperature normal; has been perspiring profusely; there are a few small pustules over the root of the nose and on his upper lip.

21st.—The erysipelas is fading; temperature normal. From this time the patient went on well, and he left the hospital on March 6th. The skin-flap had united well throughout; in many parts the trace of cicatrix is scarcely visible.

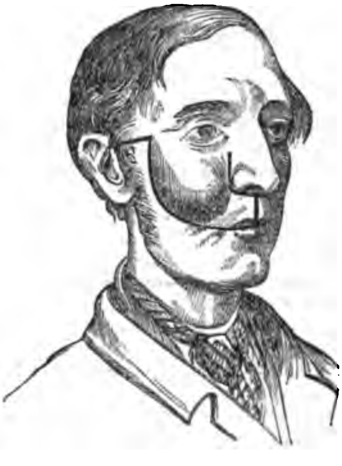
Description of tumour.—The mass filled up the antrum, and Mr. Erichsen thought went back as far as the sphenoidal cells. There were a few thin scattered bits of bone on the anterior surface, and also on the side towards the nose. On the posterior aspect the bone was quite gone. Its general consistency was something between gelatinous and pulpy; color slightly pinkish. Section of the mass hardened in spirit showed bundles of fibrous tissue, but not arranged so as to form a cancerous stroma; a number of simple rounded cells, and masses of spindle shaped cells.

FIBRO-PLASTIC TUMOUR OF RIGHT UPPER JAW; EXCISION; RECOVERY.

(Under the care of Mr. Erichsen.)

W. P——, aged fifty-six, was admitted into hospital October 5th, 1869, with a swelling the size of a Tangier orange in his right cheek. He had previously enjoyed good health. He gave the following history. Five weeks ago he first noticed a swelling in his right cheek; for a long time he had had aching pain in the right upper jaw, but took no notice of it, having been accustomed to face-ache. The swelling began in the middle of the cheek, about the level of the nares, and has spread in all directions, but chiefly upwards; he thought it was a gumboil. About a week after its first appearance, there was free bleeding from the right nostril for about half an hour; the mass has bled from the inside of the mouth several times; there is a sort of numb feeling throughout the whole mass. Since the tumour has appeared, he has lost flesh, and his appetite has failed; he complains of weakness and a feeling of languor.

On Oct. 9th Mr. Erichsen proceeded to remove the upper jaw in the following manner, as recommended by Liston. The right median incisor tooth was extracted; an incision was then made from the nasal process of the superior maxillary bone along the side of the nose, round the ala, and through the centre of the upper lip. The accompanying diagram shows the line of incision. Another incision was made from the angle of the mouth outwards and upwards, as high as the root of the zygoma, and some little distance along the process. The symphysis of the upper jaw was sawn through, the cutting pliers completing the section of the palate. Next the zygoma was cut across the outer angle of the orbit, and then the inner. The hæmorrhage was not severe. Three pieces of lint were placed in the opening to support the parts, and the flap fixed in its place by several harelip pins and interrupted sutures.



Oct. 7th.—The patient vomited several times during the night. He is taking two pints of beef-tea, a pint of milk, four eggs, and four ounces of brandy. The wound syringed twice with carbolic-acid lotion.

8th.—Passed a restless night. Pulse 84; temperature 104° F. In the afternoon Mr. Erichsen removed two of the pieces of lint. The wound was syringed, and brandy increased to six ounces. At 11 P.M. he was much easier. Pulse 88; temperature 101·4° F. There is redness and soreness of the right eye, with swelling of the eyelid. The discharge from the wound very offensive.

9th.—Passed a better night. Pulse 92; temperature 100·6°. All the lint removed. Wound syringed out as before.—11 P.M.: Pulse 96; temperature 102°. The wound is doing well.

The chief point of interest about this case was the occurrence of double suppurative parotitis; and, with this exception, the patient got on very well after the operation. The mouth was thoroughly syringed out twice a day with Condy's fluid. The abscesses in the neck were laid open, and gradually closed up, but the discharge was very copious for ten days. The patient was sent to Eastbourne in the beginning of December. Three or four months afterwards the patient came to see Mr. Erichsen; the disease had recurred, and a tumour involved the angle of the right orbit and the side of the nose.

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SUNDERLAND INFIRMARY.

CASES OF DIABETES.

(Under the care of Dr. PYLE.)

FOR the notes of the following cases we are indebted to Mr. T. F. Hopgood, house-surgeon:—

CASE 1.—J. G. B——, aged seventeen years, was admitted March 2nd, 1871, suffering from diabetes. He was quite well until three months ago. About this time he noticed that his appetite was much increased; he ate a great deal but was not satisfied, and he was constantly drinking without assuaging his thirst. He complained of no pain at this time; he passed a large quantity of water at a time and very frequently, but never saved it to know how much he passed in the twenty-four hours. At this time he weighed 9 st., but since then he has gradually decreased in weight. He has complained of slight cough for the last three weeks, and spat a little thick phlegm.

On admission the patient was much emaciated, the eyes fallen in, and very little adipose tissue remained. He was passing fourteen pints and a half of urine, and drank fourteen pints during the twenty-four hours. Upon testing the urine it was found to contain a large quantity of sugar, one-sixth of the tube being occupied by the deposit, sp. gr. 1045. Chest sounds natural except at left apex, in infra-clavicular region, where there was harsh breathing, and in the right scapular region where there were a few crepitant râles. Slept well; bowels open every day; tongue clean and moist; pulse soft, easily compressed, 70; weight 6 st. 11¾ lb. To have eight pints of skim-milk daily, and to take a mixture containing the compound decoction of aloes three times a day.

March 4th.—Has passed seven pints of water and drunk thirteen pints—viz., eight pints of milk and five pints of water; he is now to have as much milk as he can take.

26th.—To take five grains of the compound soap pill at bedtime; pulse, after taking it, 85; sp. gr. of urine 1010; quantity, five pints in the twenty-four hours.

28th.—To have one chop, greens, and eight pints of milk daily, and to take a mixture containing eight minims of the tincture of opium in the dose three times a day; weight, 6 st. 5¾ lb.

April 14th.—To have two chops, five pints of milk, and greens.

18th.—To discontinue medicine, and to take, three times a day, three grains of the compound soap pill.

20th.—Ordered a mixture containing ten grains of hydrochlorate of ammonia three times a day.

From April 20th to May 25th the quantity of urine passed daily was four pints and a half; with the exception of May 2nd and 25th, when it reached five pints, and since that time it has continued at this quantity. The diet and medicine have been the same. His weight has gradually increased. On April 7th it was 6 st. 6 ½ lb.; April 29th, 6 st. 12½ lb.; May 6th, 6 st. 13¼ lb.; May 14th, 7 st. 0¾ lb.; May 20th, 7 st. 4¼ lb. The specific gravity of the urine was 1010 on May 8th, and continued so to May 16th; it then rose to 1020, and remained so till the 26th. On May 26th and 27th he was allowed potatoes, &c. The quantity of urine and drink remained the same, but there was a slight increase in the sugar.

Two months after this he went to sea, but returned in the same condition. During the time he lived chiefly on pork-pies, vegetables, and potatoes, and lost 12 lb. whilst at sea.

CASE 2.—John R——, aged forty-one, was admitted March 28th, 1871, suffering from diabetes. Had been a sailor up to nine years ago. Since then he had worked at making sails. Three years ago he met with an accident, causing fracture of the patella, since which time he has been unable to work. Five weeks ago he had rheumatic fever. About four or five months ago he first noticed that he drank more than usual, and that he required much more food than before. At this time he drank about ten or twelve pints of fluid, and passed about a gallon of water, during the twenty-four hours. Twelve months ago he weighed 11 st.

The patient was much wasted; appetite increased, and thirst constant. Drank twelve pints during the twenty-four hours, and passed ten pints of water, which contained a large quantity of sugar, its specific gravity being 1040; weight, 8 st. 10¼ lb. He was put upon eight pints of skim-milk, and to have no other food; and to take a mixture containing eight minims of tincture of opium three times a day.

On April 4th the tincture of opium was increased to fifteen minims the dose, and he was placed on meat diet—two chops—and five pints of skim-milk.

May 1st.—He has continued as above to this date. He is now to leave off the opium, and to have a mixture containing one grain of carbolic acid three times a day, and the following diet: five pints of milk, two chops, and green vegetables.

10th.—Since he has been taking the carbolic acid he complains of feeling sick and of having a pain in the side. To omit the acid, and to have ten minims of the tincture of opium three times a day.

29th.—Has continued the above treatment to this date. To have full diet—meat, bread, and potatoes, with half a pint of beer.

He left the infirmary on the 31st improved, the sugar having diminished to one-eighth the quantity. There was a slight increase in the quantity after the full diet, and the specific gravity of the urine was 1090. He drank five pints of fluid in the twenty-four hours, and passed five pints and a half of water.

Since this time he has attended as a dispensary patient, and has been living upon eggs and bread made of bran soaked and washed and then mixed up with milk and gum acacia. He has taken five grains of bromide of iron three times a day. He says he has not been so well since he left the infirmary, but has lost in weight only 1¼ lb. up to this date (Aug. 15th), when he was again admitted as an in-patient. He still suffers from thirst, and passes an increased quantity of urine; specific gravity 1034, and contains a large amount of sugar. He was ordered five pints of skim-milk, vegetables, and meat, and to take a mixture containing opium.

He left the infirmary on Sept. 15th, having continued the same diet during the month; but, after the first few days, taking five grains of the bromide of iron instead of the opium. He was in much the same state of health as on admission; he was taking less drink, and passing less water, the quantity of sugar remaining about the same. He lost 1 lb. in weight during the time.

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ST. BARTHOLOMEW'S HOSPITAL.

ABSCESS OF THE LIVER; OPERATION; RECOVERY.

(Under the care of Dr. BLACK.)

For the notes of the following interesting case we are indebted to Mr. Henry Thompson, house-physician.

C. W——, a porter, aged thirty-one, was admitted into hospital on the 19th of August, 1871. He had served as a soldier for six years in India, during which time he had a severe attack of dysentery, from which, however, he completely recovered. This was four years and a half ago. About a year after this he was admitted into the hospital at Lucknow for some liver affection, which he has continued to suffer from at times ever since. He was discharged from the army on account of this three years ago. Since January, 1871, he has had continual pain in the region of the liver. He came as an out-patient to Dr. Church in April, who admitted him into the hospital. He was in the same ward, and an abscess of the liver was diagnosed. He left the hospital in June, somewhat relieved. He is a well-formed, moderately muscular man; has always been temperate; never had syphilis, but had gonorrhœa ten years ago. He says he has lost flesh lately and is much thinner. He complains of great pain and tenderness on the right side, especially just below the ribs. This pain is much increased by pressure; it is more severe at night, and almost entirely prevents him from sleeping. He cannot lie on either side on account of the pain it causes him. Lately he has had slight rigors—about once a week, he says.

State on admission.—Expression somewhat anxious; face pale; pulse full and strong, 84; temperature 100°; appetite bad; bowels regular, but fæces are lighter than normal; tongue red, furred in centre. Urine: specific gravity 1010; acid; no albumen, bile, or sugar. Chest well formed; percussion over left side normal; on right side in front, dull from an inch above the nipple to the level of the umbilicus, and from an inch to the right of the sternum to two inches and a half from the spine behind. Heart and lungs healthy; respiration is feeble on the right side, and no respiratory sound is heard over the dull area in front. The right side of the thorax, midway between the sternum and the angles of the ribs, pits on pressure, and is considerably fuller than the corresponding part of the left side. Circumference of the chest at its widest part, 34½ in.; the right half measures 16 in. and the left 18½ in. There is some congestion of the integuments on the right side. He was ordered a mixture containing nitro-hydrochloric acid, and a morphia draught at bed-time.

Aug. 24th.—The side is more painful but less congested; has not slept during the night, though the draught eased his pain; pulse 78; temperature 100°; tongue redder, furred in the centre; bowels confined; appetite very bad. The side is to be rubbed with chloroform liniment.

28th.—The pain was so severe last night that he was ordered twenty minims of tincture of opium, and to have the side rubbed with opium liniment; he slept for an hour or two. The swelling on the right side seems to be pointing opposite fifth, sixth, and seventh ribs; circumference 35 in. At a consultation with Mr. Thomas Smith it was decided, as fluctuation could be felt, to puncture the swelling. As the bowels were still confined one ounce of castor oil was given.

29th.—The patient having been put under chloroform, Mr. Smith made an incision over the most prominent part (about opposite the sixth intercostal space, midway between angle of rib and sternum) and then passed in a director, which seemed to enter a canal, until almost the entire length of the director was introduced. An immense quantity of pus gushed out—54 oz. When the patient coughed there was a distinct forcing out of the pus; a good deal of blood was mixed with the pus, which consisted almost entirely of broken-down liver-substance, but did not contain any bile. An india-rubber tube was then passed into the opening as far as the director would reach, and fastened to the side.

30th.—Tongue red at tip and edges; did not sleep at all last night, not so much from pain as from restlessness; bowels open once during the night; feels very comfortable this morning; has taken no nourishment except brandy-and-water. The opening has discharged freely, and continues to do so with each poultice; altogether 96 oz. have been measured, besides what came away into the bed and the poultices, which the sister computes at 20 oz. Pulse 75, rather thready; temperature 97·6°.

31st.—Says he feels "quite jolly"; slept well without an opiate, which is the first time he has done so for a month. The wound continues to discharge freely, but the tube has slipped out; pus much healthier-looking; appetite better; pulse 96, fuller; temperature 98·8°.

Sept. 2nd.—Yesterday Mr. Smith reintroduced the tube on a director; about twelve ounces of pus escaped, and it has continued to discharge freely since. Did not sleep well last night. Tongue unchanged. He had a rigor yesterday morning lasting about three hours, and a slight one during the night. Pulse 84 full, strong; temperature 99·2°. To have a chop, beef-tea, and five ounces of brandy.

4th.—Has had no more rigors. Feels pretty easy, but complains of having to lie so long in one position. Bowels open daily; appetite improving; tongue cleaner. The wound discharges freely; the pus more blood-stained.

9th.—Tongue not so red; sleeps well; appetite very good; bowels regular. Pulse 90; temperature 100·2°. The abscess was ordered to be injected daily with a lotion containing one drachm of the tincture of iodine to the pint of water.

17th.—Improving; very little pain or tenderness; much less discharge from the wound; tongue quite clean; bowels regular; appetite good.

20th.—Patient gets out of bed daily; no pain; discharge less.

Oct. 2nd.—Says he feels as "jolly as a cricket," and better than he has done for three years; looks quite fat in the face; tongue quite clean; appetite very good; no tenderness over chest. The right side dull in front, from two inches below the nipple to the level of the eighth rib; very little dulness posteriorly; respiration heard down to the tenth rib, although not quite so strong as on the left side. The wound looks like a small cloaca. The tube was taken out on Sept. 20th. There is hardly any discharge from the wound.

12th He was discharged cured.

He came to the hospital a few months after, looking as healthy as a man could, and stating that he never felt better in his life.

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WORKHOUSE HOSPITAL, MANCHESTER.

LEUCOCYTHÆMIA; GREAT HYPERTROPHY OF SPLEEN; CONSTANT DEPOSIT OF URIC ACID IN URINE.

(Under the care of Mr. W. A. Patchett.)

Mary H——, aged twenty-three, unmarried, was admitted into the Workhouse Hospital on October 20th, 1871, complaining of a tumour in the abdomen and great weakness. Up to the commencement of her present ailment she had always had good health. Five years and a half ago she had small-pox mildly, after recovery from which she felt weak and languid. She gradually lost strength and flesh, when, eighteen months after the attack of small-pox, her friends noticed that she looked "swelled," and she perceived that there was a hard mass more to the left side of the abdomen. She had no pain, and never had any. For above six years she had been accustomed to bleed at the nose every day, and sometimes several times in a day, to the amount of three or four tablespoonfuls. At times she had lost as much as a pint of blood. Catamenia had never appeared since about the time she observed the tumour. The epistaxis had been in greater quantity at monthly periods.

State on admission.—The patient is of small statue, lean but not emaciated, and of pale complexion. Before she had small-pox she says she was stout and ruddy. Is very anæmic. The abdomen is considerably enlarged. On examination the whole of the left side is found occupied by a hard tumour. The tumour is movable, and, on relaxing the abdominal muscles by drawing up the knees, can be plainly felt emerging beneath the cartilages of the false ribs from the splenic region. It is hard and smooth to the feel; the upper edge passes away to the right above the umbilicus, it then passes obliquely downwards and to the right, three inches external to the umbilicus, to the right groin, curving round below along Poupart's ligament. The tumour has a sharp anterior curved edge, and a notch is very distinctly felt in the right iliac region. The tumour can be moved from side to side; it also moves up and down with respiration. Percussion over the tumour is dull throughout; a bowel sound can be obtained on deep percussion for a distance of four or five inches to the left of the edge of the tumour opposite the umbilicus. The circumference of the abdomen around the widest part (opposite the umbilicus) measures forty-four inches. There is no fluctuation, no pain, or tenderness. The liver is enlarged. The respiratory and cardiac sounds of the chest are normal. Pulse 92, small and weak; temperature 98·2° F. Tongue clean and moist; gums pale; all teeth decayed, and some lost; appetite poor; bowels costive. The urine is pale-amber color, acid, clear, specific gravity 1018, and contains very copious deposit of light-brown crystals of uric acid, some of which under the microscope appear rhombic, others lozenge-shaped. The patient has no pain anywhere, and complains of nothing but "weakness and the tumour." On examining the blood drawn from the extremity of the finger microscopically the white corpuscles were found to be enormously increased in number, the blood at first sight appearing to consist altogether of white cells, until it was examined more closely, when the red were seen to be in rouleaux. The proportion was, on carefully counting several specimens, a hundred red to ninety white cells.

The patient had been in hospital about three weeks when she fell down some stone steps, and fractured the base of the skull, from the effects of which she died. Whilst in hospital the urine was examined daily, and always presented a copious deposit of crystals of uric acid. Epistaxis recurred almost daily, on two occasions to such an amount as to require plugging of posterior nares.

Post-mortem appearances.—The spleen was enormously hypertrophied; it weighed 12 lb. 6 oz., and measured in length, 18 in.; in breadth, opposite widest part, 10 in., and 6 in. in thickness. Liver was enlarged; it weighed 8 lb.; structure healthy. Mesenteric, axillary, and lumbar glands all enlarged. Heart and lungs perfectly healthy in structure. The blood in the right cavities of heart and large veins firmly coagulated, the clots consisting of two portions, a reddish-black layer at lower part, and a yellowish one at upper part of clot, the former consisting of red corpuscles with some white cells, the latter almost entirely of white blood-corpuscles. The sinuses of brain were filled with coagula. (A fracture of base of skull was found running across sella turcica.)

Remarks.—This was a well-marked and typical case of leucocythæmia. The persistent presence of an excess of uric acid in the urine was interesting as tending to confirm the theory of Liebig on the pathology of uric-acid deposit. Of course a deposit of uric acid does not always indicate that there is an increased excretion, but its presence was constant, and there was a copious deposit in urine voided only half an hour previously. The opponents of Liebig's theory (particularly Dr. Golding Bird) say that in all diseases attended with excessive debility, especially where an anæmic state exists, a deficiency of uric acid occurs, and that the diminution of uric acid is best observed after losses of blood. Liebig's theory is that when in the exhausted tissues containing protein the vital force is no longer able to resist the chemical action of the oxygen which is conveyed to them in the arterial blood, it combines with their elements and forms products, among which uric acid is the chief. Thus, 1 atom of the elements of protein with 91 atoms of oxygen are equal to the equivalents of uric acid, carbonic acid and water. Liebig believes that the oxygen conveyed in the red blood-corpuscles, converts the insoluble uric acid into soluble urea, and therefore whatever increases the number of red blood-discs must cause the more complete conversion of uric acid into urea, and less of the former and more of the latter will appear in the urine; and, conversely, whatever interferes with oxygenation must produce an excess of uric acid. Dr. Bird says "so far as recorded facts are concerned they fail altogether to give the slightest support to Liebig's ingenious theory." The facts in this directly confirm the views of Liebig, and oppose those of Bird. Dr. Bird says, "a deficiency of uric acid occurs in anæmia, and especially after losses of blood." In this case there was great anæmia, and there had been an almost daily loss of blood for six years and a half. In three other cases of leucocythæmia that Mr. Patchett has seen there has been an excess of uric acid, but he has not observed it in ordinary cases of anæmia.

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SEAMEN'S HOSPITAL, GREENWICH.

SUPPOSED HEPATIC ABSCESS DISCHARGING THROUGH THE LUNG.

(Under the care of Dr. Stephen H. Ward.)

T. B——, aged fifty-six, first admitted December 4th, 1871, with a history of dysentery that had occurred some nine months previously. Six weeks before entry he had been spitting up a large quantity of blood intimately mixed with some thick viscid material. General symptoms: cough at intervals; continual expectoration of a rusty-colored sputum, of an eminently brick-dust character; heart sounds healthy; urine normal; bowels regular; no dyspnœa, no genuine hæmoptysis; puerile respiration over left lung, before and behind; dulness on percussion under right clavicle; moist crepitation in front over middle third of right lung; some dulness over the same lung behind, with impaired inspiration and expiration. He was treated with bark, cod-liver oil, and meat diet. The quantity of sputum decreased from thirty ounces to about half an ounce in the twenty-four hours. The patient gained flesh, and was discharged relieved on January 12th.

He was readmitted on February 5th, when the following further history of the case was compiled by Mr. Adam Young, house-physician:—Last summer he had dysentery, of which he gradually got well. In November he began to be troubled with a cough. One day he felt great pain in the pit of the stomach and between the shoulder-blades; he had been exerting himself a good deal the day before in hauling about sails, &c. Soon afterwards, during the night, he felt a salt taste in his mouth, and on getting a light he found he was spitting blood, "heaving it up in great mouthfuls," about a pint altogether; in color bright red. His complexion is somewhat sallow; says he has not lost much flesh since the commencement of his illness; at present he is fairly nourised; tongue pale, covered slightly with a yellow fur; bowels opened regularly once a day; stools light-colored; appetite pretty good; complains of thirst; has a cough, and expectorates without any difficulty a large quantity of muco-purulent and inodorous bloody sputa; the quantity in twenty-four hours was about a pint; the color resembles brick-dust. On examining the chest dry râles were heard under the left clavicle. Large crepitation over the middle and lower lobes of the right lung behind. The patient complains of frontal headache and pain in the region of the liver, which is increased on pressure and on coughing. The liver appears to be slightly increased in size. Urine: sp. gr. 1010; no bile and no albumen; chlorides abundant.

On Feb. 19th he had an attack of diarrhœa—eleven stools in twenty-four hours; otherwise continues in the same condition, with very little or no constitutional disturbance. Still continues to expectorate the same quantity of sputa, having the same character. He was free from diarrhœa in a few days, but expectorated rather a larger quantity of the "brick-dust" sputa. Pain continues over the region of liver.

27th.—Sputa still the same; he is now beginning to lose flesh. He went on in the same condition for about a fortnight, when the expectoration became less.

On April 7th the sputa were much less, and the pain over the liver diminished. He was now gaining flesh.

He was discharged on the 14th, expectorating only a small quantity in the twenty-four hours. He has no pain, and has gained weight considerably since last note. The diagnosis of this case is not a positive one; but Dr. Ward inclines strongly to the heading given for the following reasons:—(1) The peculiar character of the sputa; (2) the history of dysentery; (3) the absence of dyspnœa and of many physical signs of pulmonary consumption; (4) the speedy rate at which the patient gained flesh and strength when the acute symptoms had subsided.

Should the patient again return, the further progress of the case will be reported.

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WEST LONDON HOSPITAL.

PERICARDITIS WITH EFFUSION; DEATH.

(Under the care of Dr. Thorowgood.)

On April 15th, William W——, aged ten, walked to the hospital with his mother, who stated that the boy had been ill for about a month, but that he had remained in bed only during the last week; no medical aid had been obtained. There was no history of rheumatic or scarlet fever either recently or at any former period.

Patient looks ill and weak; complains of pain in the chest and cough; face pale and puffy; lips slightly blue; pulse 140; weak and regular; temperature 103°. Inspection of the chest shows nothing abnormal. On placing the hand on the præcordial region, a rough grating sensation is imparted to it. On percussion the præcordial dulness is found to extend from the sternal end of the upper border of the second rib on the right side, to a point two inches and a half below and one inch to the outer side of the left nipple, and on the right side in a line half an inch to the right of the sternum. On auscultation a harsh to and fro friction sound is heard all over the præcordial region, most intense at sternal end of fourth left costal cartilage, not changed in position or character by changing the patient's posture. Towards the apex this sound is replaced by a murmur (systolic) of a soft, blowing character, heard all over the lateral and posterior regions of the left side of the chest. Lungs resonant anteriorly, and at the apices; dull at the bases. Loud sonorous râles heard everywhere. There is great tenderness over the liver, which reaches as low as the umbilicus and into the left hypochondrium. He was ordered a mixture containing citrate of potash and nitric ether, three times a day.

April 16th.—Slept a little during the night; tongue moist and clean; pulse 140, weak and regular; temperature 100°; bowels open; urine scanty, contains lithates but no albumen; feet and scrotum œdematous; cough very troublesome; physical signs not altered.

17th.—Patient was kept awake by the cough; pulse 116, very weak, but regular; temperature 99°; tongue moist; œdema of feet, legs, and scrotum increased; dulness at bases of lungs increased; other signs not altered. Ordered Dover's powder, four grains at bedtime.

18th.—After passing a restless night the patient died somewhat suddenly at 10 A.M.

Autopsy, twenty-six hours after death.—The front of the chest on being opened was found entirely occupied by the pericardium, the left lung being quite out of sight, and the anterior border of the right only being visible. The pericardium contained two ounces of turbid fluid, and the heart was very much dilated. The opposing surfaces of the heart and pericardium were covered with rough nodules, and concentric waves of lymph, which could be torn off in thick tough pieces. There were one or two recent adhesions. All the cavities of the heart were dilated, more especially the right. The walls were about their normal thickness. The mitral valve was thickened and contracted; the other valves quite healthy. The right pleura contained fourteen, and the left six ounces of clear serous fluid. Lungs collapsed; congested at the bases. Liver both displaced and enlarged; showed good example of nutmeg liver on section. Kidneys perfectly healthy.

Though the early history of this case was imperfect, still enough was gathered to show that the boy's friends considered he had been ill a month. The state of the heart and pericardium found after death testified to the large amount of disease which had gradually developed during his illness while at home and under no regular medical treatment. It was difficult to believe that the thickened and contracted state of the mitral valve could not be of rheumatic origin, though there was no history of a true attack of rheumatic fever. The treatment, on his admission to the hospital, was directed with a view to overcoming any rheumatic tendency that might be lurking in the system, and at the root of the cardiac and pulmonary disease.

CHARING-CROSS HOSPITAL.

CASE OF "ICHTHYOSIS LINGUÆ."

(Under the care of Mr. FAIRLIE CLARKE.)

"ICHTHYOSIS LINGUÆ" is so rare a disease, its origin is so obscure, its treatment is so unsatisfactory, and its results are often so serious, that every well-marked case which can be noted and recorded may help to throw light on the subject.

W. H——, aged thirty-eight, formerly a soldier, but now a porter, applied at Charing-cross Hospital in October, 1871, on account of the state of his tongue. Fourteen years before he had contracted syphilis in India, and since then had passed through nearly the whole circle of secondary affections. Among other symptoms his tongue became superficially ulcerated, and this has persisted, more or less, for eleven years. He has been a careless, free-living man, and for long he neglected himself entirely. If he applied to a surgeon when his tongue was worse than usual, he discontinued the remedies as soon as he got a little better. In this way the ulceration had crept gradually all over his tongue, and wherever it had been the healthy mucous membrane was replaced by a coating not unlike the rough side of white kid leather. But it had a peculiar silvery appearance, altogether different from ordinary "fur." In some places this coating was thicker than elsewhere, and had quite a corny appearance and hardness. This, as the patient explained, was where the ulceration had been the most severe, and where caustic had been most freely applied by himself as well as by those who had seen him.

When he came to Charing-cross Hospital there was no active ulceration going on, except a very little at the under surface of the tip, and on the inside of the lower lip; but the whole of the dorsum where the white coating extended was painful and tender during mastication, especially after any excess or indiscretion in diet. This state of the tongue seemed to have a prejudicial effect upon his general health, for he complained of various dyspeptic symptoms; but there were no other apparent manifestations of syphilis except the ulcerations that have been mentioned.

With regard to treatment the object of the surgeons should be to secure the maximum of rest, mechanical and physiological. In this case the ulcerated spots were lightly touched with lunar caustic, and iodide of potassium as well as a variety of stomachic and tonic medicines were prescribed, but with no marked effect. The ulcerations improved, but the thick white coating remained much as before. The disease is essentially of a chronic kind, and very intractable; indeed there is much reason to fear that all severe cases of "ichthyosis linguæ" have a tendency to develop into epithelioma. Notable instances of this have been recorded by Sir. James Paget and Mr. Hulke.

The disease consists essentially of an hypertrophy of the epithelial and papillary elements of the mucous membrane. It is, in fact, a warty growth, a papilloma, and it would be better if it were so called, for it is quite distinct in its characters from ichthyosis of the skin. With regard to its etiology, it may be caused by anything which sets up and maintains irritation. Here the source of irritation was superficial syphilitic ulceration. But it is not always due to syphilis. Sometimes carious teeth and persistent toothache may give rise to it. Dr. Andrew

Clark mentions that he has seen more than one case among glass-blowers, dependent apparently upon the peculiar nature of their employment. The sources, therefore, to which it may be traced are various, but they all have this in common, that they keep up a constant irritation of the mucous membrane, and this leads to an overgrowth of some of its structures. When once this overgrowth has reached a certain point it defies treatment, and shows a tendency to advance steadily towards epithelial cancer.

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ROYAL LONDON OPHTHALMIC HOSPITAL, MOORFIELDS.

MELANOTIC TUMOUR OF THE EYE; EXCISION OF THE GLOBE AND APPLICATION OF THE CHLORIDE-OF-ZINC PASTE TO THE TISSUES WITHIN THE ORBIT; RECOVERY.

(Under the care of Mr. George Lawson.)

The chief interest in this cace is the method which Mr. Lawson adopted in applying the chloride-of-zinc paste to the tissues within the orbit after the eye had been removed. His object was to avoid the destruction of the eyelids, a result which generally follows the free application of this caustic to the parts within the orbit. In this he was successful; the chloride of zinc destroyed effectually the contents of the orbit, which came away in large sloughs, but both the eyelids remained intact. The following are the notes of the case:—

J. B——, aged fifty, a tall, healthy-looking man, with no family history of cancer. About four years ago he discovered that the sight of the right eye on the temporal side was lost, and this failure of sight increased until in about six weeks he was quite blind. He then suffered from a severe attack of inflammation of the affected eye, for which he was sent to his county hospital. Under treatment all inflammation ceased, and the eye regained its external normal appearance; but the sight was lost, and with that eye he has continued blind. From that time the eye remained quiet until the present attack, which began eight weeks before he was admitted into the hospital. The eye then became exceedingly painful, the cornea ulcerated, and a vascular growth protruded, from which he had repeated attacks of hæmorrhage which obliged him to seek medical assistance. On April 9th he was admitted into the Ophthalmic Hospital.

On admission, the globe was larger than the other eye, and evidently distended by a growth within it. There were dark bluish-colored bulgings in the ciliary region on the upper and inner side of the eye. The greater part of the cornea was occupied by a vascular, warty-looking growth, from which for the last fortnight there had been frequent hæmorrhages, requiring the man to seek assistance from a surgeon in the neighborhood, who each time succeeded in arresting the bleeding by the use of strong astringent applications.

Mr. Lawson excised the globe, the interior of which was occupied by a melanotic tumour, which had forced its way through the cornea. A pad of lint was applied over the orbit, and allowed to remain for about half an hour, until all the hæmorrhage had become completely arrested. The bandage was then removed, and a speculum having been introduced between the lids, the mouth of the bag of conjunctiva from which the eye had been enucleated was carefully held open with two pairs of forceps whilst Mr. Lawson passed into it the chloride-of-zinc paste, spread on two pieces of lint. The conjunctival edges were now drawn together, and the remaining cavity of the orbit firmly filled up with a number of pieces of dried sponge, over which the lids were closed, and then a compress and bandage applied. A subcutaneous injection of morphia was given in order to avoid the pain which the chloride of zinc often occasions. On the following morning the lids were swollen, but there was no evidence of the chloride of zinc having reached them.

The patient progressed favorably; large sloughs came away from the orbit, but both the upper and lower eyelids entirely escaped the action of the caustic, and the man left the hospital on April 23rd.

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LONDON HOSPITAL.

COMPOUND FRACTURE INTO ANKLE-JOINT, TREATED ANTISEPTICALLY.

(Under the care of Mr. Couper.)

The following case is another witness to the value of the antiseptic treatment in this class of injuries. In a few days the opening into the joint was closed, the fracture became simple, and the man made a good recovery.

On December 14th a sailor, aged thirty-eight, fell from the main deck of a ship (of 1800 tons), into the empty hold, and lay there two hours before anyone came to his assistance. On the following day he was brought to the hospital. A fracture was found extending obliquely through the lower end of the right tibia into the ankle-joint. In front of the joint was a small bruised wound that admitted the little finger. A loose fragment of bone could be felt when the finger was passed into the wound. As the fragment was large and was not displaced, no attempt was made to remove it. The fracture was ascertained to be comminuted by means of a probe introduced between the fragments; the probe could not be passed into the joint owing to the irregular line of the fracture and the wedging together of the fragments, but blood issued from the wound when the ankle-joint was pressed, and it was certain that the fracture ran into the joint. The wound was dressed as follows:—First, the joint was injected with a watery solution of carbolic acid (1 to 19); the joint was not seen to bulge out because it was already much swollen and was full of blood. The tingling and burning sensation experienced in it for some minutes after left no room for doubt that the fluid penetrated the articulation thoroughly. The joint was then enveloped in a number of layers of carbolised muslin, a thin piece of green silk protective being placed between the wound and the gauze to prevent irritation. The limb was placed on a McIntyre's splint, so that the wound could be dressed without disturbing the position of the foot.

The gauze was changed on December 15th, 21st, 27th, and January 6th. At the end of a fortnight nothing remained of the wound except a patch of granulations as large as a sixpence level with the skin. A dressing which had remained in contact with it for a week was only slightly stained with serum on the under surface. On January 6th the wound was all but dry, and the foot was in excellent position. The man's temperature became normal on the fourth day, and has since remained so. He eats and sleeps well, and there is not more swelling or discomfort than is usual in a case of simple fracture. There is every appearance that union has progressed as far as it would have done had the fracture been originally simple.

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SALISBURY INFIRMARY.

CANCER OF THE PECTORALIS MAJOR MUSCLE.

(Under the care of Mr. Coates.)

A. B——, aged fifty-five, was admitted into the infirmary on the 2nd of March. Nine months ago she perceived a small swelling in the left mammary region; and she had a severe darting pain through this swelling. Up to this time she says she had always enjoyed good health. She had ten children, all healthy; the youngest is now twenty-two. There is no history of tumour in her family. On admission there was a tumour, the size of a pullet's egg, in the left mammary region; it is hard and irregular, and seems to move freely over the pectoral muscle under the left breast. This gland is not connected with the tumour. She has lately had a great deal of pain in the side, of a darting character.

On March 13th Mr. Coates operated as follows:—An incision parallel to the lower edge of the pectoralis major muscle exposed the tumour lying partially under the lower edge of the left mamma. On raising the edge of this gland the tumour was seen to originate in the pectoralis major muscle. It, together with a portion of the muscle extending three-quarters of an inch from the tumour, was removed. The breast, being found quite free from disease, was replaced, the vessels secured, and the edges brought together by wire sutures, and styptic colloid applied between the stitches. The tumour was found to be a good specimen of soft cancer.

Mr. Coates stated that cancer originating in a muscle was very rare. He had only seen two cases of it: this one, and another he recorded when a student. The latter was a case of cancer of the muscular structure of the heart as a sequence of epithelioma of the lip.

On the 3rd of April the patient was doing well, making a good recovery. She will be discharged in a few days.

The notes of the above case have been forwarded us by Mr. H. Coates, dresser.

Medical Societies.

JULY—AUGUST.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Mr. T. B. Curling, F.R.S., President, in the Chair.

ON THE RESULTS OF WARM CLIMATES IN THE TREATMENT OF PULMONARY CONSUMPTION, AS EXEMPLIFIED BY AN ANALYSIS OF 251 CASES.

BY C. THEODORE WILLIAMS, M.D., F.R.C.P.,

Physician to the Hospital for Consumption, Brompton.

The author, after dwelling on the difficulties involved in the selection of proper climates for consumptive patients, enumerates three grounds for forming an opinion. First, the alleged immunity from phthisis of certain localities. Second, the existence in certain localities of atmospheric conditions the reverse of those under which the disease was contracted. Third, the ascertained results of certain climates on similar cases. After pointing out objections to the "immunity" ground arising from diversity of the climatic conditions accompanying it, he discusses the second, or "contrast" ground of selection, and in order to explain it, the causation of phthisis is considered and shown to be probably twofold: first, inflammatory—that is, influences which excite or keep up inflammatory affections of the lungs, such as great variations of temperature, a combination of cold and damp, &c.; second, septic—that is, influences which blight and corrupt the bioplasm of the blood or lymphatics, such as foul air, bad nourishment, the combination of warmth and damp, &c.

The differences in the type and distribution of phthisis arising from each of these causes are pointed out as indications in climatic treatment, dryness and warmth being desirable in consumption of inflammatory origin, and dryness and purity of air for consumption of septic origin.

The far greater importance of the third or "fact" ground is then dwelt on, and, after regretting the paucity of published information of this character, and especially of statistics, Dr. Theodore Williams furnishes, from the practice of Dr. C. J. B. Williams and himself, a statistical account of 251 cases of consumption who passed periods varying from one to eleven winters in warm climates out of the United Kingdom.

Sex.—190 were males and 61 females.

Age.—The average age at the onset of the disease was among the males 29·04, and among the females 23·39; nearly half the number of both sexes being attacked between 20 and 30.

Predisposition.—Family predisposition was present in 52 per cent., and hereditary in 27·8 per cent.

Origin.—11 were cases of scrofulous phthisis, in 55 the disease had an inflammatory origin, in 41 a catarrhal, and in 2 a syphilitic; 2 cases followed on asthma, 6 were instances of hæmorrhagic phthisis, and 130 of chronic consumption.

Hæmoptysis existed in 62½ per cent. in varying amounts.

State of lungs.—At the time the patients quitted England 61 per cent. were in the first stage, 21½ per cent. in the second, and 17½ per cent. in the third stage. 67 per cent. had one lung alone affected, 33 per cent. both lungs.

A comparison is then made between these cases taken separately and 1000 cases of which they form a part; and it is shown that in the "climate" patients the disease was more advanced, but at the same time more local in its character.

The climates of which a trial was made were classified as follows:—1. Moist temperate, as Pau, Bigorre, and Rome. 2. Dry climates of the Mediterranean, including the Riviera, Malaga, Algiers, &c. 3. Very dry climates of Africa, including Egypt, Cape of Good Hope, and Natal. 4. Moist Atlantic, as Maderia, the Canaries, and the West Indies. 5. Miscellaneous, including India, the Andes, New Zealand, &c. 6. Sea voyages.

The average of winters passed abroad by each patient was 2¼; and of 18 patients who took voyages, the average number of voyages per patient was 2½.

The results of the climate on the *general* condition of these patients were that 65 per cent. were more or less improved, 6 per cent. remained stationary, and 29 per cent. became worse. The *local* effects on the lungs were, that in 43½ per cent. cure or decrease of the disease took place, in 14 per cent. it remained stationary, and in 42 per cent. it increased either in the way of advance or extension, or of both.

The influence of the various groups of climates is next considered; and from the results of a table it is shown that the *moist* climates, temperate or warm, yielded a percentage of "improved" varying from 50 to 55, of "stationary" varying from 4½ to 14½, and of "worse" from 32 to 45; also that the *dry* climates yielded percentages of "improved" varying from 58 to 65, of "stationary" from 20 to 25, and of "worse" varying from 10 to 21; and that of the patients who took sea voyages, 89 per cent. improved, 5½ per cent. remained stationary, and 5½ per cent. became worse. The marked difference in the effects of the dry and moist groups gives rise to an inquiry as to whether it might not be accounted for by difference in the class of cases sent to each group of localities. This results in the conclusion that, except as regards the Pau cases, which were slightly more unfavorable than the rest, there was no important difference; and the natural inference is, that the less favorable progress of the patients was owing to some element in the moist climates themselves.

The leading meteorological features of the climates of Pau, Rome, and Madeira are then sketched, the effects of each pointed out, and their less favorable results attributed to their moister and less stimulating character.

With reference to the question whether or no certain forms of consumption derive special benefit from any particular climate, the author deduces from 55 cases of phthisis of inflammatory origin who wintered in various warm or temperate localities, that a dry climate is more favorable than a moist one for the treatment of this form of the disease; and as regards phthisis of catarrhal origin the deduction from 41 patients is that "warmth and equability of climate are more important than dryness for patients of this description."

Forty of the climate cases died, and 202 were living at the last report. Among the former the average duration of life was eight years, and among the latter about nine years, which, when compared with the average of life among patients who did not go abroad, showed a slight extension of duration. The effect of cod-liver oil in prolonging life is demonstrated by the instances of 13 patients, who, though they had the full advantages of climate, either omitted oil or took it irregularly. Among these, who are all dead, the average duration was four years eight months and a half.

Dr. Wilson Fox, professing his inability to grapple with the vast array of figures presented to the Society, was fain, by a comparison of the thousand cases brought before the Society some time ago by Dr. Williams with those now before him, to come to the conclusion that climatic results are *nil*, inasmuch as no single climate exhibits a result equal to all the cases quoted collectively. He also referred briefly to the fact that many cases of phthisis, apparently hopeless, live on in London year after year, and asked the author if cases of septic and inflammatory origin can be accurately distinguished, and if so, how.

After some brief complimentary observations by Dr. Dobell,

Dr. Hermann Weber expressed his belief that mountain air most benefited cases of the inflammatory type, and that early hours and quiet habits had also much to do with success of treatment.

Dr. Heywood Smith quoted personal experiences of a winter on the Riviera in proof of the opinion enunciated by Dr. Williams, that a high and dry was better that a moist climate.

Dr. Leared had arrived at the conclusion, from personal experience, that climatic laws with reference to phthisis were empirical, and quoted Iceland as a country in which phthisis is unknown, but where the houses have no ventilation; and Palestine, part of which is below the level of the sea. He asked the author how, during life, the particular variety of phthisis could be accurately determined?

Dr. Sutro and Dr. Douglas Fowell made brief observations, and

Mr. Charles Brooke remarked that there were two distinct climates in Natal, the contrast between the coast-line, in which the rainfall was enormous, and the plateau, being very striking.

Dr. C. J. B. Williams replied to so much of Dr. Wilson Fox's criticisms as referred to the inflammatory, catarrhal, and septic varieties, and remarked that by the inflammatory type were meant those cases that commenced in a distinctly acute attack, whether pneumonic, pleuritic, or pleuro-pneumonic, and subsequently drifted into phthisis. He believed that Madeira was particularly suited to cases of phthisis aggravated by frequent catarrhs; that though statistics on this as on all other medical subjects ought to be received with caution, he could point out many cases benefited by residence in particular places.

Dr. C. Theodore Williams expressed his obligations to the Fellows of the Society for the favorable way in which his paper had been received, and, in reply to Dr. Wilson Fox, stated that the cases now before the Society could not fairly or properly be classed or compared with the thousand cases brought forward last year. In the former the terms "improved," "stationary," "worse," &c., applied to the condition of the patients after a residence abroad of one or more winters; whereas, in the latter, the result was recorded at the end of the entire system of treatment, which averaged about eight years for each case. The statistics showed plainly that life was prolonged by residence abroad, though to a small extent, the average for each patient being 2⅓ winters. He had also proved in a former paper that cases of the inflammatory type had a longer average duration than other varieties of the disease. In reply to Dr. Hermann Weber the speaker remarked that his (Dr. Williams's) experience as to septic and inflammatory varieties had been gleaned in the out-patient department of the Brompton Hospital, from a close study of the predisposing and exciting causes of the disease. Dr. Williams replied briefly to the queries put by other speakers, and remarked in conclusion that, without continuous care and labor for many years on the part of Dr. Williams, his father, the compilation of these statistics would have been impossible.

ON SOME FURTHER ATTEMPTS TO CURE LARGE INTERNAL ANEURISMS.

BY WILLIAM MURRAY, M.D. (NEWCASTLE.)

(*Communicated by* Dr. Symes Thompson.)

This is an account of some attempts to cure internal aneurisms by the insertion of foreign bodies with a view to the formation of clots in the interior of the sac. Some of these bodies were introduced and again withdrawn (needles), and their introduction was repeated at intervals with manifest thickening of the aneurismal walls on each occasion; others were of a material the presence of which would be as harmless as possible (carbolised catgut), and they were used because of the hope that their absorption might take place after the work of coagulation was completed. Lastly, wire was tried, as in Mr. Moore's case, and with more favorable results as regards the preservation of the patient's life, he having lain more than three weeks with twenty-four feet of wire in him. Although none of these efforts were actually successful in curing the patient, they mark out a line of practice which may some day be perfectly safe and successful.

Mr. Holmes thought Dr. Murray's paper useful as tending to show the harmlessness of such an operation, and that surgeons might meddle with aneurisms to a much greater extent than was generally imagined. After remarking that the introduction of carbolised catgut was manifestly useless (because it would soften rapidly) he said that Mr. Moore's case (the preparation is now in the Middlesex Hospital) showed markedly the dangers of the operation, and it was evident that wire or any other material of sufficient hardness to help coagulation when introduced into the sac, must, by rubbing against the walls of the latter, tend to set up a dangerous degree of inflammation.

Dr. Andrew demurred to the wisdom of any surgical interference, inasmuch as external ruptures of the sac are extremely rare, and any modes of procedure such as those described would tend to produce a clot around the strongest instead of the weakest portion of the sac.

PATHOLOGICAL SOCIETY OF LONDON.

Mr. John Hilton, F.R.S., President, in the Chair.

Dr. Sanderson delivered an address—

ON PYÆMIA.

He began by relating his first experiments as to the effect of inoculating the lower animals with pyæmic liquids. In the autumn of 1867 he had injected the purulent liquid contained in the ankle-joint of a patient who had died a few hours before with metastatic abscesses, general suppurative arthritis, and intense septicæmia, under the skin in three animals, a dog and two guinea-pigs. The two guinéa-pigs died within short periods (fifteen and twenty days), and exhibited symptoms of great intensity. Both had metastatic abscesses, but in one of them the lungs were already beset with minute nodules resembling miliary tubercles. The dog lived seven weeks. In this case there were no secondary abscesses, but miliary tubercles of the liver and spleen. From one of the guinea-pigs two others were inoculated, of which one died of pyæmic, subcutaneous abscesses without visceral disease; the other, which lived longer, had no abscesses, but tuberculous disease of the lungs. During the same winter other experiments of the same kind were made, all of which seemed to show that by the inoculation of pyæmic products two sets of lesions might be produced—as an immediate result, metastatic abscesses accompanied by a general typhoid state which was often fatal; as an ulterior result, either disseminated nodules, at first hard but afterwards becoming caseous at their centres, or interstitial induration—both forms of lesion having their seat chiefly in the lungs, spleen, and liver, but also occurring in other viscera.

Having stated these facts, which he said had even in 1868 led him to regard it as probable that the two forms of infective lesions—the tuberculous and the pyæmic—were connected together etiologically and genetically, he referred to another fact which resulted from experiments made in 1871, as to the existence of bacteria in animal liquids, and the circumstances which determine their occurrence. These experiments had shown that whereas bacteria could not be shown to be present either actually or in germ in the healthy liquids or tissues, or in the products of healthy inflammation, they were present potentially in pyæmic liquids—that is to say, that whereas ordinary pus could be kept for days or even weeks free from bacteria, provided the precautions against "spontaneous generation" were observed, pyæmic pus cannot be so kept, and moreover possesses the property of at once determining the development of bacteria in any suitable liquid to which it is added. At that time he had concluded, from insufficient observation, as he now knows, that pyæmic pus did not itself contain visible bacteria.

A short account was next given of certain researches made during last summer in association with Dr. Klein as to the channels by which infective poisons are distributed from their centres of origin. Referring to the last occasion on which he had brought the subject of the intimate pathology of tubercle before the Society, and to the doctrine he had then advocated that tuberculosis is an irritative overgrowth of a pre-existing tissue, he said it had then been shown that the process, whether in its disseminated or interstitial form, has its seat in a certain tissue, and this tissue had been termed adenoid or lymphatic, both words implying its intimate and special relation with the lymphatic system; but the precise anatomical nature of this relation had been imperfectly made out. No further progress was made till last May, when Dr. Klein came to England with the distinct object before him of co-operating in the investigation of this very question. The field taken up was the peritoneum—the reason of the choice being that that membrane, and especially the omentum and diaphragm, had already been the subject of investigation as favorite seats of tuberculosis. These researches have not merely served to elucidate one or two anatomical facts of very great importance to the pathologist—*e.g.*, the existence of a lymphatic system in the omentum, and its distribution, and the mode in which the peritoneum communicates with the lymphatic system but have rendered it possible to give an account which, so far as the peritoneum is concerned, is tolerably exact and complete, both of the normal process of absorption and of the changes which the absorbing tissues undergo when they are entered by infective agents.

In the course of these experiments it was found that not only as regards the property of any given peritonitis to assume the infective character, but as regards the intensity of the infective results and their duration, there were endless varieties. In one set of cases the secondary lesions were suppurative, the constitutional disturbance intense, and the fatal result rapid; in another the lesions were vascular new growth, firm at first, afterwards becoming caseous, the progress slow and the functional disturbance imperceptible. And then it appeared that in all those instances in which the pyæmia—that is, the acute character—manifested itself bacteria were present, not merely in the purulent fluids but in the blood. Under these circumstances attention was directed from the effects to the poison itself. Soon after the opening of the Brown Institution, it was found that the practice of the hospital for animals was likely to afford the required material—in short, that pyæmia occurred in dogs under circumstances very similar to those which determine it in human beings and exhibited similar symptomatical and pathological aspects. A series of experiments were therefore commenced in January last, having for their object to acquire a knowledge of the morbid poison, and particularly to discover by what conditions the variations of its intensity were governed. With reference to these experiments Dr. Sanderson would not anticipate the complete account of them in the report of the Medical Officer of the Privy Council, but would confine himself to giving an account of one series, and exhibiting the action of the pyæmic poison during life, and the post-mortem appearances. But before doing so he would state shortly what he understood to be the signification of the term "pyæmia". He then proceeded to say: "The word pyæmia is apt to be used in somewhat different senses, according as the person using it has before him the medical or surgical aspect of the disease. To define it completely we must, I think, take into account its mode of origin, its symptoms, and the anatomical changes which it produces, not confining our attention to either of these to the exclusion of the rest. With this consideration in view I would comprehend in my definition the following propositions:—

"1. Pyæmia originates by the introduction into the living tissues, and eventually in the blood, of a poison which is itself a product of inflammation. 2. The action of the poison manifests itself in an alteration of the blood, and in disorder of the vital functions. The former of these is characterised by the presence of bacteria, and by change in the optical characters of the blood, which often becomes obviously more transparent and darker by reflected light than it is naturally. Of the latter—viz., the general disorders of the vital functions—the most prominent phenomenon is fever, which in the more intense forms of the affection is followed by collapse, which culminates in death. 3. More remotely the disease manifests itself in secondary suppurations—*i.e.*, in the formation of metastatic abscesses, —which may occur either in the internal organs or underneath the skin. The special character of these metastatic (as I am in the habit of calling them) infective abscesses are those which are well known both to surgeons and physicians. They have the additional less-known character, that the pus they contain is full of bacteria. 4. Pyæmia differs from tuberculosis in the rapidity of its progress, and in the obvious character of the anatomical changes of which it consists. Whereas by tuberculosis we are understood to mean anatomically the overgrowth of cells in certain tissues which we designate lymphatic on account of their proved anatomical relation to the lymphatic system, the secondary inflammations of pyæmia result in the formation of infective abscesses. 5. Pyæmia resembles tuberculosis in its mode of origin. Both spring from inflammations, and, so far as relates to the anatomical characters of the lesions, both are inflammations. To both, therefore, the term secondary or infective inflammation is applicable.

"So much for the disease itself. Let me now," said Dr. Sanderson, "draw your attention to the nature of the poison. I wish to show, first, that every pyæmic abscess contains a poison, which, when introduced either into the circulation or into a serous cavity, produces the symptoms of pyæmia; and secondly, that we have this poison so entirely in our possession, and so far under control, that, beginning with an agent so mild in its action that it produces no marked symptoms, we can convert it into an agent of such intensity that it kills in two or three hours with the formidable symptoms seen in the case we have now before us. This intensification is effected by a process which may be called cultivation. Dr. Klein made the important discovery, that if a pyæmic liquid was transferred to the peritoneum of a guinea-pig, and allowed to remain there for a couple of days, although it did not at first produce any intense symptoms in the animal itself, its toxic intensity increased in such a degree that when the transudation liquid produced in this way was injected into another animal it had acquired the most deadly activity; and that all such extremely active liquids were crowded with bacteria of a particular character, the increased number of which seemed to be in proportion to their toxic properties.

Dr. Sanderson then proceeded to exhibit a dog, into the abdominal cavity of which six drops of a pyæmic transudation liquid had been injected three hours before. The animal was in a state of profound collapse, accompanied with vomiting, purging, and cramps of the extremities. Shortly afterwards the animal was killed, and the abdominal cavity opened. The peritoneum contained liquid slightly stained with blood, which on microscopical examination was found to be crowded with bacteria. The intestines were distended with a frothy liquid that possessed none of the characters of the natural contents which had been found in other cases to be charged with shed epithelium. The internal suface on the whole of the alimentary canal, from the stomach downwards, was intensely injected, and presented appearances which (as had been found by more careful investigation in previous cases) were due to the separation of the epithelium from the surface of the mucosa, and the infiltration of that tissue with liquid.

The material which obtained these results was obtained as follows:—Pus from a pyæmic abscess of spontaneous (i.e., accidental) origin was introduced into the peritoneal cavity of a guinea-pig, and allowed to remain there for two days. It was then withdrawn from the guinea-pig, and some of it at once injected into the peritoneum of a dog. The dog was effected in exactly the same way as the animal exhibited to the Society. The remainder of the liquid was kept for five weeks in hermetically sealed tubes, after which six drops were injected in the peritoneum of a guinea-pig, which showed its action to have become relatively feeble. After two days the transudation liquid produced was (the day before) tested with a third guinea-pig, and found to be extremely active. This afternoon it was injected into the peritoneum of the dog exhibited.

Dr. Sanderson then concluded by saying: "Such are the facts. The all-important question remains, Do these experiments concern us as physicians and surgeons, or not? I think they do. But what I want is to prove it; for I am well aware that unless clinical observation comes in aid of pathological experiment, the result of the latter do not tell practically. Let me state what are the lines of inquiry I desire to see taken up. The first question is—Do the characters which we have shown to be present in the products of acute secondary inflammation in the lower animals also exist in similar products in man? The second is more important still—Can it be shown that human pyæmic products, when tested by inoculation, possess exactly the same morbific properties as those which are possessed by the liquids to which our experiments relate? It is for these inquiries that I earnestly ask the assistance of hospital surgeons.

"Finally I would say a word as to the limits of the question now before us. With regard particularly to the question of bacteria, I desire to keep to the bare facts of disease, and not to diverge into discussion as to their origin. It is a matter to me of comparative indifference how they originate. Our observations lead us to conclude—first, that they afford a *characteristic* by which we may distinguish the products of infective inflammation from those which are not infective, and that their number affords an indication of the degree of infectiveness; and, secondly, that their presence in the blood is an indication of that constitutional disturbance which accompanies infective inflammation, not merely when that disturbance assumes the degree of intensity of which we have an example before us, but in the slighter form of irritative fever, If these facts prove to be true, not only in the lower animals, but in man, their importance is quite unaffected by any theory we may entertain as to the origin of bacteria."

The President said the subject was one of great importance. Though it might not be possible to assent to all that Dr. Sanderson had said, yet the experiments were most interesting. The effect produced upon the dog by the injection of five or six drops of the peritoneal fluid was very remarkable.

Dr. Crisp said he differed from Dr. Sanderson in several of his conclusions. He saw little or no resemblance between pyæmia and tuberculosis; one was highly infectious and the other not communicable. There was a great difference between tubercle in man and that in the lower animals. Pyæmia in the lower animals was of rare occurrence. He did not think bacteria were the cause of pyæmia, they were found in many diseases of the lower animals; also, as he had first pointed out in splenic apoplexy, they were probably the effect. He thought the irritating matter used by Dr. Sanderson acted more like a poison, such as prussic acid, &c., causing death in a definite time.

Dr. Bastian said the facts brought forward by Dr. Sanderson were most important. With the exception of one or two rare diseases, there was no case on record where bacteria were found in the blood of man during life. Did Dr. Sanderson see in the blood of pyæmic animals well-formed bacteria, or only granules? Then what relation did bacteria bear to pyæmia? Were they to be considered as the cause or the consequence? Again, admitting that bacteria exist in the blood in pyæmia, and are the cause of it, how did the contact of bacteria incite the pyæmic process? Bacteria were constantly in contact with open wounds and mucous membranes; they were to be found in the mouth and back of the throat of every individual. If they were the cause of pyæmia, how was it any lived? Then fluids containing bacteria had been injected into animals, and no disease had resulted; he had often done so with frogs. It was rare to find bacteria in the blood of patients dying of typhoid fever or acute rheumatism, yet in both these diseases where the temperature had been high they had been found a few hours after death. Why were bacteria found in these cases? If bacteria were essential to pyæmia, how was infection brought about.

Mr. Hulke asked if Dr. Sanderson used the terms "pyæmia" and "septicæmia" as synonymous. He would draw a wide distinction. He considered pyæmia the result of the introduction of pus into the vessels; septicæmia, of some putrescent matter. He thought the dog on the table died of septicæmia, not pyæmia. If perfectly filtered healthy pus were injected into an animal, the symptoms of pyæmia simply resulted; if unfiltered, the same way symptoms *plus* multiple abscesses, &c.—i.e., septicæmia.

Mr. Spencer Wells asked if a physiological poison was always the cause. Might it not be a chemical one? He mentioned a case in which the peritoneal fluid was taken, and an alkaloid prepared, one milligramme of which killed a rabbit. The rabbit had effusion into the peritoneal cavity. Some of this was taken and injected into a second rabbit, which died; and fluid from a second killed a third; and fluid from the third killed a fourth; but the fifth recovered. This was a chemical poison, and no bacteria were there.

Dr. C. J. B. Williams said that, from the statements made, he could come to no conclusion whether bacteria were a cause or a consequence. He thought many poisons were developed in disease as the result of decomposition.

Dr. Murchison asked if pyæmia could originate in the body independently of poison from without. He had often examined the bodies of patients dying of pyæmia after typhus fever, on whom there had been no bedsores or any ulcerated surface, yet evidence existed of intense pyæmia—pus in joints, under the skin, and abscesses in internal organs. Some epidemics of typhus were characterised by sequence of pyæmia, and when one case had occurred a number followed, perhaps from bad hygiene.

Mr. Henry Lee thought Dr. Sanderson had shown that the products from simple inflammation differed from those originating from a putrid element; the latter was much more fatal than an inflammation spontaneously originating.

Dr. Anstie said he had met cases of pyæmia arising without external wound, and apparently without the introduction of any putrid element. One class of cases was the result of cold, no other cause being assignable; a second class where there had been exposure to bad smells, drains, &c. Of this latter he mentioned a case which he had seen at the Westminster Hospital.

Dr. Cayley asked what became of the animal into whose peritoneal cavity the pus had been injected and remained for forty-eight hours? Could the fluid removed be the same as that injected?

Mr. Hulke said that a chemical substance—septicin—had been long ago separated from purulent fluids by German chemists.

Dr. Sanderson shortly replied that, as regarded bacteria in general, he was well aware, from his own experiments, that the ordinary bacteria of putrefaction possessed no toxic action; that fluids containing them could be injected into the circulation of animals without result. As regarded the bacteria of pyæmic products, he had carefully guarded against the inference that they were the efficient cause of pyæmia. He regarded them as characteristic inhabitants of infective fluids, and therefore probably the carriers of infection. As respected the term "septicæmia," he understood it to mean a state of the blood which was only present in the most intense forms of pyæmia. He quite agreed with Mr. Hulke in regarding metastatic abscesses as an accident rather than an essential of pyæmic infection. The theory that the poison was of the nature of an alkaloid was set aside by experiments on the diffusibility of the pyæmic poison; it cannot be diffused.

A vote of thanks was given to Dr. Sanderson for his observations.

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CLINICAL SOCIETY OF LONDON.

Sir William Gull, Bart., M.D., F.R.S., President, in the Chair.

Dr. Greenhow related the history of a case of Diphtheria which was followed by Paralysis. The patient, a brass-finisher, aged thirty-two, was admitted into the Middlesex Hospital on May 27th, 1871, with all the well-marked symptoms of diphtheria. Under treatment he so far recovered that, at his own desire, he left the hospital on June 27th. His voice had still a nasal twang, and he had difficulty in swallowing solids. The left side of the soft palate acted imperfectly, and he had some asthenopia, so that he was only able to read for a few minutes at a time. On August 9th, he was readmitted into the hospital with symptoms of paralysis rapidly coming on. He had the sensation of pins and needles up the legs as high as the knees, and in the hands and arms up to the elbows. For a time the loss of sensation and power increased until he could scarcely grasp with his hands or walk without assistance. The two symptoms, however, which gave special interest to this case were the slowness of pulse (which only set in as the throat symptoms began to subside, the pulse falling as low as fifty-six), and a remarkable tenderness on pressure of the large nerves of the extremities. Firm pressure on the ulnar, radial, and crural nerves produced great pain, sufficient to make the man call out and to cause his face to flinch. The patient ultimately recovered, and was discharged from the hospital in the following October.

Dr. Broadbent remarked on the importance of the paper, inasmuch as the pathology of these disorders was exceedingly obscure.

Mr. Timothy Holmes exhibited a patient from whom he had excised the whole of the second metatarsal bone, and afterwards the whole row of the tarso-metatarsal joints, the latter operation being undertaken as a substitute for Lisfranc's amputation. The original disease in the second metatarsal bone depended on a traumatic cause; and the affection of the tarso-metatarsal joints was due probably to violence done in removing the head of that bone from the box in which it is embedded between the cuneiform bones. The man recovered, and the foot, though hardly yet quite sound, promises to be perfectly useful. The case was exhibited partly as an interesting example of a probably unique operation, but chiefly to show the success with which excision may be substituted for amputation in affections of the bones of the foot due to traumatic causes.

Dr. Dyce Duckworth exhibited three patients suffering from Molluscum Contagiosum, which had occurred in members of the same family; thus affording strong evidence of the contagious nature of the disease. The tumours began three years ago in the first child of a healthy women; the mother was next affected about the face and breast; and the second child presented numerous tumours shortly after birth. The first child was taken to St. Bartholomew's Hospital, the tumours were removed, and no more subsequently appeared. Lastly, the grandmother, who had slept with the eldest child, and had often nursed the baby, became affected. Dr. Duckworth wished to glean opinions from dermatologists as to the contagious nature of the disease, inasmuch as in many cases no history of contagion could be obtained. The pathological anatomy of the tumours in these cases had been thoroughly worked out.

Dr. Tilbury Fox exhibited a patient, aged eleven, suffering from tubercular leprosy, in whom a marked amelioration had been induced by the use of large and continued doses of quinine. Dr. Fox brought the case forward as illustrating the fact that leprosy could be greatly relieved by proper treatment, and not merely by the Beaupurthey and Bhau Dji plans recently extolled. This was the fourth case which Dr. Tilbury Fox had treated recently in a similar manner. The child has a well-marked leonine expression of countenance, caused by thickening from deposit of leprous granulative tissue in the skin of various parts of the face. There was also much discoloration of the whole skin, with a few anæsthetic spots about the conjunctiva and the mucous membrane of the palate, &c. The general thickening, however, of the integuments of the face and body had diminished, the voice was clear, the snuffling from thickening of the nasal mucous surface had diminished, and altogether the child had greatly improved. The oil of cashew had been applied for the first time a few days before the meeting, in order that the members might see its action. Dr. Tilbury Fox stated that not only the oil of cashew, but carbolic acid and arsenious acid, used locally, dispersed the tubercular formations; but the oil of cashew in his hands had been very successful in this respect, though it required to be used with care.

Dr. Anstie exhibited a case of Lepra Anæsthetica. The patient was a gentleman aged twenty-nine, who had been exposed to malaria, want of food, bad lodging, and other hardships in India. The symptoms were—(1) the usual eruption of patches —brown at the edges, white, shiny, and anæsthetic in the centre—on both arms and on one leg; (2) wasting of certain muscles; (3) great nervous and mental depression. The case was not one of the most severe grade: still the prognosis was gloomy. Under quinine, cod-liver oil, and faradisation of the affected skin and muscles, a great improvement took place, and he was able to return to his occupation. Dr. Anstie said this case reminded him of three others which were curiously analogous to it, and which he detailed. One of them had been brought before the society by Dr. Buzzard. The general inference which he was inclined to draw was, that leprosy was by no means so perfectly and typically isolated from other diseases as some persons supposed. Probably it would be found hereafter that leprosy might be developed from a great variety of factors, of which only one (possibly a hereditary degenerative tendency in the central nervous system, closely allied to that which produces Cruveilhier's disease) was constant and indispensable, and privation of food was the next most common.

Dr. Tilbury Fox had noted the contagious nature of molluscum contagiosum some years ago, in a woman who was affected with it from the forehead of a child while suckling. He mentioned the fact that seven children were brought to him all suffering from the same disorder, which had commenced in all at one and the same time.

The Chairman remarked that the nomenclature of leprous diseases was very imperfect; that all tuberculated cases had anæsthesia, and that hence the Committee appointed by the College of Physicians divided the cases into the tuberculated, and non-tuberculated varieties. He would like to know if any member had ever seen a tuberculated case without anæsthesia?

Mr. Brudennell Carter asked if it was true that the Committee referred to had reported a case not to be leprosy because muscular wasting existed. He had seen many such cases in the hospital at Christiania, and believed that muscular wasting was a characteristic phenomenon of the disease.

Dr. Buzzard said that his patient, who was drowned some weeks ago, had the right median nerve much thickened. The case during life was declared by two experienced authorities to be identical with Oriental leprosy, but the patient had never been out of London. According to his own belief all evidence as yet adduced as to so-called Oriental leprosy went to prove that it was a condition of mal-nutrition not peculiar to any particular climate or locality. It prevailed largely in England between the tenth and sixteenth centuries, as evidenced by the fact that no less than three lazar- or leper-houses existed at that period in this country. He believed that the disease still lingered amongst us in so slight a form as to be easily overlooked, being represented perhaps by a slight discoloration of skin, or an unexplained localised atrophy of muscular tissue.

Dr. Tilbury Fox had no doubt whatever that Dr. Anstie's case was one of genuine leprosy. He remarked that he had never seen tubercular leprosy without anæsthesia.

The Chairman remarked that no doubt was now raised as to the existence of genuine leprosy in England.

Dr. Burney Yeo read a paper on a case of Paralysis of the Senses of Taste and Smell following Concussion of the Brain. The patient, a man aged fifty, came to King's College Hospital on January 11th, having four months before been thrown out of a cart. He was admitted into St. Thomas's Hospital in a semi-conscious state, under the care of Mr. Croft. A slight contusion was discovered on the back of the head, but there was no fractured bone. He vomited, was delirious and insensible during the night, and in the morning complained of much pain in the head, which was relieved by aperients. He soon recovered, and, feeling well, discharged himself after he had been in the hospital about a week. He found, however, that he could neither taste nor smell. This condition had persisted ever since the accident. He had suffered also a good deal from giddiness, and from severe pains on the right side of the head. He had been a perfectly healthy man, and quite free from any syphilitic taint. The patient's statements were thoroughly tested. Salt, sugar, solution of quinine, dilute acids, tincture of assafœtida, were applied to the tip, the centre, and the back part of the tongue, to the under as well as the upper surface, but were not recognised in any degree. The tongue, however, was sensitive to the slightest touch, and its movements were perfect. The sense of smell, similarly tested, was found to be absent. The patient was ordered five grains of iodide of potassium three times a day. The first doses produced such violent symptoms of iodism that it could not be continued. The dose was therefore reduced to two grains; and as this also acted in the same way, only one grain three times a day was given. This dose, at first, produced iodism, and afterwards purged him; he, however, continued to take it. In a few days the sense of taste began to return. On January 25th he could taste solution of quinine as bitter, and tincture of assafœtida as bitter; but he could detect no odor in the latter. He could distinguish salt from sugar, but not tea from coffee. The sense of taste continued to return rapidly. On Feb. 11th he was able to taste the flavor of meat for the first time for twenty weeks. He could also now perceive strong odors, but smell came back less rapidly and less perfectly than taste. In connexion with the return of smell, he complained of a curious subjective affection of this sense; a foul smell comes into his nose like that of a stale tobacco-pipe, and it seems to come down from the head. By the 20th

March the patient was quite well. Dr. Yeo thought this case offered many points of clinical and physiological interest. He thought it a somewhat rare circumstance to find the senses of taste and smell clearly absent together without any other affection of the nervous system. The absence of taste could not, in this case, be simply the consequence of loss of smell, as solution of quinine and tincture of assafœtida were, at first, not distinguishable from water; and when the latter was first recognised it was as a bitter substance, and its peculiar flavor was not perceived; moreover, the sense of smell returned much later than the sense of taste. It was difficult to say what could be the lesion which would produce paralysis of these two special nerves and of no others. This difficulty was increased rather than diminished by the very remarkable manner in which the administration of iodide of potassium had been followed by the recovery of both senses.

Dr. Lockhart Clarke thought that the condition depended on shock, and had seen several cases originating in this way. He did not think that any lesion existed, and thought that cure was produced by stimulation of the mucous membrane. He related the case of a child who from standing on its head produced symptoms of concussion, and of hyperæsthesia of the leg except the parts about the knee. The patient had suffered from chorea, and had been confined to bed for four months, but by local treatment the limb recovered in six weeks.

Dr. Hughlings Jackson thought the case quoted very puzzling, and had never seen one in which both taste and smell were completely lost, although a patient would declare that he had lost the sense of taste, when examination proved that the smell alone was gone. The subjective sensations complained of by Dr. Yeo's patient were very important, and occurred as a so-called aura in some epileptiform seizures, especially in those cases in which consciousness was lost without convulsions. The speaker believed that in these cases there was disease in the neighborhood of the anterior cerebral artery, and that the subjective sensations augured ill as to the amount of mental failure.

Dr. Broadbent had seen two cases in which incomplete loss of smell and taste had become permanent, and took exception to the statement that there was total loss of taste in Dr. Yeo's patient.

Dr. Buzzard, referring to a case related by Dr. Anstie, in which neuralgia of the fifth nerve was accompanied by loss of smell and taste, suggested that in Dr. Yeo's case a lesion of the trigeminal nerve had caused the loss of these two special senses, the integrity of that nerve being apparently as necessary for the proper action of the olfactory lobes as for the perfection of the sense of taste.

Mr. Christopher Heath had seen several cases in which taste and smell were gone, and spoke of a lady who, as the result of an accident, lost both taste and smell.

The President thought such cases not rare, and believed that it was very difficult to separate taste, touch, and smell, and that there may have been in this case shock to the sympathetic centres in the neck, and so to the nerves of taste and smell.

Dr. Lockhart Clarke referred to a case of locomotor ataxy, in which both taste and smell were lost.

Dr. Greenhow then read notes of a case of Progressive Muscular Atrophy.

Dr. Baumler communicated a case of Enteritis, illustrating the dangerous effects which in cases of this kind may follow the use of the mildest purgative even after all acute symptoms have subsided. The patient, a young man aged twenty-two, was taken ill, after some irregularity of diet, with symptoms threatening peritonitis. There was great pain in, and extreme tenderness of, the abdomen, with some distension, besides high pyrexia. With rest and opium these symptoms subsided in a few days; and on the sixth day of illness a furred state of the tongue, slight tenderness in the cæcal region, and constipation, were the only abnormal symptoms remaining; pulse and temperature being quite normal, and the appetite returning. On the following day, the bowels not having acted for five days, and the descending colon being filled with solid fæces, half an ounce of castor oil was given. No sooner had this commenced acting than all the previous symptoms returned, the temperature again exceeding 102° Fahr., and the patient for some days being in a somewhat critical state. Gradually, however, the symptoms subsided. On the seventh day of this relapse the pyrexia declined, and the bowels, which had not been open since the action of the castor oil, moved spontaneously. From that day the recovery was uninterrupted.

Editorial.

THE THERAPEUUICS OF TUBERCLE.

We have great reason to fear lest in regard to the treatment of tubercular disease we act too much on the motto of "Rest and be thankful." This motto is a dangerous one either for politicians or physicians. But it is not the less seductive on that account. The treatment of phthisis has so strikingly improved, and the results are so much more satisfactory than they once were, that we are apt to think that no more remains to be done. One physician after another tells us that in this, its commonest form, tubercle is ever so much more amenable to treatment than it used to be. We are apt, indeed, to speak as if the very idea of curing consumption on the part of physicians dated from the introduction of cod-liver oil. It may check our boasting to remember that the idea of curing consumption is very much older. To go no further back than the beginning of this century, Dr. Beddoes confidently thought he cured phthisis with digitalis. "Were I to affirm," he said, "that it cures three out of five cases of consumption, I think I should keep within the limits of the truth." Nobody now believes that digitalis will cure consumption; but we ought to remember that the idea of curing this disease was not unknown among writers fifty years ago. Not only so; the curability of the disease was suggested to physicians not so much by the action of medicine and observations on the living as by the post-mortem examinations of pathologists, who observed in persons that had died of other maladies the dried remains of deposits and the scars of contracted cavities in the lungs. Dr. James Henry Bennet, in the late edition of his book on Pulmonary Consumption, tells us that in 1840 he wrote a paper on the Curability of Consumption, based on observations made in the dead-room of the Salpêtrière—a large asylum hospital devoted chiefly to aged and infirm women. In many of these women who had died of other diseases, in advanced life, he found large cretaceous deposits and puckered cartilaginous cicatrices—proofs of long previous phthisical disease, which had undergone spontaneous cure, in spite of what we must now regard as a bad kind of treatment. We are all familiar with the published observations of Dr. Hughes Bennet on this subject, which more than anything else have in this country familiarised medical men with the idea of phthisis being cured or arrested. He concluded that the spontaneous arrestment of tubercle in its early stage occurred in the proportion of from one-third to one-half of all the individuals who die after the age of forty; and he quotes the observation of Roger and Boudet, made at the Salpêtrière and Bicêtre Hospitals of Paris, amongst individuals above the age of seventy years, as showing the proportion in such persons to be respectively one-half and four-fifths.

These observations, it will be seen, refer to a period when cod-liver oil had not come into such use as to be a factor in the case. Dr. Hughes Bennett's monograph on Cod-liver Oil—based chiefly on what he had seen of the use of the oil in scrofulous and tuberculous diseases in Germany—it is true was written thirty years ago (in 1841). But it clearly could have nothing to do with the healed cicatrices noticed by Dr. Henry Bennet in 1840 and the French physicians under whom he studied, and it could have very little more to do with the same post-mortem appearances noted by Dr. Hughes Bennett and commented on by him with so much force and effect in 1845.

We should be sorry to seem to say a word in disparagement of the present treatment of phthisis as set forth by all authoritative writers at the present day. Our present object is rather to indicate what we have gained by it, and, secondly, to point out some respects in which it is unpractical. Clearly, then, from what we have said, we must not attribute all arrest of tubercular disease to treatment, which we have been rather apt to do; for such arrest occurred frequently in a large proportion of the population dying after forty before cod-liver oil was used. But it is equally beyond doubt that the present treatment of phthisis is beneficial. The life of phthisical patients is lengthened and a virtual cure is obtained in a larger number of cases than formerly. We must accept the evidence of plain facts on this point, whether the clinical records of such practitioners as Dr. Williams and Dr. Henry Bennet, or the obvious effects of treatment witnessed by practitioners daily in increasing appetite, weight, and strength, and in diminishing cough, perspirations, expectoration, and waste.

There is one element of treatment much urged by all the specialists on this subject which, to repeat our own word, is unpractical. We mean the climatic element. We have no doubt as to the value of climate as either positively beneficial or as enabling the patient to live more continuously in the open air, which, after all, is the most powerful antiphthisical influence, excepting perhaps good food. But the prescription of another climate to the bulk of patients is merely the prescription of the impracticable. We want, then, to know more of the possibility of curing phthisis under the conditions of our own fickle and somewhat rough climate. Dr. Henry Bennet's experience of phthisis, as controlled by all the comfortable conditions of life in the Riviera, is very striking; but it is only tantalising to people that cannot betake themselves to Mentone, but must stay in England. Dr. Williams's large experience of the quadrupled life of phthisical patients now as compared with former times is liable to the same criticism—that it applies for the most part to comfortable people, so comfortable that they could give up work and reside abroad for one or more winters. A remedy like this is simply no remedy to the majority of people. Hence we urge upon physicians dissatisfaction with themselves till they have shown the profession how to control phthisis at home. It is surprising to what an extent this is really done already, as may be witnessed in the out-patient practice of our hospitals and dispensaries, and in patients living in anything but hygienic conditions. Doubtless good air, good food, cold sponging, not too much exercise or activity, sleep, and all strengthening things, habits and conditions, *plus* cod-liver oil, are, as Dr. Henry Bennet says, the principal curative agents. But we are sorely in need of means of getting such influences to bear upon the mass of patients; and it is still a desideratum in medicine to know how best to treat phthisis successfully among the not rich. Clearly they are often the subjects of arrested phthisis. What we want is to define more exactly the way in which cases of such arrest may be multiplied.

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MUSCULAR IRRITABILITY.

A VERY considerable body of evidence favoring the old view of Haller of the existence of a *vis insita* in muscles has been gradually accumulating during the last few years, though it is scarcely more than alluded to in recent physiological works. Part of this evidence has been obtained from the observations of practical physiologists, and part from those of pathologists who are principally engaged in the treatment of paralysis by means of electricity. That the muscles will retain their irritability after section of the nerves distributed to them for a long period, provided they still remain *in situ* or otherwise receive a due supply of blood, has long been known. Longet showed that when the facial nerve was divided the distal extremity lost its power of conducting impressions in four days, whilst the muscles to which it is distributed preserved their contractility for three months; and M. Brown-Séquard found that even after avulsion of the facial nerve at its root the facial muscles remained irritable for no less a period than twenty-one months, whilst for the first week after the operation there was a notable increase in their contractility. Both of these physiologists, however, found that after section of a mixed nerve like the sciatic, instead of a purely motor one like the facial, the contractility gradually though slowly diminished, with concomitant fatty degeneration of the fibres, so that after two months the muscular substance had almost disappeared; those fibres, however, that remained unchanged in physical characters still retaining their functional activity, and responding to a strong galvanic stimulus. The observations of M. Duchenne, which have been repeated and corroborated by many other pathologists, seemed at first sight to be opposed to the statements of Longet and Brown-Séquard, for he found that in many if not in all cases of lesion of nerves, whether traumatic or rheumatismal, and whether affecting the purely motor or the mixed nerves, the contractility of the muscles—in man, at least—is greatly diminished, if not altogether abolished, in the course of *a few days*. This was observed both in cases where the interrupted current was applied to the surface, and where by means of the apparatus for electro-puncture the current was directly applied to the muscles. The difference between the pathologists and the physiologists must, it is natural to suppose, be due to some difference in the conditions under which their several observations were made; and it is probably in part explicable by the circumstance that the experiments of the former were made upon the muscles through the whole thickness of the skin by means of moist conductors, or with the implantation of needles only; whilst in those of the latter the muscles were fairly exposed. But the careful experiments of M. Erb have shown that it is essentially due to the nature of the stimulus applied. As the result of his numerous researches, he found that towards the end of the first week, after the violent pinching or section of a nerve, the excitability of the muscles supplied by it diminished both for induced and for continuous currents. At the expiration of this period a marked difference occurred in the action of the two currents. The induced or interrupted currents became less and less powerful as excitors, though never altogether disappearing; whilst the effect of the application of the continuous currents became stronger and stronger, till at length the current derived from a single pair of plates produced contractions which could only be equalled by a current of double or treble the same strength in the normal condition. The duration of this period of augmented excitability for galvanic currents is, in man at least, not indefinite; for if the nerve recovers its conductivity, it disappears in the course of about three months, the sensibility of the muscle to faradaic electricity gradually returning. Independently of these changes of the contractility of muscles in regard to the different kinds of electrical current, it is found that mechanical excitations act more energetically on those that have been paralysed by the division of their nerves, and the act of contraction is slow, resembling that of the smooth muscles. Researches of a similar nature to those of M. Erb have been undertaken by MM. Ziemssen and Weiss, with the same results, except that in their experiments the excitability of the muscles to the stimulus of faradaic electricity was found to diminish earlier even than was supposed, being usually perceptible and sometimes complete in the course of the first day, when tested through the shaved and moistened but uninjured skin; the diminution appeared to supervene more slowly in proportion as the nerve was injured nearer to the centre from which it rose, and to be of longer duration in proportion to the severity of the nerve lesion. That these are essentially accurate statements is shown by the fact that they have been repeated by MM. Omnius and Legros, and still more recently by M. Vulpian, who has given a masterly analysis of the whole subject in M. Brown-Séquard's *Archives*, and who promises to add some important observations of his own. The fact remains that the muscles respond to the direct action of continuous and, though less perfectly, of interrupted currents of electricity long after the division of their nerves, and certainly long after these have ceased to be able to conduct impressions. It is therefore reasonable to conclude that the contractility of muscle is a property inherent to the tissue itself.

Medical Annotations.

"Ne quid nimis."

DR. PETTIGREW ON CIRCULATION.

DR. PETTIGREW will add to his already considerable reputation by his lecture delivered before the Edinburgh College of Surgeons on Friday, the 3rd

May, on the "Physiology of the Circulation in Plants, in the Lower Animals, and in Man." Starting from the thesis that the circulation in plants and animals may be regarded as so many links capable of being combined into a chain which in its ultimate and perfect form represents the circulation as found in the higher mammalia and in man himself, he proceeded to show that plants and the lowest forms of animals have neither vessels nor hearts for propelling the nutrient juices; that in animals somewhat higher in organisation, vessels appear which are capable of contracting and expanding so as to force their contents in a given direction; that, higher still in the scale, the vessels are provided with a single heart—a single pulsatile cavity; and that these hearts or cavities increase to two in the fish, to three in the serpent, and to four in the bird and mammal. Concurrently with this differentiation or increase in the heart-cavities, there appear valves, sluices, or floodgates, which determine the course of the blood by causing it to flow in certain directions, "just as sluices can be made to let water out but prevent any from getting in." Whenever and wherever circulation exists, its object is to give to and take from the tissues—to build up and to demolish; this "giving to" and "taking from" necessitating two or more currents running in directions diametrically opposite. In plants and in the lower animals the currents are interrupted or disjointed; while in the higher animals they are united so as to form a circle, the heart being situated on one part of the circle, the lungs on another, and the tissues to be nourished on a third. A tree, in fact, and an animal consist essentially of a mass of currents running in opposite directions, the interior of both tree and animal displaying a wonderful amount of activity—"never suspected by those who simply look at the surface of things." The circulation and its propelling force are studiously concealed; the tree invested in its bark and the animal in its skin being in some senses a sealed book. Dr. Pettigrew then gave a history of the circulation from Hippocrates to Harvey; and showed that, while very many facts as to the circulation were made out anterior to the latter, still it required Harvey's master-mind to grasp and explain the circulation in its totality, as it is at present received. To illustrate the currents flowing in opposite directions, he showed that a plant grows upwards into the air and downwards into the earth and outwards by branches and roots; to which end the currents and vessels engaged in the building-up of the plant must necessarily travel in two or more directions—in the directions, indeed, in which growth is proceeding. Animals also increase in an upward, downward, and lateral direction. The lecturer concluded with a demonstration that the parts of a plant are so arranged that its vital forces are made to act in the same direction as certain physical forces—those of endosmose and exosmose,—forces which are major factors in the circulation of plants and in the capillary circulation of animals. Numerous diagrams contributed to illustrate the exposition; and at its close the lecturer sat down amid loud applause, having still further strengthened his title to the praise of being "the most distinguished disciple of Goodsir."

On Friday, the 10th May, Dr. Pettigrew delivered his second lecture on the "Physiology of the Circulation in Plants, in the Lower Animals, and in Man," before the Royal College of Surgeons in Edinburgh. The vessels of plants and animals, he showed, were both constructed on the same type—the function of both being to convey nutritious juices to the tissues which on all occasions lie outside the vessels. Imbibition is the process by which animal and vegetable tissues are nourished—a fact to which there is no exception; the vessels having no other object than the conveyance of nutriment to the immediate neighborhood of the growing tissues. In animals there are three kinds of vessels: the arteries carrying pure or bright-red blood, the veins effete or dark-purple blood, and the capillaries mixed blood. The tissues imbibe from the latter. In plants there are also three kinds of vessels—those conveying crude sap, those conveying elaborated sap, and a vascular system lately discovered in the leaves and roots, which he compared to the capillaries of animals. Vessels in plants, unless when quite young, are rigid; those of animals are elastic. While the vessels of plants can take no active part in the circulation, those of animals have, in some cases, distinct rhythmic movements, which greatly assist that process. Such rhythmic movements are found in the veins of the bat's wing, the saphenous veins of the rabbit, the mesenteric vessels of the frog, and the caudal vessels of the eel. Dr. Pettigrew proceeded to show that the vermicular movements of the alimentary canal (of the œsophagus and stomach particularly) greatly resemble those of certain vessels, and of the heart itself, having a power of alternately contracting and expanding, which they employ in propelling the food and the circulating fluids. Between the stomach, the heart, and the hollow muscles generally, all of which are similarly constructed, there is an analogy—it being a matter of indifference whether the material conveyed be fluid, semi-fluid, or solid. The power possessed by the hollow muscles of contracting and expanding in parts, the sphincters, when existing, opening or expanding when the viscera to which they are attached close or contract, he dwelt upon as enabling the muscles alternately to seize and dismiss their contents—to manipulate the same with regularity and precision. In the vegetable cells, movements occur in great variety, the cells of plants having a circulation of their own, quite apart from the general circulation of plants, but quite as distinct. The movements of plants, he further showed, resemble in some respects those of animals, the spores of certain algæ being furnished with cilia "enabling them to lead an itinerant life until they find a suitable habitat." Dr. Pettigrew called particular attention to the rhythmic movements of the leaflets of the *Hedysarum desmodium gyrans*, which approach or recede from each other every few minutes. More curious and wonderful still are the rhythmic movements of the *volvox globator*. "At a certain period of the development of this simplest vegetable organism, there appear in each zoospore, or in the bands of protoplasm with which the zoospores are connected, vacuoles, spaces, or cavities of about 1-9000th of an inch in diameter, which contract with regular rhythm at intervals of from thirty-eight to forty-one seconds, quickly contracting and then more slowly dilating again, precisely as in their own hearts." Very remarkable are the rhythmic movements of plants, the more especially as they occur in the absence of nerves, muscles, and all those structures commonly regarded as essential to this kind of movement. Dr. Pettigrew sat down amid much applause.

Dr. Pettigrew delivered his third lecture on the 17th May, before the Royal College of Surgeons, Edinburgh. Entering more fully than on the previous occasion into the forces concerned in the circulation of plants, he showed that the gap between vegetable and animal circulation is smaller than might have been expected—a fact which becomes obvious when the lowest animals are compared with the highest plants. In spring and autumn the circulation is carried on mainly by single tubes; while in summer, when the leaves are formed and the roots are sprouting, the tubes are united in the leaves and roots by a system of vessels analogous to the capillaries of animals, forming a system of syphons of which the free extremities are directed alternately in the direction of the leaves and roots. These syphons enter into various combinations, but a certain number of them being open towards the roots and leaves, they are equally adapted for sending up surplus sap from the ground, and sending down excess of moisture from the air. The two systems of syphon-tubes, moreover, balance each other—the contents of the syphons moving in an upward or a downward direction according as the impulse is conveyed from the ground or the air. It is a matter of indifference, Dr. Pettigrew thinks, whether the sap is presented by the roots or the leaves; in either case it necessarily passes through the tree. A tree is always surcharged with tenacious juices; and when watery fluids are presented either from above or from below endosmose and exosmose are established. Dr. Pettigrew illustrated this by two tubes, one end of which was closed by an animal membrane. The one tube contained water, the other syrup. When the tube containing the syrup was placed in water the water rose in the tube just as the sap rises in the stem of the tree. When the tube containing the water was placed in syrup, the water descended in the tube just as moisture presented to the leaves of trees passes down through their stems. Physical forces contribute largely to the circulation of plants. Equally interesting was Dr. Pettigrew's explanation of the circulation in the individual cells of plants. Taking as his starting-point the counter-currents produced by endosmose and exosmose, he showed that when a cell is placed between two opposite currents, one current washes and penetrates one side of the cell, while the other washes and penetrates the other side. In this way the contents of the cell are made to gyrate —just as happens if opposing forces are made to act upon a wheel. If one side of a wheel is depressed while the other side is elevated, gyration results. The two forces, indeed, though acting in contrary directions, produce a common result. Movements occurring in cells "inaugurate" the general circulation; but when the general circulation is fairly established it contributes, nay actually produces, the gyration of the cell-contents. "The general circulation is then a differentiation of the intra-cellular circulation." Dr. Pettigrew concluded by saying that little importance attaches to the swaying of branches and stems in the wind as a cause of circulation—a process which goes on in hot-house plants and in trees nailed to the walls.

PATHOLOGY OF HERPES ZOSTER.

Dr. O. Wyss had the opportunity of carefully watching during life, and examining after death, a patient of von Witte's, of Rheinau, suffering from herpes. The patient was sixty-eight years of age, and on September 16th he had headache and febrile symptoms. On the 19th scattered vesicles appeared of herpes labialis. On the 20th the right side of the forehead, and nose and cheek as far as to the border of the lower jaw, were injected. On the 22nd the right eye itself was inflamed, and on the 23rd the left. Two days afterwards an eruption of herpes appeared on the right side of the face, which affected the cornea and conjunctiva. On the 28th the patient died. The post-mortem examination was conducted with great care. The herpetic vesicles and scabs were very accurately limited to the right side, and to the parts supplied by the first branch of the right trigeminal nerve. The left eye was perfectly normal. The nerve above mentioned was found to be broader and thicker than that of the left side, of a deeper grey-red color, of softer consistence, and with the several nerve fasiculi separated by greyish-red soft tissue, containing many vessels. This alteration in its character extended from the point where it entered the orbit to the finest branches as far as they could be traced with the simple lens. The other nerves traversing the orbit were perfectly healthy. Outside the orbit and extending from it to the ganglion Gasseri, the first branch of the fifth was surrounded by extravasated blood. On the proximal side of the ganglion Gasserianum the fifth nerve was normal in appearance. The ganglion itself was larger and somewhat more succulent than the left; upon its inner side was a red mass that appeared to be caused by an ecchymosis. The proper substance of the ganglion was not of a yellowish-white color, but bright red. The fifth nerve was healthy at its apparent origin from the brain, where it entered into the Gasserian ganglion. There were numerous ecchymoses. These were especially visible also in that part of the ganglion where the first branch of the fifth arises, whilst that from which the second and third branches arise was little altered. Microscopical examination of the skin showed that the papilla and the corium were strongly infiltrated with cells. In some parts the rete was preserved, in others, together with the upper layer of the corium, destroyed. The fasciculi of connective tissue on the forehead exhibited an infiltration of cells, especially in the vicinity of the vessels of the sweat-glands; where the cutis was most deeply ulcerated these fasciculi presented a lustrous, homogenous character, corresponding to the vitreous swelling of Neumann, or hyaline degeneration of O. Weber. The cornea was abundantly infiltrated with cells, especially in its upper layers; the ulcerations of the surface penetrated to the substantia propria. This is an important communication, and shows conclusively that herpes zoster is a consequence of inflammation of the corresponding spinal ganglion and of the nerve traversing it. (*Centralblatt*, No. 7.)

ARREST OF ARTIFICIALLY-PRODUCED EPILEPTIC FITS.

M. Brown-Sequard, in the last part of his "Archives," states that he has discovered a mode in which the fits of epilepsy which can so easily be induced in guinea-pigs in which the spinal cord or the sciatic nerve has been half cut through, may be suddenly arrested. It consists in stimulating the throat and larynx of the animal with carbonic acid gas. The proportion of the gas in the air thrown in must be sufficient to render it almost irrespirable by the operator, and it must also be injected with a certain amount of force. No effect is produced when it is inhaled or injected by the nose in the guinea-pig, though he thinks it would be otherwise with man. In some instances the effect seems to be for some time permanent, and the animal has no more fits, though previously, after an attack had been artificially induced, several fits often occurred spontaneously. M. Brown-Séquard has repeated and corroborated Rosenthal's experiments, showing that pulmonary insufflation can arrest the convulsions caused by strychnia; but he considers that the explanation given is incorrect, and that the arrest is not due to superoxygenation of the blood, but to the mechanical irritation exerted by the forcibly-injected air upon the ramifications of the vagi in the bronchi and of the nerves distributed to the diaphragm. Section of the spinal cord above or even below the origin of the phrenics and section of the vagi alike prevent the arresting action of the insufflation. M. Brown-Séquard thinks there can be no doubt at all that carbonic acid is one of the most powerful stimulants of the vagal branches, and perhaps also of the branches of other nerves distributed to the fauces, larynx, and bronchi, and that the irritation of these branches possesses the power of producing the arrest, suspension, inhibition, or, if the term be preferred, the cessation of activity, of those parts of the nervous system which are active in the production of convulsions due to epilepsy, poisoning by strychnia, or a rapid and considerable loss of blood.

COMPARATIVE RESEARCHES ON THE COMPOSITION OF THE BONES.

Prof. Aeby contributes an original paper to the *Centralblatt*, containing the results of a considerable number of analyses on the composition of the bones in the ox, having especial relation to the proportion of water, the specific gravity, and the amount of carbonic acid in the ashes. The conclusions at which he has arrived are that the bones of the ox contain, upon the average, more than 4 per cent. of lime salts in excess of those of man, with a corresponding higher specific gravity and smaller proportion of water. The variations in the proportion of inorganic and of organic substance do not exceed 2½ per cent. Thus, whilst in man the organic material reckoned in the dry state amounts on the average to 31·43 per cent., in the ox it is 28·49. The amount of water in man is 12·21 per cent., in the ox 9·49; and the specific gravity in man is 1·936, but in the ox 2·064. As age advances the bones of the ox contain an increased proportion of lime, and possess a higher specific gravity, so that whilst in a two year old the organic substances amount to 27·75, and the specific gravity to 2·069, in six and seven year olds these numbers are respectively 26·34 and 2·080. There is, however, at the end of the third year, in both sexes, a remarkable diminution of the specific gravity, which appears to be coincident with a process of resorption that occurs at this period, and is indicative of the termination of the period of development. Examination of the bones of the prehistoric period shows that from 7 to 8 per cent. of the calcareous salts have been withdrawn, which have been replaced by from 3 to 4 per cent. of carbonate of iron and manganese. This, he thinks, considering the poverty of the sea-bottom, is free carbonic acid, and in dissolved iron and manganese indicates enormous antiquity for the remains of oxen coincident with the stone age.

PEPSINE AND ITS ACTION ON BLOOD-FIBRINE.

Dr. V. Willich contributes a long paper to Pflüger's *Archiv* (Band v., Heft 8) "On the Ferments effecting the Digestion of Fibrine." The digestive fluid he employed was the fresh glycerine extract of the minced mucous membrane of the stomach of the pig. The fibrine was obtained from fresh blood. This was macerated in a solution of hydrochloric acid, containing 0·2 per cent. From the results of his experiments it appears that fibrine absorbes pepsine very energetically; that the process of digestion commences with the formation of a feeble chemical combination between the pepsine and the acid, and that this compound is the really active substance. In regard to temperature, digestion proceeds slowly even at 40° Fahr., but with the greatest rapidity and energy at temperatures between 95° and 112° Fahr. Higher temperatures than this retarded or altogether prevented the action. For the digestion of a certain quantity of fibrine, definite quantities both of acid and of pepsine are requisite. Meissner's parapeptones and metapeptones are initiatory stages of the action of the pepsine on the fibrine, and, if the action proceeds, are converted into peptone, but if the amount of pepsine be insufficient they may remain unaltered.

THE FORMATION OF GLYCOGEN IN THE LIVER.

Some years ago Dähnhardt removed the glycogen from numerous livers, and then found that, by the action of gently oxidising substances, glycogen could be obtained, which, by the action of saliva, was again convertable into sugar. In other words, he maintained that there was or might be a post-mortem formation of glycogen. Dr. Luchsinger has lately repeated these experiments in Kühne's Laboratory with a different result. He thinks that Dähnhardt did not entirely remove the glycogen originally present, and that if this were thoroughly and completely extracted by rapid division of the organ after death, and boiling the pulp in successcessive quantities of water till all opalescence had ceased to appear, no further formation of that substance would occur.

Foreign Gleanings.

TREATMENT BY COMPRESSED AIR.

Carbonic-acid gas, instead of combining, like oxygen, with the white corpuscles of the blood which emerge from the spleen, lymphatic vessels, and lymphatic glands, remains in a state of solution in the serum of the blood. Its more or less ready elimination is dependent on the different amounts of atmospheric pressure to which the body is submitted. Confined rooms and low habitations may even increase the amount of this gas, and then oxygen cannot penetrate and effect its healthy action. This is remedied by a stay in elevated localities, when an invigorating change takes place.

Baths of compressed air may advantageously be used instead of repairing to mountainous districts, as shown for the first time by Tabarié, a French engineer. His essay on the subject was presented to the Academy of Sciences of Paris in 1838, and the author was rewarded in 1852 by that learned body, after the baths had had extensive trials. The system was carried out on a large scale at Montpellier in 1840, and especially at Lyons at about the same period by Pravaz.

Tabarié was led to his discovery by the fact of the discomfort felt by travellers on great altitudes when the air becomes much rarefied. It was natural to inquire what would be the effect of a contrary state of things—namely, *increased* pressure. It was found that the rising of the mercury for an inch only upon the manometer was immediately followed by more energetic respiration and circulation. The practical application of this was first exaggerated, and the method made no progress; for it was a mistake to expect good results from *considerable* pressure, as from 5° to 15° of the manometer are sufficient. In fact, the disturbance of the equilibrium between the atmospheric pressure of the apparatus and the open air is already a therapeutical measure, mostly useful, and sometimes the only one necessary.

There are two ways of using pressure: (1) Perturbation—namely the more or less rapid change from ordinary to increased pressure, and the return from the latter to the former; and (2) medicating action—namely, an increase of pressure steadily continued, and kept up for a variable time, about one hour and a half on an average. The second of these methods is most frequently employed. But the difficulty lies in determining the amount of pressure fit for a given patient, as regards his morbid condition, and the changes in his symptoms from one day to the other. M. Bertin, in his work "Clinical Essay on the Use and Effects of the Compressed Air Bath" (Paris: Delahaye, 1868), gives a sketch of the apparatus, and the manner of graduating the pressure of the air, the latter, under varying circumstances, being left cold or slightly heated.

M. de Vivenot, of Vienna, has published carefully noted phenomena obtained during the baths, as regards the expansion of the lungs, and says: "The result of the pressure submitted to daily for two hours is an increased exhalation of carbonic-acid gas, and, as a consequence, a greater absorption of oxygen."

M. Bertin states, in the work above alluded to, "The compressed air bath has two actions:—1st. By increased mechanical pressure it augments the tonicity of the capillaries, and may thus favor interstitial absorption. 2nd. It favors the introduction into the lungs of a larger quantum of stimulant—i.e., the absorption of an increased amount of air, and consequently of oxygen. These circumstances act through the lungs on the economy at large, and promote general renovation by improving nutrition and instilling vigor into the frame."

It should also be noted that a pulse of 110 falls to 70 and 80, and remains at this lower figure up to the bath of the next day. Nor should it be forgotten that the changes which compressed air effects on circulation and respiration must, by favoring the incessant renovation of organic tissues, determine corresponding changes in the function of nutrition. This position is proved by the development of the appetite during the treatment by compressed air and the increased weight of the body. This latter fact was ascertained by M. de Vivenot, by means of experiments upon himself.

The benefits of this treatment are warmly and cleverly insisted upon by M. Raoul Le Roy, in a late number of the *Bulletin de Thérapeutique*, March 15th, 1872. An establishment of the kind might perhaps effect some good in this metropolis.

TRACHEOTOMY WITH THE GALVANIC CAUTERY.

M. VERNEUIL has laid before the Academy of Medicine a case wherein, instead of the usual operation, he divided all the tissues and even the trachea with the galvanic knife. The patient, thirty-eight years of age, required tracheotomy for phthisis laryngea. The steps of the operation are very minutely described in the report to the Academy—perhaps too minutely. At all events, the operator's object was to prevent hæmorrhage, and herein he succeeded perfectly; the canula was introduced with hardly any loss of blood, and the patient has done well.

It is but just to add that M. Verneuil stated distinctly that, in the present case, the patient being a thin adult, the galvanic knife had a better chance than it might have had with a stout suffocating child.

We should look upon these operations with moderate enthusiasm, and consider them as useful *adjuncts* to the modes of operating when an appropriate case presents. M. Chassaignac, who has rendered good service with his écraseur, rode his hobby so hard as to attempt amputation of a limb and lithotomy with it. There have been surgeons at Orleans who must needs take off a leg by successive galvanic section. Among ourselves, resection of joints is performed with a little too much regard to the attraction of novelty and imitation. M. Verneuil nevertheless deserves credit for his innovation, simply because it requires more courage and activity to strike a new path than to remain in the beaten track. Still, hæmorrhage can be avoided in other ways, as has been properly remarked by the editor of *La Tribune Médicale* (May 5th, 1872). Langenbeck's dilating hook is entered between the first two rings into the trachea, and, the handle being brought under the chin, the spring separates the two halves of the hook, the bistoury divides the skin and trachea, and the canula is introduced. M. Isambert operated upon a child a short time ago in this manner, with success as far as the operation went. His first case occurred in 1868. We subjoin an example of another way of using the galvanic cautery.

M. Amussat, in 1870, operated on a boy who for a month had a pebble lodged in the trachea. The operator run through the integuments and the trachea a curved needle carrying a double platinum wire for the length of two-thirds of an inch. The needle being withdrawn, M. Amussat seized one of the wires with two forceps in communication with a trough, and cut through the texture caught by the platinum loop without causing any discharge of blood. The trachea being thus opened, the boy expelled the pebble in a fit of coughing.

CONFESSION NO PROOF OF GUILT.

The *Lyon Médical* of April 28th, 1872, refers to the case of a girl, aged twenty, supposed to be seven months pregnant. After an attack of hæmorrhage her size seemed to have considerably diminished, and the girl, being closely questioned on the subject, said that, becoming aware of a discharge, she repaired to the closet, where she stayed ten minutes. She added that all had escaped, but that she had not time to look, as she was being called by her mistress. A midwife and the parish surgeon both declared that the girl had been recently confined. She was now again assailed with questions, and told that, for her own sake, she had better make a clean breast of it, as no fœtus had been found in the closet. Perhaps, it was suggested, she had thrown it into the pigstye. The poor creature at first denied such a thing, but at last confessed that it was so. A search was made, but no child was discovered. She was tried for concealment of birth, on her own confession, and sentenced to six-months' imprisonment. The girl had not been taken into custody in consideration of her free confession, and she quietly proceeded to the gaol. When admitted it was found that she was far advanced in pregnancy, and soon gave birth to a healthy girl. By the French law she could no longer appeal, as more than ten days had elapsed since the verdict; but the judge, having the power of appealing within two months, did so, and the girl was acquitted.

This case shows that confession, which is looked upon as the clearest proof of guilt, cannot always be relied upon. And what shall we say of the surgeon and the midwife? The examination was probably hurried and incomplete, and the conclusion arrived at on seeing the signs of recent abundant hæmorrhage. The case, even in a simple obstetrical point of view, is full of valuable hints.

GRAFTING WITH RABBIT SKIN.

M. Coze, surgeon at the military hospital of Perpignan, has submitted to the Academy of Sciences of Paris three cases of obstinate ulcers which he succeeded in healing by grafting upon them cutaneous particles taken from the rabbit. M. Larrey, who presented the paper, thought so much of the successful issue that he proposed to place the essay before the committee appointed to award prizes to the best works on medicine and surgery.

TRANSVERSELY STRIATED MUSCLES IN ACARI.

M. Schultze's *Archiv* (Band viii.), J. H. Floegel states that he has ascertained the presence of transversely striated muscle in a species of Trombidum or red spider found in gardens. The animal should be immersed for an hour or two in a one per cent. solution of prussic acid, and dissected in weak glycerine and water. The transverse striæ are remarkably distant from one another—ten myriamillimetres.

HUMAN FOSSIL REMAINS.

Cuvier did not believe that such remains existed. Was he in error? This question is handled by M. Garrigon, in a paper submitted to the Academy of Sciences of Paris. There will probably be a report upon this paper which will show whether this learned body has, since Cuvier, modified its opinions.

PERFUMED COD-LIVER OIL.

Take of the essence of eucalyptus fifteen minims, cod-liver oil three ounces, and put into a well-corked bottle. The fish-oil thus loses its nauseous taste. For brown oils the quantity of the essence may be increased.

News Items, Medical Facts, &c.

Coca.—A correspondent makes some inquiries regarding the properties of coca. There is considerable difference of opinion as to its effects upon the human subject, and the published accounts are somewhat conflicting; but we think that there is strong evidence in favor of its being a stimulant and narcotic of a peculiar kind, and of some power. An interesting and popular account of coca was published in Johnston's Chemistry of Common Life, and descriptions of its action have been given by various travellers in Peru and South America. The *Erythroxylon coca* is a shrub of about six feet high, with green leaves and white blossoms, the fruit consisting of small scarlet berries. The leaves are gathered and carefully dried. When chewed, they have a slightly aromatic, bitter taste, not unlike that of green tea. Dr. Mantegazza published a prize essay in 1859 on the Properties of Coca, in which he gathered together most of what is known, and detailed some experiments which he made with an infusion of the leaves of the plant. Extraordinary stories are told of the wonderful properties of coca in enabling those who chew it to overcome the feeling of hunger, and to undergo great muscular exertion and fatigue whilst fasting altogether or only using the sparest possible diet. The Incas and Indians, who constantly masticate coca, are said to go through excessive labor with ease, and to require very little food; indeed they believe that it may be, in a great measure, substituted for food. Making every allowance for exaggeration, the accounts that have been given us by some physicians and travellers in Peru and South America would lead to the belief that coca has some remarkable properties in this respect, and we have often wondered that these have never been investigated in this country. It is very possible that in coca may be discovered a valuable aid, in fevers and some nervous and wasting diseases, for sustaining the strength under conditions where ordinary food cannot be taken.

Dipsomania and Drunkenness.—Dr. Skae, of Edinburgh, insists upon the clear and certain diagnosis which may be made between dipsomania and drunkenness. In the dipsomaniac there is a genuine disease of the nervous system, often hereditary, and displaying itself in periodic paroxysms. These paroxysms are preceded by general perturbation of the system. "The patient is sleepless, is ill at ease; he has a perspiring skin, a quick soft pulse, and experiences a feeling of nervous prostration so great that he is driven to the use of stimulants for relief from his suffering. In the intervals such a man very frequently abhors the very sight and smell of strong drink; he is, while his disorder is in abeyance, amiable, and exemplary in the discharge of his duties, and is frequently possessed of mental endowments of a high order. How different is the condition of the sot, who drinks for the sake of drinking, and the pleasure he derives from the gratification of his appetite! The habitual drunkard is never exemplary in the discharge of his obligations; he does not drink in insane paroxysms, followed by bitter repentance, but quietly makes up his mind that drunkenness is happiness, and indulges his vice for the pleasure he experiences in doing so." The moral is, that whilst there is little hope from medical treatment in the case of drunkards, dipsomaniacs may and ought to be confined at the earliest possible date.

PRINTED AND PUBLISHED BY

Wm. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 9. NEW YORK, SEPTEMBER, 1872.

Lectures

ON

THE SURGICAL TREATMENT OF ANEURISM

IN ITS VARIOUS FORMS.

Delivered at the Royal College of Surgeons, June, 1872.

By Timothy Holmes, M.A., F.R.C.S. Eng.,

Professor of Surgery and Pathology to the College.

LECTURE I.—Part I.

Mr. President and Gentlemen,—Before speaking on the subject which I have chosen, I must be allowed to tender my heartfelt thanks to the Council of this College for the favor they have shown me in nominating me your professor of Pathology and Surgery. It is a great distinction to have been thought not unworthy of succeeding to the long series of famous surgeons who have delivered the lectures on Surgery at this College. I am fully sensible, indeed, how little anything I have done in surgery has merited such an honor, and still more painfully sensible how little I can do to support it. I can only trust to your indulgence to compensate my deficiences. Still I can assure you that however little I may be able to do, that little shall be done sincerely and honestly. I hope you may not have to charge me with having used the position you have given me to broach crude theories, or to recommend rash treatment more dangerous than the maladies for which it professes to provide a cure. My subject will lead me to speak of many of the great exigencies of surgery, of desperate lesions of the great channels of the circulation—maladies which have hitherto defied our best efforts to control them, and which, indeed, till comparatively recent times were hardly considered objects of deliberate surgical treatment. I shall endeavor to give, perhaps, a somewhat less gloomy view of some of those affections than is often taken, to prove that they are sometimes at least under the control of surgical art, and, in the terms of my professorship of "Pathology and Surgery," to show you that some of the more modern plans for the treatment of internal aneurism rest upon a sound pathological basis. If so, the inference that their practice is justifiable cannot be questioned. Yet I would not be understood as putting these measures on the same footing as those which we have been in the habit of using for external aneurism, whose action we are familiar with, and whose success has been established by practice. The measures which I shall have to treat of as used in aneurism of the thorax and abdomen are at best doubtful remedies for diseases which are wellnigh incurable. Those who practise them must be prepared for frequent disappointment—nay, for a preponderance, I fear, of failure over success,—a mortifying contrast to the brilliant results of many other kinds of operation. But this surely ought not to deter us. Our art is promoted by facing difficulties, not by avoiding them; and if we can succeed in rescuing even a small proportion of patients laboring under such deadly maladies from the fate which is otherwise inevitable, every feeling of humanity and of surgical duty forbids us to shrink from the attempt, however difficult it may prove, and whatever mortifications we may endure in making it.

I have selected my present subject—the surgical treatment of aneurism in its various forms—for several reasons; partly because it is long since it has been treated of at this College, and in the interval the most remarkable progress has been made—in fact, the practice of surgery has, we might almost say, been entirely revolutionised; partly because so much doubt and difference of opinion still prevail with respect to many of the questions connected with this subject; and again because so much interesting and instructive matter exists in literature, in our own pathological museum, and in the various other museums of London and elsewhere, which if I can succeed in bringing together into an intelligible whole, I may hope to contribute to the settlement of some of the questions which are yet in abeyance.

Our profession, Sir, is more free, I think, than any other from those miserable jealousies of nation against nation, and school against school, which hinder the progress of knowledge in so many of its branches. We are always glad to receive any improvement in practice, whether its origin is British, Continental, American, or Asiatic. But we do not profess ourselves, nor ought we to feel, indifferent as to whether English surgery is maintaining the place in which our fathers handed it down to us; nor would it be other than a humiliation, and a most just one to us, if English surgeons had changed from teachers to learners. Thank God that is not so at present; though we are glad enough to learn from anyone, we can still say with truth that very much of what has been taught to the world in modern times of the art of surgery has been taught by the members of this College. I need hardly mention that the greatest masters of two of the chief improvements of modern surgery —the practice of excision of joints and the practice of ovariotomy—sit on our Council. So also in the branch of surgery with which I am now concerned. Foreign surgeons have contributed much that is valuable to the treatment of aneurism, but the successors and pupils of John Hunter have borne, as they ought to have borne, the foremost part in it. As it was from him that that great invention emanated which first brought aneurism within the power of successful treatment, so it is from those who were trained in his school that the ideas proceeded by which that success has been exextended. The treatment of aneurism has, indeed, improved since Hunter's day, but anyone who studies the subject can see that there are few of those improvements (excepting the grand discovery of anæsthesia) the germs of which are not contained in the operations and the writings of those famous surgeons of Hunter's school whose busts adorn our vestibule, and whose undying memory is the most precious of all the possessions of our College.

The subject of aneurism was treated at this College forty-three years ago by Mr. Guthrie in the famous course of lectures delivered by him as professor of anatomy and surgery, and which formed the substance of his work "On the Diseases and Injuries of Arteries." That work has become one of our surgical classics, and added fresh lustre to the name of one of the most celebrated surgeons in Europe. But, admirable and complete as that treatise was when it was published, it dealt with the subject of surgical treatment in a manner which has now become antiquated. The treatment by compression, though it could count authorities in its favor, though tried by Hunter, and strongly advocated by Freer, was then in its infancy, and is dismissed by Mr. Guthrie in a few half-contemptuous sentences.* The distal operation is indeed discussed more adequately, but from imperfect data, and with a result which can hardly be accepted as satisfactory. In fact, the conclusion to which we are brought is, that an aneurism which is not curable by the Hunterian operation is, with few exceptions, not curable at all. Since the surgeons of Dublin showed how to apply pressure to an extent which is tolerable without chloroform, so as to produce the gradual consolidation of the aneurism, all this is totally changed. Acting on this fertile suggestion, surgeons in all parts of the world have been studying fresh and milder methods of curing popliteal and other aneurisms. We have seen with astonishment the success of the simple plan of flexion of the limb in popliteal aneurisms, whereby that formidable disease is sometimes cured in a day or two by a mere change in position so slight as hardly even to cause inconvenience, much less pain or danger, to the patient. More wonderful still, perhaps, we have seen the great modification which has been introduced into the practice of surgery by the use of rapid pressure continued for many hours (in a recent case more than two days) under chloroform, whereby deep-seated aneurisms in the abdomen, and even aneurism of the lower part of the abdominal aorta itself, have been permanently cured, and a disease has been proved to be under the control of our art which in Mr. Guthrie's day would have been regarded as utterly beyond any surgical treatment.

Stimulated by these great successes, surgeons have now begun to regard no form of aneurism, however near the heart, as absolutely incurable; and to seek for means of checking the growth, at least, if not of finally obliterating, those tumours in which the vessel affected is itself inaccessible. Hence the introduction into general use of a number of new plans of treatment, such as electropuncture, manipulation, the introduction of foreign bodies into the sac, coagulating injections,—and hence also the renewed impetus given to the use of the distal operation. Now I will admit at once that none of these novel methods has as yet proved regularly successful; but I cannot help contending that some of them, at any rate, have shown enough in their favor, both from pathological anatomy and surgical experience, to justify—nay, to necessitate —their further trial in appropriate cases, and to excite confident hopes of their future successful application regularly to certain classes of cases in which they have already succeeded, though only occasionally, and, as it were, hap-hazard. We should be committing, I believe, the same error which those fell into who passed over the treatment by compression as unworthy of serious discussion on account of its rare successes and dubious advantages when first tried, if we similarly repudiated without further inquiry the use of the methods to which I have alluded, because hitherto, applied as they are nearly exclusively in the more formidable classes of aneurism, they have for the most part proved unsuccessful. It is only too evident, however, that the discussion of topics of this sort will lead me into questions on which the widest difference of opinion prevails; and it is equally evident that a course of six lectures affords too little space to do more than indicate in the briefest manner my reasons for the conclusions to which I shall desire to lead you. I must be pardoned, therefore, if, in speaking of the surgical treatment of thoracic, subclavian, and abdominal aneurisms, I omit all but the most indispensable references to the pathology and diagnosis of those affections. You will, I hope, attribute this omission to the limits imposed upon me.

The treatment of a subject such as this has not become more easy since Mr. Guthrie wrote. There was then almost a deficiency of material; there is now almost a plethora. So many and so well-stocked have become our pathological museums, even in London alone, that it is difficult for anyone occupied by other duties to master their contents. I have, however, to thank the authorities of the various museums for the kindest assistance and the most liberal communication of all the stores contained in their collections. More especially I have to express my gratitude to the surgeons of the Dublin hospitals, and more particularly to the

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* On the Diseases and Injuries of Arteries, p. 142.

Council of the Royal College of Surgeons in Ireland, for placing at our dispsal a large number of highly valuable preparations, some of them of great historical interest, which I pointed out as likely to elucidate my subject. Personally unknown as I am in Ireland, I cannot construe this unusual courtesy as done merely to myself, but it affords a gratifying proof of their good feeling to this College, and of their desire to assist any effort, weak as it may be, to improve the knowledge of a disease from whose successful treatment Irish surgeons have already done so much. It is my duty as well as my great pleasure to acknowledge above all the very kind assistance which my Friend, Mr. W. Stokes, has rendered to me in this matter.

As with pathological specimens, so with published material; its bulk in the present day outruns the best efforts of any one inquirer to keep up with it. Statistical writers indeed afford us the clues to the vast labyrinth of literature in which the various topics are treated. But their collections, even if they could be made free from serious errors, which usually is far from being the case, are, after all, composed of materials of the most widely different value: many of the cases upon which their numerical summaries are founded being, when investigated, hardly worthy of serious attention; so that in all cases the original sources must be used. I have, therefore, entirely renounced the idea of treating any of these topics with reference to statistical completeness, nor, in enforcing my own opinions as well as I am able, will I weary you with a tedious enumeration of those of others. I will only cite, from the literature to which I have had access, as much as I think necessary to support my conclusions.

I have already put into your hands a detailed prospectus of each lecture; but I think it will also be useful to state more succinctly the main propositions which I hope to establish in this course. They are as follows:—

1. That aneurisms, of whatever form and however near the heart they may be, ought not to be regarded as incurable, but should be made the objects of definite methodical treatment, internal or external.

2. That there is definite proof, from pathological anatomy and from surgical experience, of the curative influence of Brasdör's operation in innominate aneurism, and of its beneficial effects in some cases of aortic aneurism.

3. That arteries may be successfully tied and obliterated without their continuity being interrupted; and that this modification of the ligature, whilst affording much security against secondary hæmorrhage, and thus much diminishing the danger of the operation in general, may very probably in future enable surgeons to deal successfully with cases in which it may be necessary to tie the first part of the subclavian (whether on the distal or proximal side of an aneurism) or the innominate artery.

4. That galvano-puncture may be used with at any rate temporary benefit in thoracic aneurism; that its use is not so dangerous as to render further trials of it inexpedient; and that there is good hope that the method may be so far perfected as to make it a safe and regular plan for the treatment of thoracic, subclavian, and other forms of aneurism.

5. That many cases, such as those in which ligature of the artery near to the heart has been resorted to for the cure of subclavian and subclavio-axillary aneurism, may be made amenable to improved methods of pressure.

6. That aneurismal tumours situated even as high as the lower part of the abdominal aorta, those of the mesenteric and other branches of the aorta, and of the iliac arteries, may be treated with success by rapid coagulation of blood under pressure; but that this method is a dangerous one, and should not be used until internal treatment has failed.

7. That there are cases of abdominal aneurism in which Mr. Syme's suggestion of reviving the old operation is worthy of further trial.

Besides these positive conclusions, I shall hope to suggest the main considerations applicable to questions on which I see no ground at present for any definitive judgment, such as the value of manipulation in subclavian, or of distal pressure in abdominal aneurism.

The first fact that I will start with, as a basis for all that I am going to say about the surgical treatment of internal thoracic aneurisms, is the very elementary fact that such aneurisms are not incurable, whether they affect the aorta or the innominate artery. Numerous instances are on record in which persons affected with aneurism of the arch of the aorta have, either spontaneously or as the result of medical treatment, been restored to a condition of tolerable health, have lived for a long period of time, and have ultimately died from other causes. I do not wish to weary you with instances of what I take to be admitted on all hands, but I shall have occasion incidentally to refer to various instances of this as I proceed.

I do not for a moment question, what indeed pathological anatomy proves beyond dispute, that aneurisms of these great arteries are very often caused by a widespread degeneration of the arterial system, and that even if we could cure the aneurism which is for the time under treatment, another may very probably soon show itself, or there may be another actually present, but undetected, which may cause death quite unexpectedly while we are directing treatment to the one which is visible. This fact is strikingly exemplified by a case which Mr. Christopher Heath recently published,* where a patient who had undergone spontaneous cure of an aneurism of the left subclavian soon afterwards came under treatment for aneurism of the subclavian and root of the carotid on the right side. He was judged to be an unfavorable subject for operative interference, so that no active treatment was undertaken, and the suspicion was verified by his sudden death from the rupture of an aortic aneurism, the symptoms of which had been masked during life by those of the other disease. The cure, therefore, of the one aneurism did not prevent the rapid development of the second, and treatment directed to it, however successfully, would not have averted his death which was inevitable from the growth of the third.

But allowing that it is difficult to obtain complete consolidation in an aneurism of such large arteries, and that the cure when obtained affords no guarantee for permanent recovery, it is still evidently our duty to attempt the treatment of the disease if cure is possible—nay more, if even alleviation and the prolongation or life is possible. Now this latter is not only possible but easy. Judicious medical treatment, with rest systematically enforced, is almost always followed by temporary improvement. And in aneurisms which are growing in any definite direction, and whose growth in that direction threatens important organs, or is likely to lead to rupture of the sac, there is always a chance of stopping such growths for the time, and so possibly preserving the patient's life. I venture to say that this point is hardly enough considered. We think so much of whether an aneurism can be completely consolidated or not that we are apt to forget how often its partial consolidation only is followed by such an alleviation of the symptoms as amounts almost to temporary cure.

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* Clin. Soc. Trans., vol. ii., p. 8. Patho. Trans., vol. xx.

Let me spend one minute here in combating an objection which I recently heard made against all endeavors to apply treatment with a view of producing consolidation in the external accessible part of a thoracic aneurism. Such consolidation, it was said, allowing that it could be produced, would be injurious instead of beneficial; for the external part of the sac acts as a sort of safety-valve to protect its internal portion from pressure, so that the effect of obliterating the part which is growing under the skin would be to throw the force of the circulation on the intrathoracic portion, and so hasten the rupture of the sac. This objection looks plausible at first sight; but it seems to me to have no real foundation either in clinical experience or in pathology. In fact, the history of such cases as those which I shall bring before you shows that the consolidation of one part of such a tumour is never, or hardly ever, followed by any symptoms of increased growth in another part; and the way in which an aneurism extends appears to me to be, not by a passive yielding to fluid pressure applied generally to its interior, but by an active growth, attended very probably with inflammatory softening of a part of its wall. And I hold that the more of the circumference of an aneurism we can defend, and, as it were, plaster up with solid and adherent clot, the safer do we render our patient against this fatal tendency of the sac to bud out and ultimately to burst.

I may instance the case which furnished this series of drawings lent me by Dr. Cockle. You will notice that the tumour, which was of moderate size externally when the patient first came under observation, has grown actively towards the skin till it diffused itself in the cellular tissue, forming an immense cushion, enveloping all the root of the neck, and causing death either by dyspnœa or subcutaneous hæmorrhage. If the external portion could have been solidified, the internal part of the sac, for anything we know or could know, might have been already solid enough to bear the weight of the blood-stream, and sufficiently far from any source of irritation to be free from the risk of active softening. In fact, why leave your patient exposed to one danger, visibly imminent, because you think that, if you deliver him from this, he may fall into another of the existence of which you have no proof?

The prospects of cure in a case of thoracic aneurism vary with an infinite number of circumstances besides the presence or absence of extensive disease of the arterial system. One of the most obvious of these is the shape of the tumour. Cylindriform aneurisms (such as that here figured from the museum of St. George's Hospital*) are obviously far less susceptible of a cure than those which are sacculated or lateral. In these cases, however, the symptoms are less serious. The patient in question suffered but little from the aneurismal dilatation, though it extended downwards as far as the axilla, and reached up the neck half-way to the jaw; and his death was very probably unconnected with this disease. Lately I saw at the Royal Free Hospital two patients of Dr. Cockle's affected with an apparently similar disease, but who had had it for ten years and four years respectively, and were suffering really very little from it.

But though the cure of such a condition is not probable, it is far from being impossible. The preparation here produced, from the museum of this College (No. 1641) shows a smaller dilatation of this kind, affecting only the arch of the aorta, in which the whole tube is lined with layers of fibrine, leaving a channel in the middle about the natural size of the aorta; and here, as you will notice, the cure is accompanied by impaction of clot in the mouth of the left subclavian. The preparation is unfortunately unaccompanied by a history; but there can be little question that the aneurismal dilatation was at least temporarily cured.

The opposite condition, that of sacculated or lateral aneurism, is shown in this drawing from a preparation in the museum of St. Thomas's Hospital (Series Y, No. 61.) I do not suggest that an aneurism so enormous as this is curable by any surgical means. I shall use the case afterwards in speaking of the treatment of thoracic aneurisms by galvano-puncture. But it is clear that, if we could produce coagulation in the primary sac—that which is lying between the arch of the aorta and the chest-wall—before the formation of the large external secondary sac, there is no reason why the further growth of the tumour should not be permanently checked, since the stream of blood through the mouth of the aneurism is neither very direct nor extremely large. And that medical treatment may prove efficacious in curing an aneurism as enormous as this, the following very interesting drawing and history of a preparation in the museum of St. Bartholomew's Hospital will prove. The following is the history of the patient in the fifth volume of the Pathological Society's Transactions, related by Mr. Harvey Ludlow:—

"G. A——, aged forty-seven, a publican, short in stature, consulted Mr. Stanley, at St. Bartholomew's Hospital, in October, 1847, for a large pulsating tumour in the right breast, which was rapidly increasing. The tumour was semi-oval in shape, of considerable size, and projected to the right of the sternum, almost immediately behind the nipple. Its long axis was oblique, and corresponded with the direction of the fibres of the lesser pectoral muscle. Although no bruit was audible, the aneurismal nature of the tumour was at once manifest. The pulse in both radials was feeble, and the superficial veins of the left side of the neck were turgid and dilated. The lower edge of the second rib, a portion of the whole thickness of the third, and the upper edge of the fourth, had been removed by absorption. He did not complain of much pain, but more of a general sense of uneasiness and oppression. His respiration was increased in frequency, he had a troublesome cough, and expectorated large quantities of a clear slimy mucus. He had suffered from pain in the chest, of a sharp spasmodic character, for a year before he consulted Mr. Stanley, and he had been teased for some time with a hacking cough, accompanied with expectoration of a clear adhesive mucus. An external swelling was first observed about five months before; it was then of small size, and pulsated strongly.

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* The case is to be found in the Patho. Trans., vol. iii., p. 50.

"The disorder being obviously irremediable by art, the man was advised to return home and keep himself as quiet as possible. In conformity with this advice he remained in bed, and took nothing but beef-tea, milk, and light puddings. The tumour rapidly increased and pulsated more strongly. Three months after his consultation with Mr. Stanley it equalled in size and shape the half of a cocoa-nut, had produced absorption of part of the second and fifth ribs, and had entirely removed about two inches and a half of the third and fourth. The greater distension and varicosity of the superficial veins of the neck and abdomen, joined to the aggravated cough and increased difficulty of breathing, seemed to show that the disease was advancing both within and without the chest. It was now Christmas, 1847, and from this time no further enlargement of the external tumour was observed; its walls gradually lost their compressibility, and slowly hardened, first at the extreme convexity and then at the base. The pulsations too progressively diminished in force, and by the end of March, 1848, were scarcely perceptible.

"While these changes were taking place in the tumour, the man himself had become pale, emaciated, and so feeble as to require an improved diet and stimuli. He had not left his bed for six months, but in the spring he began to move about in a wheel-chair. The respiration was labored and the cough remained troublesome, but neither seemed to get worse. The external tumour became, at length, pulseless, hard, and incompressible, and stood up like a solid growth from the right wall of the chest. In this state he lived four years and a half, and might probably have lived longer had not the general election of 1852 excited him to deviate from the tranquil and abstemious habits which he practised. He exposed himself imprudently, was seized with suppurative pleurisy, and died."

The preparation shows the entire consolidation of both the primary or intrathoracic, and the secondary or external sac. The opening is seen to be in precisely the same situation as in the former preparation—viz., midway between the aortic valves and the innominate artery, and it leads into a cavity which (as Mr. Harvey Ludlow describes it) would have about contained a horse-chestnut.

I could refer, of course, to many cases in which aneurisms of the thoracic aorta have been found to be spontaneously cured in the post-mortem examination of persons who have died of other diseases*; but I have preferred to adduce the above case as illustrating better than any of the others that I have met with the truth that persistent and systematic efforts should be made to save life or to prolong life under such circumstances. Too often, I fear, the disease is regarded as inevitably fatal, and as hardly an object for definite treatment at all. I do not know that I could express my meaning better than by reproducing what Signor Ciniselli says with regard to the cases of spontaneous cure of aortic aneurisms†:—"Allowing that the rarity of the examples of such cures proves to us that we ought not to wait for the resources of nature in cases of the same kind, they nevertheless show us that cure by coagulation of blood is possible in every species of aneurism, and that it may occur without the obliteration of the affected vessel. That which nature shows to be possible, art ought to endeavor to imitate or promote. It is on this maxim that the progress of the physicial arts and sciences rests, as well as the chief achievements of surgery."

Other thoracic aneurisms susceptible of surgical treatment are such as effect the innominate artery, or the roots of the left carotid or subclavian within the thorax, or, as in usually the case, one or more of these great arteries simultaneously with the arch of the aorta.

Now, as to pure innominate aneurism (which I incline to think is less rare than is generally believed), it, like aneurism of the aorta, may be either of the tubular (periphera) variety or of the sacculated (lateral). But this is not a matter of the same importance in respect of its curability as in the case of an aneurism of the aorta, since a pure innominate aneurism, though tubular as regards the innominate, is sacculated as regards the aorta—that is to say, it stands out as a bud from the main stream of blood, and coagulation may readily go on in it. Nay, unless its origin from the aorta is very large and diseased, complete coagulation must, I think, occur in such a tumour if the distal orifices (those of the subclavian and carotid) can be obliterated.

Many preparations might be adduced which show the possibility of cure in innominate aneurism. As an instance of spontaneous cure, this preparation (from the museum of St. George's Hospital) with its history is especially in point. I shall have occasion to refer to the case more in detail hereafter while discussing the indications for the distal ligature of the carotid in such tumours and its effects. Suffice it to say here that the aneurism was so far cured by distal impaction of clot in the carotid artery that the patient suffered no further symptoms of aneurism during the four years and a half which preceded his death from consumption, though the whole of the sac is not obliterated.

As an instance of the cure of such an aneurism by surgical means, I would adduce Mr. Fearn's well-known case, in which the double distal operation was performed. Here also the sac is not completely obliterated, and the patient's death happened much sooner after the last operation than in the previous instance; still the symptoms of aneurism had been entirely relieved, and the probability is that the disease would have given no further trouble.

It being admitted, then, that cases of growing aneurism, affecting the thoracic aorta, or the innominate, or the roots of the left carotid or subclavian within the thorax, or affecting the aorta and one or more of its primary branches simultaneously, are not to be regarded as wholly incurable, the next question is, What are the means hitherto devised for their treatment? They are three in number—namely: internal or medical treatment; the distal ligature, or Brasdor's operation; and galvano-puncture.

I pass over the injection of ergotine into the cellular tissue around the sac, which has been said in a foreign medical journal to have been recently recommended by Langenbeck, since that distinguished surgeon has not as yet published his own version of the proposal. The theory of the matter as explained by the journalist seems to me utterly unintelligible, but I have made trial of the method in two cases, with results which were perfectly nugatory; and I have not succeeded in discovering that the experience of others has been more satisfactory. I pass over, also, the plan of introducing a mass of fine iron-wire into the sac, as practised by my late friend, Mr. Moore; since I think no one will deny that it is not more certain than galvano-puncture, while it is far more likely to cause inflammation of the sac-wall. Since Mr. Moore's death, the operation, I find, has been twice repeated—namely, by Dr. Murray of New-Castle, in a case which terminated by suppuration of the sac; and by Mr. Dumville of Chatham, without any definite result. The sac has also been transfixed with fine needles with the same object.

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* Good examples may be found in Hodgson, Diseases of the Arteries, pl. vi.; Dr. Bristowe in Patho. Soc. Trans., vol. x., p. 83.

† Sulla Elettro-punttura nella cura degli Aneurismi.

With regard to the internal or medical treatment I shall say very little, since I could add nothing to the admirable exposition lately given of it by Mr. Jolliffe Tufnell.* The extreme bleedings recommended by Valsalva have been discarded as calculated to excite the heart to irritable action and to impovish the blood, thus both impairing nutrition, and so endangering the rupture of the sac, and rendering the blood less fit for coagulation. But the valuable parts of Valsalva's treatment have been retained and methodised. The patient is confined strictly to the recumbent position during a very long period, the most minute precautions being taken to ensure that he can be kept without raising himself once from the bed during many weeks; his food is restricted to the smallest quantity of nutritious solids that can satisfy his appetite, and his drink to the very smallest quantity which can be tolerated. Much stress is justly laid by Mr. Tufnell on this latter precaution, and Dr. Sibson also testifies to the extreme importance of restricting the quantity of fluid to the lowest possible point. The medical treatment of the case otherwise consists almost exclusively in the administration of such narcotics as may be absolutely necessary to calm pain and procure sleep, and such purgatives as may be required to keep the patient comfortable, and avoid all straining in defecation. The great object is to keep the pulse at an equable rate, a little over 60, and free from all excitement. Though Valsalva's extreme bleedings are avoided, a very small venesection may be occasionally serviceable with this view. I would strongly urge any surgeon undertaking for the first time the care of a case of internal aneurism to study Mr. Tufnell's invaluable little pamphlet, which shows how much may be done for the relief of conditions usually regarded as hopeless, and by how simple, safe, and painless a method. I will not discuss the value of the drugs which have been recommended as producing the coagulation of blood in an aneurismal sac further than to say that my experience of the iodide of potassium and the acetate of lead has been hitherto quite negative, though I have made careful trial of both; that I can point to no less than three cases in which an aneurism first showed itself and continued to grow while the patient was suffering from symptoms of lead colic, and that I am therefore disposed to accept the dictum of the recent Croonian lecturer at the College of Physician, Dr. Bristowe, who said that in his opinion there is not any medicine capable of bringing about, or assisting in bringing about, the result of plugging an aneurism with coagula except in so far as it aids in promoting tranquillity of circulation. He adds, also, that he cannot think that any other plan of treatment which might tend to promote coagulation at the expense of the patient's general health would be admissible.* For Dr. Bristowe argues that if the medicines which have been supposed to procure the spontaneous coagulation of the blood in aneurisms really did so there would be a risk that they might produce clots in the vessels or the cavities of the heart and so cause a disease more formidable than the one they are intended to cure. I may add that there is a patient now in St. George's Hospital in whom the symptoms of aneurism first showed themselves while he was suffering from lead colic, and in whom iodide of potassium, administered with the view of assisting in the internal treatment of the disease, produced rapid iodism, with no effect on the tumour.

The internal treatment of aneurism is no novelty. The plan has been described very sufficiently by Abernethy;† but the merit of methodising it, of insisting forcibly on its paramount claim to a preliminary trial in internal aneurism before any of the more dangerous methods of treatment are adopted, and, above all, of showing with what success it may be practised, cannot be denied to Mr. Tufnell. Allow me to exhibit this original drawing with which Mr. Tufnell has kindly favored me, taken from a case (related in his book) three years after the consolidation of a very large aneurism of the arch of the aorta on the right side, where its prominence is still seen, and its effects are also to be traced in the great enlargement of the superficial veins of the chest and abdomen. The man was perfectly well, and following his occupation of an engraver. So strong, indeed, is my sense of the value of this "internal treatment" of aneurism, that I beg in commencing these lectures on the more properly "surgical" treatment of the disease, to state distinctly that I only regard intrathoracic or, abdominal aneurism as a proper subject for such measures on the failure of the internal treatment, which should in every case be the first resource of the practitioner, even in those where the abdominal aorta is amenable to compression, since such compression cannot be effectually put in force without danger, as we know from instances which have been recently published.

LECTURE I.—Part II.

Let us pass on now to Brasdor's operation. The name, as is well known, was derived from a Parisian surgeon, of whom there is a tradition that he used in his lectures to speak of the feasibility of tying an artery as it leaves the sac of the aneurism; but he never performed the operation, nor did Desault, who gives us the proposal in writing.‡ This proposal of Brasdor and Desault was merely that, in cases of axillary and inguinal aneurism, where, as Desault thought, the artery above the sac is inaccessible, it might be exposed and tied as it leaves the sac. This operation was attempted by Deschamps, but very unsuccessfully, in a case where the common femoral was obliterated below the sac, and where, consequently, the surgeon had much trouble in recognising it. It seems, however, that he did succeed in embracing it, with some neighboring structures, in a ligature. Brasdor is said to have witnessed this operation. It was judged necessary soon after to perform the old operation, with fatal results. The French surgeons, however, did not, as far as I know, point out the circumstances in which Brasdor's method may be confidently expected to succeed—namely, when no artery except the trunk which has been secured opens out of the tumour. This was clearly explained by Mr. Hodgson.* But Mr. Wardrop was the first to show by actual practice the success with which the operation may be performed in the only kind of aneurism which fulfils this condition—namely, carotid aneurism; † and it was he also who suggested the possibility, even in other aneurisms, such as those of the innominate artery, of sufficiently checking the circulation by tying the carotid and third part of the subclavian to effect a cure, in spite of the presence of the four great branches from the first part intervening between the ligature and the heart.

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* The Successful Treatment of Internal Aneurism. London: Churchills. 1864.

* See the Croonian Lectures for the present year.

† Surgical Observations on Aneurism, eighth edition, 1825, pp. 303-4. I may add that I myself described the plan tolerably fully in the year 1862. (System of Surgery, vol. iii, p. 364, first edition.)

‡ Œuvr. Chir., ii., 568. Desault was restrained from operating, on the only fit case which he ever met with (one of axillary aneurism), by the fear that the sac, which seemed to be a thin one, would burst under the increased pressure which he thought would follow the operation.

How entirely the proposal of Brasdor had passed out of sight since the failure of the two or three cases operated on by his method, and detailed by Mr. Guthrie in his lectures at this College, may be judged by the testimony of Boyer. In Boyer's "Taité des Maladies Chir.," ii., 236, ann. 1845, edited by his son, le Baron Ph. Boyer, Brasdor's operation is thus spoken of:—"This operation was proposed verbally by Brasdor, professor at the old College of Surgery. Desault, in his lectures on pathology and operations, used to discuss the question whether it did not offer some chance of success in aneurisms inaccessible to the ordinary method; but this celebrated surgeon never put the matter to the test of experience when the opportunity offered." After the description of Deschamps' operation, at which Brasdor was himself present, and which was exactly that proposed by Desault, the editor adds a note in which he says that both Brasdor's proposal and the notion of distal compression were almost forgotten till Wardrop revived the distal operation; and he proceeds as follows:—"In spite of the success which has been obtained in about a quarter of the number of cases operated on, and in spite of the important difference established by Mr. Wardrop between those cases in which no collaterals exist between the ligature and the tumour and those in which they do, we are obliged to confess that this operation offers such great chances of failure that it ought not to be used in any cases in which other treatment is possible."

It is clear that the operation described by Desault is a very uncertain and dangerous one; it has most of the risks of the old method of Antyllus without its completeness and certainty, and in the present condition of operative surgery it has no precise application unless it be to innominate aneurism. Rossi's or Hobart's operation of tying the first part of the subclavian and the common carotid in an aneurism assumed to be purely innominate is strictly an application of the proposal of Desault or Brasdor; but neither of these surgeons thought of tying a healthy artery in healthy parts at a distance from the tumour, as Wardrop did in carotid aneurisms; still less had they imagined the novel and ingenious theory by which that surgeon endeavors to convince us that coagulation may be brought about in an innominate aneurism by tying the healthy carotid and the subclavian in its third part.

In order to form an opinion as to the merits of Wardrop's suggestion in cases of intrathoracic aneurism, I propose in the first place to discuss the theory of the matter, and next to see what support the method derives from pathological facts and from the experience of the cases in which it has been tried.

First with regard to the mechanical theory. Mr. Wardrop insists with indubitable force on the fact (too generally overlooked at his time, and I fear hardly sufficiently realised even now) that the ligature of an artery opposes only a temporary check to the circulation, and that the collateral circulation is re-established in so very short a time that there is no such lengthened period of total repose for the blood in an aneurismal tumour after the Hunterian operation as some have imagined. That this is so in animals can easily be proved by direct experiment. Thus Broca, in his work on Aneurism, p. 507, relates that a ligature was placed under the femoral artery of a dog in the groin, the leg was amputated at the knee, and the distance to which the pulsating jet from the divided artery extended during both the systole and diastole of the heart was carefully noted. The ligature under the femoral was now tied, by which of course the jet from the popliteal artery was stopped for the time; but the artery recommenced to bleed at the end of one minute, and at the end of five minutes the jet (which, however, no longer pulsated) had attained a quarter of its former maximum distance, in spite of the diminution of the mass of blood in the body. Porta,* again, relates an experiment on a dog, showing that after ligature of the abdominal aorta the collateral circulation was re-established to such an extent that fluid injected into the aorta passed into both femorals, and that the commencement of this process was perceptible almost immediately after the operation. Wardrop himself has related a striking proof of the rapidity with which the circulation in man is replaced after the ligature of a large vessel. "The enlargement of these [the anastomosing vessels] to a certain extent takes place almost immediately after the trunk has been tied. I observed this in a child in whom I had secured the carotid artery. I could see the branches of the temporal and occipital underneath the delicate integument enlarging, and thus actively acquiring great additional vigor, immediately after the operation." Nor is this by any means a solitary observation. In the case of temporary deligation of the abdominal aorta under the care of Mr. W. Stokes,† in which that artery was compressed by an instrument so firmly that on post-mortem examination no fluid could be made to pass down it, the pulse was nevertheless perceptible in the left femoral artery nine hours after the application of the instrument, though the man was dying. In Earle's case * of ligature of the subclavian artery the pulse was perceived at the wrist two and a half hours after the operation. In Mott's case † of ligature of the subclavian the pulse was perceived at the wrist on the evening of the operation. In Mr. Heath's case of double ligature ‡ the pulse returned in the temporal artery on the evening of the ligature of the carotid, and in the brachial two days after the subclavian had been tied. In Sir A. Cooper's second case of ligature of the common carotid,§ after the vessel had been tied with two ligatures and cut across, "the pulsation in the tumour had not entirely ceased, although it was so much diminished as to become obscure. I concluded it to be the effect of the return of blood by the internal carotid artery from the brain, in consequence of the free anastomosis which exists between the bloodvessels within the skull."

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* Hodgson on Diseases of Arteries and Veins, pp. 291-2. 1815.

† Wardrop (p. 59) speaks thus:—"When I first applied a ligature on the distal side of an aneurism, I considered that I had established an important principle in the treatment of those aneurisms in which no ligature could be placed on the cardiac side of the tumour, and at the same time no branch intervenes between that ligature and the heart."

* Alt. Pat. delle Art., p. 347.

† Dublin Quarterly Journal, Aug. 1869.

These facts are sufficient to prove that in man, as well as in animals, the collateral circulation commences very shortly (indeed, we might almost say immediately) after the ligature of the artery, and that in some cases it has attained so great a development in a very short time that the pulse has been felt at a great distance from the seat of ligature on the day of operation.

The effect of this diversion of the stream of blood is shown in some cases of Brasdor's operation by the remarkable fact that the aneurismal tumour becomes visibly smaller and more flaccid, instead of becoming more tense. Now the tension of the tumour would certainly increase immediately after the operation, if no fresh side sluices were opened near the dam which has been thrown across the stream of blood beyond it. This fact was first pointed out by Wardrop, who noticed it with surprise, and it has since been repeatedly verified. Its non-occurrence in a case of supposed innominate or carotid aneurism appears to me to be a fact of evil augury for the curative effect of the distal operation.

Thus we see that the problem is not merely a mechanical one, but that the effect of the collateral circulation, and the probable rapidity of its development, must be taken into consideration, and, in fact, are the most important features in the question.

Now Wardrop, in his original treatise on the subject, pointed out the difference that exists between cases where no branch intervenes between the ligature and the sac, and cases where there are such branches; and Hodgson, as we have seen, had previously noticed the same point.

We may take a purely carotid aneurism as an example of the former, and an innominate aneurism treated by distal ligature of the carotid and of the third part of the subclavian simultaneously, of the latter. In the case of the carotid the ligature, the artery below it, and the sac, are equally withdrawn from the pressure of the circulation by the enlargement of the collaterals, and the consequent derivation of the stream of blood (which used to pass through the tied carotid) into the subclavian of the same side and the opposite carotid. Theoretical reasoning, therefore, seems to show that in such a case the aneurismal sac is placed in the same condition as the portion of artery intervening between the ligature and the nearest collateral below it, and has therefore the same prospect of being occluded. That this prospect does not amount to a certainty we have plenty of evidence. See, for instance, such a preparation as this, from the museum of St. George's Hospital (s. vi., 147; also 1584 A, museum of the College of Surgeons). It is a fact that even when the vessel has been ligatured in the ordinary way with a silk thread, and therefore divided by ulceration, the portion obliterated is sometimes merely that which has been tied, and in such a case the aneurism may remain unconsolidated. But when, as in Astley Cooper's original case (see the plates 1 and 2, fig. 1 in Med. Chir. Transactions, vol. i.; also No. 1585 in the museum of the College), the clot extends down the whole length of the vessel below the ligature into the innominate artery, no reason that I can see exists why the same thing should not occur, supposing a portion of the carotid artery to be aneurismal; in fact, judging from the known tendency to coagulation in aneurismal tumours, the reason would seem to be all the other way.

There can then, I think, be no doubt that in aneurism limited to the root of the carotid artery Brasdor's operation will, in all reasonable probability, effect a radical cure; and in speaking of carotid aneurism on a future occasion I shall hope to adduce practical proof that the operation has been successful.

Matters are quite on a different footing, however, when there are collaterals of large size between the ligature and the tumour, as in the double distal ligature of the carotid and third part of the subclavian. These branches must increase at once, in order to carry on the collateral circulation. The size which some of them ultimately attain is shown in Mr. Heath's specimen in the museum of the College (No. 1596 A); and I see no reason to doubt that this size is reached very early. The reasoning, therefore, on which Mr. Wardrop based his proposal seems to me faulty, notwithstanding that it has been endorsed by the high authority of Mr. Erichsen, and adopted by many reasoners on this matter. Wardrop assumes that the carotid artery, the sum of the four branches from the first part of the subclavian, and the continuation or third part of this artery, may each be assumed to be equal in area; and that therefore, by tying the first and last, the tumour will be relieved of two-thirds of its volume of blood, and this, he thinks, will probably suffice for coagulation. It is surprising that his own most correct and ingenious observations on the rapid enlargement of the collaterals did not show him how very transitory this relief would be. In fact, the amount of blood circulating through the innominate artery, or through the aneurism, must be the same as before, with the exception of so much of it as is carried by the collaterals derived from the left carotid and the upper intercostal branches of the aorta. What the amount of this latter may be it would be very difficult to estimate; but it must be greatly under two-thirds, or even one-third. And I think we are justified in arguing that, after ligature of the caroti and third part of the subclavian, a more active stream than before must pass through the first part of the latter artery to supply blood to the thyroid axis and the other branches which have now to enlarge in order to take up much of the supply to the arm, neck, and brain; so that the complete consolidation of the

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* London Med. Gaz., vol. vi., p. 241.

† Amer. Journ of Med. Sciences, vol. vii. 1830, p. 309.

‡ Heath: On the Treatment of Intrathoracic Aneurism by the Distal Ligature, p. 11.

§ Med.-Chir. Trans., vol. i., p. 272.

innominate aneurism is impossible, under these circumstances, if the tumour be (as it usually is) a tubular enlargement of the artery itself. Mr. Fearn's case, however, appears to show that, even in such an aneurism, enough clot may be deposited to allow of a practical cure, the circulation going on through the centre of the clot.

Turning to the results of practice, they are twofold—viz., the inspection of morbid specimens, where the arteries have become obliterated beyond the aneurismal tumour, and the histories of cases in which the operation has been performed, especially with regard to post-mortem examinations.

The obliteration of one or the other of the two main branches coming off from an innominate aneurism is a very common post-mortem phenomenon, and I think there are sufficient specimens at hand to show that it is usually accompanied by obliteration of the portion of the tumour which is in connexion with the obliterated vessel. I will use as an illustration this drawing from the case of innominate aneurism which underwent spontaneous cure at St. George's Hospital (s. vi., 133 in St. George's Hospital museum). It will be seen that the carotid artery is entirely obliterated, and is converted into a solid cord from the sac to its bifurcation. There can be no doubt from the symptoms that this was the result of impaction of clot. The patient from whom this preparation was taken, George C——, a sailor, died on Aug. 24th, 1853, at the age of forty-four, from phthisis. The history of the case is given at length by Dr. J. W. Ogle in the Pathological Society's Transactions, vol. ix., p. 167. The aneurism could be traced back as far as to the year 1846, when he was first in St. George's Hospital under Dr. Bence Jones's care; but he had been suffering for some time previously from pain, which was considered to be rheumatic, and which was attributed to exposure to cold while steering a vessel in a storm. During his first stay in St. George's Hospital in the years 1846–7, no particular change seems to have occurred in the tumour. He returned in the early part of 1849, and while in the hospital he had the attack of head symptoms which indicated and accompanied the impaction of clot in the carotid artery. Those symptoms attracted much attention, and were carefully noted at the time, but the notebook has been destroyed, and all that is now known about them is that he became entirely unconscious, and, as is believed, hemiplegic on the left side with strabismus; that he remained in a critical condition for about a month, but that then he rapidly recovered; the pain, which had been great, was permanently relieved; the pulsation disappeared, and he left the hospital suffering only from debility and some little dyspnœa. Coincidently with the accession of the head symptoms the pulse disappeared from the right carotid. He remained free from all symptoms of aneurism, and was admitted again under Dr. Bence Jones's care in May, 1853, with well-developed phthisis. The only trace of the aneurism which then existed was a slight bulging of the parietes of the thorax over the tumour, and this bulging can still be seen in the preparation.

These notes point very decidedly to the explanation of the anatomical appearances: the expansion of the aneurismal sac being accompanied by intense pain; the impaction of clot in the carotid artery being followed by severe head symptoms and partial and transient paralysis; and the obliteration of the carotid being followed by coagulation of blood in that part of the sac through which the stream passed into the mouth of the carotid, while that portion through which the subclavian stream passed remains pervious. I will show presently that exactly the same state of parts may be produced by Brasdor's operation on the carotid artery in cases of pure innominate aneurism. And in commenting on the well-known case in which Mr. Evans, of Belper, tied the carotid artery on the distal method, I will try to show that the severe symptoms which accompanied the obliteration of the subclavian and its branches, and preceded the cure of the aneurism, depended, in all probability, as in this instance, on impaction of clot. In the case before us the cure was regarded as complete during the patient's lifetime; and, in fact, Dr. Ogle, on post-mortem examination, describes the sac as having been completely obliterated. It occurred to me, however, in recently examining this specimen, to cut a hole into the subclavian artery in order to see the reason of its preserving its size, and to ascertain the condition of its orifice of communication with the aneurism; and, having done so, and passed a probe down it, I found that the probe traversed the aneurism and passed down into the aorta; and it will be seen that the "subclavian portion," as it may be called, of the aneurismal sac is still pervious and unobliterated.

That the same thing takes place after distal ligature of the carotid for innominate aneurism, or for mixed aneurism of the aorta and left carotid, is shown by the cases operated on by Dr. Wright, of Montreal, and Mr. S. Lane. I only refer to these cases here to prove the identity of the effect of the distal ligature and of spontaneous distal impaction of clot. I shall discuss their general surgical bearing more fully in my next lecture.

In Dr. Wright's case* (which was purely innominate) the act of tightening the ligature produced an instantaneous alteration in the tumour, which became less tense and pulsated more feebly. After the patient's death, which happened eighty-seven days after the operation, "the aneurism was solid; and, upon division, its cavity was found filled with a hard mass of indurated fibrin, disposed in concentric laminæ, and of a buffy light-red color. The only communication through the aneurism was a channel, admitting a bougie, through which the blood flowed from the aorta into the subclavian."† By Dr. Wright's kindness I am enabled to exhibit an accurate drawing of the preparation, and it will be seen how exactly the state of parts corresponds in the aneurism in which the consolidation has been produced by distal ligature and in that in which it resulted from distal impaction of clot.

In Mr. Lane's case‡ the tumour was partly aortic; at least the sac extended down into the aorta, so that the mouth of the carotid artery was much dilated. After the ligature of the left carotid no diminution in the size of the tumour was noted, and its pulsation somewhat increased. Gradually, however, the size of the tumour diminished, and it became harder. A month after the operation it is noted that the tumour was "rapidly decreasing, and that there was pulsation merely at its inner and lower side." Nevertheless, the aneurism burst into the lung, and after death it was found adhering to the apex of the left lung, with which it had communicated by a circular ulcerated aperture. The effect of the obliteration of the carotid, however, was shown here, as in the former case; for the sac "was filled with firm layers of coagulum, except at commencement from aorta and part adjoining left lung." The carotid artery was obliterated from the tumour to its bifurcation. I can see no reason to doubt that the increased pulsation which in this case followed the operation was due to the increased stream which was passing into the left subclavian close to the mouth of the aneurism, in order to replace the ligatured carotid; and that the subsequent gradual diminution of the pulsation and size of the upper portion of the tumour proceeded from coagulation extending down into the sac from the tied artery.

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* The Treatment of Aneurism of the Arteria Innominata by Ligature of the Right Common Carotid Artery, p. 16. Montreal, 1856.

† Ibid., p. 25.

‡ Cooper's Surgical Dictionary, by Lane, vol. i., 214.

In connexion with this part of the subject the accompanying beautiful preparation which Sir. Wm. Fergusson has been so kind as to lend me will be found of much interest. It is taken from a case in which he tied the common carotid for innominate aneurism, the patient dying on the eighth day. It will be seen that hitherto there has been no formation of clot in the tied artery, but there is a good deal of laminated coagulum in the upper part of the aneurismal sac. If the carotid artery had become filled with clot, as usual, this would undoubtedly have joined on to the coagulum in the carotid, and thus all the upper portion of the sac would have been oblitered had the patient survived.

Precisely the same effect follows on the obliteration of the subclavian artery as it leaves the sac. This is shown by the accompanying preparation, for which I am indebted to the courtesy of the authorities of Westminster Hospital, in whose museum it is preserved. The preparation is one of extreme, I had almost said one of melancholy, interest, since it shows a condition of things which I think no one, after the evidence which I here produce as to the effects of Brasdor's operation, would deny to have been in all probability curable; yet the disease was allowed to go on and to end in the patient's death by bursting of the sac into the trachea; and this must, I think, have proceeded from the want of confidence which is commonly felt in the distal operation. The case is reported by Mr. Heath in the Pathological Transactions, vol. ix.; and it is curious that the same volume should contain so excellent an example of the effect of obliteration of each terminal trunk in innominate aneurism, the case of spontaneous cure (at St. George's Hospital) from obliteration of the carotid being reported at a subsequent page of the same volume.

In the present instance the diagnosis was most accurately and successfully made. The limitation of the aneurism to the innominate artery was at least thought highly probable. The obliteration of the right subclavian artery was inferred from the absence of pulse and comparative coldness of the right arm. The left subclavian was also obliterated, but this was not discovered during life, since the collateral circulation had been so completely reestablished that the pulse, though weak, was quite distinct. However, all the essential features of the case were thoroughly well ascertained; and if it had been an admitted fact—as I hope it will be henceforth—that obstruction of the subclavian artery as it leaves the sac will probably be attended by obliteration of the subclavian portion of the sac, and obstruction of the carotid at any point by obliteration of the carotid portion, could any indication in surgery have been clearer than to tie the carotid in this case? It is true that if the aneurism had extended down to the arch of the aorta the operation would probably not have cured the aneurism, but we can hardly doubt that it would have stopped its growth towards the windpipe, and so saved the patient's life.

Again, in the most instructive case which Dr. H. H. Davies has reported in the "London Hospital Reports," vol. i., p. 1, the same state of parts existed. There is here, it is true, the difference that the aneurism extended, as in the diagram from the case of tubular aneurism at St. George's, for a considerable distance along the tube of the artery, involving not the innominate only, but the greater part of the subclavian also. This terminal portion of the sac, however, was obliterated by the obliteration of the subclavian artery and its branches. "The end of the sac was solid with dense coagulum, and there was also much solid material at the posterior part of its thoracic portion. About the opening of the carotid, however, and from thence upwards and downwards, there was a considerable cavity unoccupied by clot." "Thus, then," says Dr. H. Davies, "the common carotid (right) was the only vessel still pervious which the innominate supplied. If this artery had been tied beyond the sac (at a part where it was itself quite healthy) the blood in the latter would have been quite stagnated. It must be noted that the only part of the sac in which the blood was not coagulated was the pouch near to the origin of the carotid. Nature had done half of the distal operation, and by far the more difficult half; and, as the dissection proves to our chagrin, nothing would have been easier than to have put the remaining fluid contents of the sac in a state of complete stagnation."

The accompanying preparation from St. Bartholomew's Hospital (ser. xiii., 69) is to the same effect, though it has been spoilt for our present purpose to a certain extent in making the section. It is cursorily alluded to by Sir W. Lawrence in the sixth volume of the "Medico-Chirurgical Transactions." In the Catalogue of the Museum it is stated that the opening of the subclavian artery out of the sac was obstructed, as it obviously has been, though the obstructing clot has been cut through in laying the artery open. Here again the subclavian portion of the sac is firmly obliterated, and I see no reason whatever to doubt that the aneurism might have been cured by tying the carotid. Another circumstance in this preparation which is worthy of notice is, that there is a large coagulum floating loosely in the sac. If a detached piece of this had fallen into the mouth of the carotid artery, the sac would, I have no doubt, have been finally obliterated. I believe this is exactly what took place in Mr. Evans's case, and I exhibit the preparation in order to show how easily such a cure might have been produced.

I cannot prove from dissections that the same obliteration of the subclavian portion of the sac would ensue if we could artificially obliterate the subclavian at its mouth, though there can be no rational doubt on the subject.

The cases in which the subclavian alone has been tied for innominate aneurism have been only two in number hitherto, and as in both cases the ligature has been placed on the third part of the artery, and branches have therefore been left between it and the sac, which must have instantly enlarged to carry on the circulation, neither of them is strictly analogous to the above cases, where the subclavian is obliterated at its origin from the sac.

But I may remark that both in Wardrop's and Broca's cases the immediate relief to the dyspnœa from which both patients were suffering severely at the time of operation, testifies to some diminution of the part of the tumour which compressed the trachea.

I trust that the above facts, which are all that time allows me to refer to, afford definite proof of the curative power of distal obliteration of the arteries in innominate aneurism, and show that there are some cases of this disease in which the operation is not only allowable, but imperative.

In my next lecture I will try to show that there are cases of aortic aneurism in which distal obliteration may prove beneficial, and will then review the experience already acquired of the distal operation for thoracic aneurism.

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ABSTRACT
OF
Clinical Lectures
ON
SURGERY.

Delivered at Guy's Hospital during the Session 1871–72.

By J. Cooper Forster, M.B. Lond., F.R.C.S.,

Surgeon to the Hospital and Lecturer on Surgery.

LECTURE V.

ON A CASE OF FRACTURE OF THE NECK OF THE FEMUR, WITH DEATH BY STRICTURE AND HÆMATURIA.

Gentlemen,—The operating theatre affords us nothing to-day wherewith to educate the eye; I therefore take the opportunity of discussing with you a case which has, I regret to say, disappeared from view by the death of the patient, and which most of you will remember as being possessed of a twofold measure of interest. It is that of John S——, aged fifty-one, admitted on Nov. 17th with an intracapsular fracture of the neck of the thigh-bone; and who, when all appeared to be going on well as regards the leg, died rapidly and somewhat unexpectedly from hæmaturia, consequent upon stricture of the urethra.

The history of the case is shortly this:—The patient, while occupied in moving some chests of tea in an iron-floored warehouse, slipped backwards, and fell upon his left buttock and hip. He found that he was immediately unable to stand, and he had also much pain in the hip. He was carried to the hospital soon afterwards, and when admitted chloroform was administered and a careful examination made of the injured parts. He was found to have an intracapsular fracture of the neck of the left femur, the symptoms on which he diagnosis rested being (1) a slight amount only of shortening; (2) eversion of the foot; (3) extreme pain.

A long side splint, reaching from the foot to the axilla, was applied to the limb and trunk, and some weights attached to the foot by a cord to make a slight but continuous amount of extension. With the exception of a rather severe attack of asthma, he was quite comfortable under this treatment till Nov. 21st, when a very urinous smell was noticed about him; and the next day the urine passed was thick and ropy, very offensive, and with a large sediment of mucus and pus in it.

Nov. 23rd.—At about 3 a.m. this morning he had a desire to micturate, and passed some urine, which was at first quite clear, but at the latter part of the act was high-colored and bloody. He was quite positive as to the fact that the urine passed first was clear, while the bloody urine came with the other, but later than it. Nothing more passed between 3 a.m. and 8 a.m., when again bloody fluid passed, and has now been passing at frequent intervals for two or three hours, with much pain and straining. The appearance of the fluid is almost that of pure blood, and it clots readily round any foreign body in the vessel. The bowels acted this morning after a dose of castor oil without pain. Apart from the urinary trouble, he does not seem ill. The dresser attempted to pass a catheter, but could not succeed in introducing even a small one.

24th.—He has had very much pain at times since yesterday, lasting often for an hour or more when a clot is coming; as these pass the pain is increased. Has passed a large quantity of claret-colored urine, but none clear since last night.

When pressed to-day as to his previous history, he gave the following details:—For the last year or two his urine has now and then scalded him much; it has also at times been gravelly, and often cloudy. The stream has been of fair size, and micturition easy and quite unaccompanied by any straining. Latterly the stream has been forked. He has not wished to pass it oftener than usual. He complains now of pain over the region of the bladder, and also specially in the urethra, extending from the anus two inches forwards. No dulness over the bladder indicative of any retention. Ice applied to the hypogastric region relieves the pain. The urine is still very offensive. No more clots have come away; but a slimy jelly-like matter settles at the bottom of the vessel. He was ordered ten minims of tincture of opium and three ounces of infusion of matico every four hours, and the bladder to be washed out if possible. The latter part of the prescription was, however, found to be impracticable. From this time he continued to bleed up to the time of his death, though towards the end the water became more like a mixture of blood and urine. He died Nov. 27th, eleven days after admission, and five after the commencement of the hæmorrhage.

At the post-mortem, the kidneys weighed fifteen ounces. Their capsules were thin, though perhaps rather too adherent. The pelvis of each was ecchymosed, indicating some amount of pyelitis; and the calices were dilated, with flattening of the pyramids, as if some obstruction had existed. The bladder and penis were taken out together. The former contained a pint and a half of purulent urine of a very offensive nature; its cavity was large, and the muscular wall hypertrophied; the mucous coat, though apparently entire, was in a most intense state of inflammation, and was coated everywhere with thick adhesive mucus, and in many places by a layer of calcareous phosphatic matter. It was also deeply congested, and over one large patch at its posterior wall was of a dark-blue color. I dare say many of you will call this to mind from having seen it in the post-mortem room. On slitting up the urethra, the lining membrane was arborescent in its vascularity, and coated with earthy matter as far forwards as half-way along the cavernous portion. Here there existed a tight stricture, which would not admit of the passage of a probe. Behind this the channel was much and uniformly dilated till just where the cavernous bodies terminated posteriorly. At this point the lower wall of the urethra was dilated, forming a pouch which extended backwards, along the floor of the canal, towards the bladder, for nearly an inch. This sac contained several secondary pouchings or lacunæ, but by none of these could the probe be made to pass out of the urethra, and the lining membrane appeared to be at all places entire. Immediately beneath this pouch the tissues were all in a gangrenous condition and broken down into a claret-colored pulp. The scrotum around was extensively gangrenous. No aperture or orifice of any vessel could be found to account for the extensive hæmaturia which the patient had undergone during life. On dissecting the parts around the left hip-joint, the muscles were found somewhat bruised, more especially the psoas, iliacus, and vastus internus. Neither the muscles nor the capsule of the joint were lacerated. The great trochanter, and with it the shaft of the bone, was displaced slightly (within one inch) upwards and backwards. A rough abnormal prominence was seen and felt just in front of the small trochanter, well covered by fibrous structures forming part of the attachment of the capsule, in which blood was extravasated. The extravasation was continuous from this point down into the vastus muscle. On cutting the capsule all around close to the acetabulum—the muscles having been all singly and carefully divided—and detaching the round ligament, a slit of about an inch could be seen in the fibrous covering of the neck of the femur at the upper part. Through this a fracture of the neck was visible, the two surfaces being rough, though not very so, and some delicate bands passing across the fissure from one surface to the other. On passing the finger within the capsule, around the neck, the whole line of fracture could be felt, except about an inch at the underpart, where it appeared to have extended under the attachment of the capsule. From four-eights to five-eights of an inch was all the displacement that could be effected, even at this stage.

And now for a few words about the injury, and a few on the disease.

In calling attention to the fracture, I ask you first to notice the way in which it was produced in this instance. It was by a direct fall upon the buttock. This is very unusual in the class of cases to which I am referring: they are generally caused by a much less severe hurt. By a direct fall upon the hip one would expect rather to get an impacted fracture, the bone being broken, and one fragment driven into the other; or if not that, then that the parts would be much crushed and comminuted. But an intracapsular fracture, on the contrary, is generally produced by some indirect violence, such as a trip or tumble; the fracture being caused by the attempt to recover from or avert a fall. In this way an aged person by merely catching the foot in the carpet will sometimes fall helpless to the ground. Another unusual feature in the case is the age at which the injury was produced. Fracture of the neck of the femur almost always occurs in old people; the age of this man was only fifty-one. Now we can hardly call fifty-one old; indeed, as far as I recollect, this is almost the youngest man in whom I have seen such an injury. Next, we come to the all-important question—supposing you to have been called to such a case as this, what are the points which would lead you to recognise it? What were they which enabled my dresser to do so? It does not require much thought to enable you to suggest at once the chief symptoms. And, first of all, you would say that, in common with all other fractures of the lower extremity, an intracapsular fracture, except in very exceptional instances, causes an inability to stand. Secondly, pain may be mentioned, which is generally very severe when the limb is moved. This symptom, however, may be present in a bruise or a dislocation, though these are not generally accompanied by any such severe amount as is found in hip-joint fracture. Thirdly, we have eversion of the foot—the toes turn outwards. This arises probably from the natural inclination of the leg when at rest to roll outwards. The patient with fracture has no power to move his muscles; the leg is quite useless, and he cannot control it; and the foot naturally turns out, from the falling backwards of the femur or the rotation of its head in the segment of a circle. I used to be taught that this eversion was due to contractions of the glutei, or to the action of some other muscle, according as my teachers held this or that view of its cause. But, gentlemen, I do not agree with either. It is probably much more correct to say that the muscles, having lost their fixed points of action, refuse to steady the limb as is their wont in the normal state of the part, and the foot naturally rotates outwards. The same thing occurs, though to a much less extent, in health whenever we lie down in bed, when the foot at once turns outwards. In very rare cases a rotation inwards of the leg has been noticed, and possibly this may be the result of muscular action at the time of the accident, or of a peculiarity in the line of fracture. Shortening of the limb is another symptom of intracapsular fracture; and, as with the other points that have been mentioned, this also is common to all fractures. If with a history of injury in a previously well-formed frame we find shortening of the leg, then we are quite sure that some solution of continuity of bone must exist. Coupled with the features that have just been mentioned, shortening of the limb means fracture with displacement of the fragments; while by the extent of the deformity we judge in great measure as to where the fracture exists. The amount of shortening in intracapsular fractures may vary from next to none at all up to three-fourths of an inch, or at the outside an inch. If you think of the parts, you will see that by no possible means can it be more than this, as the capsule will not allow the pieces to separate further; and in most cases, unless the measurements be taken very carefully, it is hardly noticeable. In fractures occurring close to the neck, but yet outside the capsule, there may be two or three inches difference in the length of the broken limb as compared with the leg of the opposite side. Remember, then, that in an intracapsular fracture, while shortening is generally present, it is the slight amount of it which must arrest your attention and put you on the right track. I shall only ask you to bear in mind one other diagnostic mark; but that one is very useful at times, though not usually noticed, unless the patient is fully under chloroform. I refer to the diminution in the arc of the circle which a femur fractured through its neck makes when inwards or outwards, compared with the segment of the larger circle it describes when the shaft is continuous with the head of the bone. In health, of course, when performing the action of rotation, the upper part of the bone describes a circle of which the acetabulum is the centre and the outer part of the trochanter major the circumference; but let the bone be broken through its neck, and the circle described will then have the fractured point for its centre, and, as before, the outer part of the trochanter for the circumference. The radius of the circle will then manifestly be diminished to the extent of the head of the bone which has become detached from the shaft. The excessive pain, however, which these patients suffer renders them unwilling to be pulled about, except when rendered insensible; and therefore you will not often be able to avail yourselves of this last symptom. Still, as I say, it may be of use sometimes, and, with the others that have been pointed out, you will, I apprehend, have no difficulty hereafter in recognising this lesion. "But," I hear some one say, "nothing has been said about crepitus,—is not that a point worth looking out for?" No, there is no necessity to get crepitus; the other symptoms are quite sufficient in the great majority of cases, while, if you do try to obtain information from this source, in very many cases you will not succeed. In exceptional cases, perhaps, it may sometimes afford a valuable corroborative proof, and in such you will be most likely to obtain it by pulling the femur well downwards before attempting to rub it against the head of the bone. But do not *expect* to obtain any grating, and, above all, do not go away doubtful as to your diagnosis from the mere fact of not having felt it.

Then you will be expected to give some prognosis in these cases; and what is it to be, favorable or the reverse? In a man of the age of our patient it would most certaintly be favorable; and I may tell you that, unless it be in very old people, over eighty, or in others who have damaged their viscera in early life, it should, as a general rule, be a hopeful anticipation rather than otherwise, as these cases generally do well.

And now we must pass on to the more important disease, which in this instance unfortunately caused the death of the patient. The man had been lying in bed quietly, with no sign of any approaching danger, when, six days after his admission, he suddenly passes a large quantity of bloody urine, and when I saw him the next day I was astonished at the quantity of blood the man had passed—two large vessels full of nearly pure blood. Where did it all come from? This was the question I had to decide upon; and it is the one which, I should think, would and did actually occur to many of you when you saw the case with me in the ward. That it had nothing to do with the injury was pretty clear from the late date at which it occurred after the fall. Any traumatic origin would most probably have manifested itself, or its effects, without allowing six days to elapse. Putting aside the hurt he had received, we, then, knew that the blood must come from one of three sources—kidney, bladder, or urethra. First, did it come from the kidney? Renal hæmaturia is principally decided upon by the state of mixture between the blood and urine. If the two are intimately combined, then probably it will be kidney; if not, then more likely the bleeding source will be found nearer the external parts. Another very important means of deciding is to carefully examine the blood for clots, for very possibly the shape of these, should there be any, may determine the site of hæmorrhage. As it happened, neither of these observations helped us in this case, because no clots of any definite shape were passed, and the blood was very intimately mixed with the urine; although, from a point in the history which the man gave us, it could hardly have come from the kidney. He told us that, when the attack first commenced, clear urine came away before any blood. It was only at the end of the act that hæmaturia began. This one fact put the kidney entirely out of court. Having excluded that organ, we still had to decide between bladder and urethra. In the case of the former, however, as a matter of experience, we know that the two great causes of hæmorrhage are malignant disease and stone. Both these cause very urgent symptoms, which were conspicuous by their absence; the man had had no trouble with his waterworks. The next point to be determined was whether an instrument could be passed into the bladder, and we found that it could not by reason of a very tight obstruction not far down the passage. I was therefore inclined to the opinion that the blood which was passing away came from the urethra; but so unusual I knew this to be that I left the bedside obliged to exclude the bladder and kidney as sources of hæmorrhage, and yet very unwilling to accept the fact that the urethra must be the *fons et origo mali*.

Well, the bleeding went on in spite of our efforts to check it, and the man died. At the post-mortem, as you have heard in detail, there was intense cystitis, a tight stricture existed half-way along the cavernous portion of the penis, and the urethra was dilated considerably behind this. Behind the corpora cavernosa the lower wall of the urethra was dilated into a sac, beneath which the subjacent tissues were becoming gangrenous, in common with part of the scrotum. No aperture of any vessel could be found to account for the hæmorrhage. Thus you will see that even after death some difficulty arose in accounting for the bleeding. It was suggested in the post-mortem room that all might have come from the bladder, one portion of the mucous membrane of which was of a dark-bluish color; and to support this theory the stomach was adduced as a similar example of a mucous membrane pouring out a large quantity of blood. The stomach, however, does not afford an analogous case, because it only exudes these large quantities of blood when its circulation is obstructed from other diseases. I felt unable to admit that any active inflammatory condition such as existed in the bladder could possibly have got rid of so much blood from its surface. Moreover, I thought the hæmorrhage was quite fairly explained in another way.

In all cases of stricture of any duration, if the passage be much narrowed, the urethra becomes dilated behind the obstruction. This case formed no exception to the rule, and you find it noted that a pouch existed in the inferior wall or floor. When such pouches have been in existence some time, ulceration frequently occurs in their lining membrane, and a small extravasation takes place. I believe such a sequence of circumstances occurred in this patient. He complained, you will remember, of soreness at one particular spot in the urethra; and here, I believe, some ulceration took place which probably weakened, if it did not open, some large vein near the prostate, of which there are so many, and that the bleeding then commenced. In this way can one explain the fact that the urine passed at the commencement of micturition was clear, whilst when blood once began to flow it never afterwards ceased. The gush of urine probably partially emptied the bladder, and then came some squeezing and straining to produce a thorough evacuation; the vessel was torn, and the hæmorrhage began. Mark you, it is an important point in the history that this pretty clear and partly bloody urine only occurred once. It did not recur every time, more or less, as is usual with bladder cases. This explanation of the fact is further corroborated by the extravasation which occurred shortly before death. It appeared to have taken place exactly at the spot he had complained of during life; and though no opening from the urethra could be discovered, one must have existed somewhere to enable the urine to escape. A tight stricture existed in the urethra, so that even a probe could not be passed. This is an interesting fact, and one worth remembering. Dr. Moxon remarked in the post-mortem examination that it was not the first time he had seen a very narrow stricture in a man who had never complained of anything of the kind during life. I have myself met with such instances several times. This man, when pressed closely, only admitted that the stream had been forked. He had had no other symptoms of stricture, not even frequent micturition. I may here diverge slightly to remark that frequency of micturition, though a common symptom of stricture, may exist with a quite healthy bladder; indeed, the frequency or not with which each person relieves his bladder is quite a matter of education. If it be evacuated frequently, the habit will increase upon the person, and by simply taking every opportunity of micturating a man may ultimately get such an irritable bladder that it positively will not hold its contents longer than a very short time. I have known men who have told me, when they came for advice as to this very complaint, that they used always to empty the viscus whenever they could, so that in case they were placed in situations in which the opportunity was wanting, such as ladies' society or urgent business, they might start with the bladder empty. No man ought to pass his urine more than four or five times in the twenty-four hours, and be sure that the oftener the act is performed the more frequently will the desire to urinate return.

Lastly, a few words as to the treatment adopted in this case, both for the fracture and the hæmaturia. When admitted the patient was placed under chloroform that the injury might be thoroughly examined. This was a very wise precaution on the part of the house-surgeon, and quite the proper thing in a hospital if any doubt exists as to the nature of an accident; but I ought to tell you, though all very well here, it won't do in private practice for you, directly you are called to supposed case of fracture, to suggest the administration of chloroform; you will find people are not quite so fond of it; they think a good deal of the risk run, and therefore you cannot use it.

Having made your diagnosis, what is the next thing to do in a case of fractured femoral neck? I advise you to put the patient on a hard bed, or what we call a fracture bed—that is to say, on a mattress that will not give much with the patient's recumbent weight, and then at once to apply a straight side splint. I say *at once* because I can see no reason for postponing the application of a splint for eight or ten days till inflammation and swelling have subsided. On the contrary, and this applies to all fractures, the sooner the limb is put up after the injury the less swelling and inflammation will there be. It is in the very cases that are left longest, with the rough fragments of the bone constantly rubbing and tearing the muscles so well supplied with blood, that so much swelling occurs. This man had, in addition to a long splint on the right side of the thigh, a weight attached to his foot. This is a good plan for making extension, and now much used at all hospitals; the cord attached to the foot should run over a pulley at the end of the bed, to obviate as much as possible any friction that may occur, and the bed should be tilted at the foot so that the head may lie lowest. In this way the body acts as a counter extending force, and the full advantage of the weight is gained, which is often not the case when the patient, lying level, slips down as the weight pulls.

What did we then do for the hæmaturia? I am sorry that it was not in our power to do much. He was ordered ice to the region of the bladder constantly, and we gave him internally styptics. He took three ounces of infusion matico with ten minims of tincture of opium every four hours. I believe that in this class of cases the various styptics are very valuable remedies, and matico is one of the best. At one time during my absence, being very much depressed, he was ordered some brandy, and I must say that I could not altogether approve of it. Cases of hæmorrhage require very careful management with regard to stimulants. On the one hand you have to guard against fatal collapse, and on the other against any excess of circulatory activity when reaction is established. I was not sure in this case whether the brandy might not possibly increase the hæmorrhage, and therefore I did not continue it. Be careful in these cases. In all abdominal injuries with rupture, however slight, of any of the viscera, there is always much collapse. The public are very fond of pouring in the alcohol, and it certainly rallies the patient; but with the reaction comes also a quickened and excited blood-current, forcing out any clot which may have become lodged during the time of the feeble or nearly stagnant flow, and ensuring a fatal termination, whereas perhaps if the case had been left quietly alone the rent might have become consolidated, and a valuable life saved. Do not yield to the public in their ever-ready advice in the way of stimulants, but let any case where you fear ruptured viscera lie quiet and collapse if need be. Do you nothing more than to guard against the tendency to death, avoiding stimulants except as a last resource.

linical Lecture

ON

OTOMY.

oke's Hospital, Cam-

PHRY, F.R.S.,

of success in ope-
s in life, is a painstak-
of procedure; and in no
ore true than in lithotomy. In
y of the patient depends, perhaps
rectly than in any other, on the manner in
hich it is performed. Accordingly, when, thirty years ago, having just emerged from the period of pupilage, I had the good fortune to be appointed surgeon to this hospital, where I knew I should often be called upon to perform lithotomy, I was much impressed with the responsibility of the task, and set about seriously to consider the mode in which it could best be done; and I laid down for myself a plan to which I have ever since closely adhered. A careful investigation of the subject and an investigation of the different modes of operation practised by the most eminent men convinced me that in this, as in most operations, the minimum of deep cutting was the safest course and gave the best results. Reading confirmed what observation had shown me, and what reason had suggested, that, not the brilliant, but the cautious operators were the most successful. I came to the conclusion that in deeply seated parts it is better to make nineteen or twenty cuts, amounting in the aggregate to nine-tenths of an inch, provided that is sufficient, than one cut amounting to an inch. The extra tenth will, now and then, turn the scale against the patient, though the quicker proceeding may seem to tell in favor of the operator.

You saw on Friday last the plan I am in the habit of following. The patient, a middle-aged, healthy man, was admitted on the previous Wednesday, and had not therefore undergone any preparatory treatment, which I think as often does harm as good by depressing the spirits and lowering the bodily tone. He was not very clear as to the duration of his symptoms, but it appeared that they were not of long standing, and the sounding indicated the stone to be small. Why, then, did I not perform lithotrity? I believe it would have been a good case for it, and I was half disposed to do it. But a good case for lithotrity is one in which lithotomy, carefully performed, is attended with very little risk, with quite as little, I believe, as lithotrity. It is less painful, quicker, and more certain; and the greater number of our patients prefer this more expeditious process. I use the common staff with the groove on the convex side in preference to one in which the groove is placed laterally, as recommended by Mr. Liston, because the groove, when on the convex side, is more easily felt in the perineum, and the knife is more easily pressed into it and is less likely to escape from it. A common scalpel is quite as good as any of the variously formed knives which have been employed in this operation; and it is advisable to adhere to a few instruments, just as it is advisable to adhere to a few remedies. By using many of either we merely dilute and waste our experience. The man who uses a few weapons is more likely to wield them skilfully than he who tries, and so perplexes himself with, many. Having made a tolerably free incision through the skin and superficial fascia, I pushed my left forefinger, through the tissue of the perineum, down upon the staff a little behind the bulb, so that I could feel the groove of the staff sufficiently to guide the point of the knife upon my finger-nail into the groove. I then cut a little way along the staff, withdrew the knife, examined with my finger, and, again introducing the knife as before, cut a little more, repeating the process till the part of the prostate nearest to the surface was divided, and till I could insert my finger, alongside the staff, through the prostate, into the bladder. Thus the opening was dilated. It was further dilated by my passing the forceps, beside the finger, into the bladder, which I did after having explored the bladder and felt and ascertained the size of the stone. I make a point of doing this, as it greatly facilitates and gives safety to the further steps of the operation, and, having the advantage of a long finger, I am almost always able to examine the size and position of the stone, and select the forceps accordingly. These should be so made as to hold the stone securely and not let it slip from between the blades; and the blades should be broad, so as to cover the stone and prevent its sides catching and rubbing roughly against the tissues as it is drawn through the wound. In the present instance the forceps did not open easily in consequence of the hinge not being quite free. To have used force in opening these might have endangered the wall of the bladder; so I at once withdrew them and resorted to the scoop, which I guided upon the finger again introduced, and with which the stone was quickly removed. An elastic tube, guided upon the finger into the bladder, was secured there, and removed on the third day. The man has not had the slightest pain or discomfort since the operation, and is doing well.

The points on which I lay most stress are, the pressing the finger down upon the staff in the membranous part of the urethra and guiding the scalpel by it into the groove, the careful cutting into the prostatic part of the urethra, and the completing the operation with the finger and the forceps or the scoop. I rarely do more cutting than this. If it is a large stone, I divide the prostate a little more freely with the knife, and then allow the wound to be dilated by stretching or tearing of the tissues as the stone is drawn through it, which I do slowly and steadily, though I have sometimes to use considerable force. By this plan there is no greater division of the tissues than is absolutely necessary, and there is less danger of ill consequences than if the knife is resorted to. I ought to add that it is necessary to be very careful not to allow the finger, still less the knife, to stray into the loose tissue on the exterior of the prostate. Particularly is this care needed in operating upon children. I am sure that many of the disasters which have occurred in the operations upon young subjects have been caused by the finger pushing its way on the exterior of the prostate and bladder, where the tissue easily gives way before it, and where a cavity is easily made by the finger, which the operator mistakes for the bladder. Into this he plunges the forceps, perhaps seizes the bladder or the rectum, or does some fatal mischief. In a little child it is sometimes difficult to enter the bladder. The tissues are soft, and readily give way before the finger; the bladder seems to be at an unexpected distance, and the operator is unnerved by the difficulty. I can give no more valuable hint than that of preparing a young operator for this difficulty, and of warning him to be content to go slowly on, not to be seduced into quitting the guidance of the staff, not on any account to withdraw the staff till he is certain that his finger is in the bladder, and till he feels the stone with it, but to work quietly and gradually on with finger and knife till he has brought the finger into contact with the stone. Indeed it is a good rule, from which I hardly ever depart, not to allow the staff to be withdrawn from the bladder till the finger is upon the stone, and not to withdraw the finger till the forceps or scoop is in the bladder. I do not use a very large staff, because it increases the difficulty of passing the finger along it through the prostate. I introduce it with a slight wriggling, rapid, but gentle movement. I am not satisfied unless I hear it strike the stone as well as feel its contact with it. The ear is in this case a better guide than the finger; and if this were more attended to, the cases in which a stone is not found would be fewer. The touch is often deceptive, especially in children, in whom the projecting and slightly roughened rugæ of the bladder frequently communicate, when the sound is passed quickly over them, a sensation to the finger like that given by a stone. I was present not very long ago when an operator opened the bladder and could not find a stone. To my inquiry, two or three times repeated, "Did you hear the staff strike upon the stone before commencing the operation?" the reply was, "Oh, we felt the stone quite distinctly; there was no doubt of that."

Rules in surgery, however, must sometimes be departed from, and judgment and skill may be more shown in the breaking than the observing them. A lad was in the hospital two years ago with severe symptoms of stone, the urine constantly passing. The sound appeared to grate upon something, but we could not hear it strike a foreign body. Several trials gave the same result. The symptoms were so bad that it seemed best, at any rate, to open the bladder and search for the cause. Accordingly, having introduced the staff as usual, and perceived the grating, I cut carefully upon it and along it quite to its end. I could not discover a stone, and I soon became certain that neither my finger nor the staff was in the bladder. The staff, being no longer of any use, was withdrawn. Then my finger touched the end of a rough stone. Supposing that this had escaped from the bladder into the surrounding tissue, I was endeavoring carefully to extract it when it disappeared; and my finger, following it, passed through what was evidently the prostatic part of the urethra into the bladder, and felt the stone there. It was removed without difficulty. It is, as you see, composed of two oval parts, united at an angle of about 100 degrees. Of these one has a nucleus of lithic acid, and lay in the bladder; the other consists of phosphate of lime, and lay in a cavity or urethral pouch outside the bladder; and the bent neck joining the two occupied the prostatic part of the urethra. The staff, no doubt, had passed into the urethral pouch, beneath the calculus, had probably penetrated the delicate wall of the pouch, and found its way into the cellular tissue behind the bladder, into which I travelled along its groove with scalpel and finger. The stone lying in its concavity was not discovered by my finger till the staff was withdrawn. The boy quickly recovered.

You observed last Friday that I quickly relinquished the forceps, and resorted to the scoop. This is an excellent instrument and very safe, because it does not go much beyond the range of the finger. It is not trusted in the bladder without the finger, and is not opened and shut and moved about in the bladder so freely as the forceps are; and, with a little practice, the stone is very easily caught between it and the finger, and drawn through the wound. Even large stones may be thus removed. This stone weighed eight ounces. It is the largest I have ever seen extracted by lithotomy. It is nearly spherical, is rough on the exterior, and was grasped by the bladder so that I could not lay hold of it with the forceps, and should probably have failed to complete the operation had I not fortunately taken with me (it was several miles from Cambridge) this strong well-bent scoop. I contrived to insert it between the bladder and the stone; and it held to the stone so well that with it and my finger I succeeded in gradually drawing the stone through the wound. The patient, a stout farmer, aged sixty-five, recovered without any unfavorable symptom. The handle of the scoop should be large and rough, and the stem round and strong. In some of these scoops I have had the bowl set sideways on the stem, which sometimes facilitates the catching of the stone between it and the finger.

With regard to hæmorrhage: I am in the habit of telling my pupils that hæmorrhage after an operation is almost always attributable to some fault on the part of the surgeon. It indicates that he has not been sufficiently careful in searching for the bleeding vessels, or that he has not properly secured them. This is, at any rate, a very safe doctrine to inculcate; and it applies to lithotomy as well as to other operations; for the vessels which bleed most briskly, and are most likely to give trouble after the patient is placed in bed, are commonly within reach, and may, with care, usually be found and tied. They are the artery of the bulb, or the transverse artery, or some branch divided near the pudic; any of which may be tied. After the removal of the stone I always attend to this point, and sponge out and explore the wound carefully, if there is any bleeding which is likely to give trouble. When the hæmorrhage is from a deeper source than I have indicated—that is, from the neighborhood of the prostate—it cannot always be arrested; but in such cases it usually ceases spontaneously after a short time, there not being commonly any arteries in this situation sufficiently large to keep up, or to cause a return of, the bleeding.

I leave an elastic tube, passed through the wound, in the bladder, in men, to avoid the difficulty which is sometimes experienced by the patient in voiding urine after the operation. I do this because I was, in several instances, summoned, after a few hours, in consequence of the patient being in severe pain and unable to pass urine; and I was obliged to introduce my finger into the bladder to afford relief. This trouble, which was perhaps a consequence of the small size of the incision I am in the habit of making into the bladder, is quite prevented by the tube. The tube may be removed in two or three days, or earlier if desirable, and is not again required.

In one instance, a gentleman, aged sixty, in whom the operation was unattended with any particular circumstances, there was rather severe inflammation of the bladder ten days after the operation, with discharge of tenacious and semi-purulent mucus. For several days I introduced, two or three times a day, an elastic catheter by the urethra into the bladder, and injected through it warm water, which, passing out at the wound, washed out the bladder. The symptoms subsided; and he was soon quite well, and has remained so. In another case, when the wound was healing, the patient, a farmer, aged sixty-four, had retention, and required the catheter to be introduced two or three times daily. After a time he quite regained the power. In a third patient, a middle-aged man, in the hospital, the symptoms of bladder irritation set in while the wound was healing, and increased in severity, so that I introduced a sound, and, discovering a calculus, I reopened the wound, partly with the knife directed by the staff, and partly with the finger, and removed a rough phosphatic calculus, which I have no doubt had formed in the interval since the first operation, when the stone removed was lithic acid. I mention these cases because it is possible that the symptoms may have been dependent upon the incision in the prostate having been small, and the parts having, in consequence, sustained some injury in the extraction of the stone. I do not know that it was so; and if it was, the occasional occurrence of such events would not induce me to deviate from the practice I am in the habit of pursuing.

With regard to the after-treatment, it is very seldom that any is required. The diet should be, not according to any rule, but regulated by the condition and the desires of the patient. He usually prefers to limit himself to tea, broth, &c., for a day or two; and this generally suits him best. Then appetite returns gradually, and may be yielded to. We do not often find that wine is necessary. The man now under treatment, as I just said, has had no pain or discomfort whatever. Generally some pain is experienced when the urine flows, or at other times; but whether there is much pain or little, whether the patient sleeps or does not sleep, I refrain from giving opium or any other sedative. Pain, even severe and long-continued, though hard to bear, does not seem to do the body much harm. I have often been surprised how little wear and damage it does—not so much, I think, as the sedative which is given for its relief; and the exhaustion of a sleepless, restless night or period is usually followed by sleep, which is more likely to occur naturally at an early period if sedatives have not been used. Under ordinary circumstances a restless night is less damaging than a night of sleep induced by sedative. The patient commonly wakes from the latter paler, weaker, lower, more enervated, and less able to bear pain. I have long had this conviction; indeed, throughout life I have acted upon it. It may be regarded as a prejudice; but I think the patients recover better and quicker, and are less liable to pyæmia, erysipelas, and other unfavorable sequelæ of operations, when thus treated. These, at any rate, we rarely see in Addenbrooke's Hospital; and an immunity from them is, I think, partly due to the practice of non-interference after operations so generally followed in this hospital. You will understand that I make a distinction between the use of opium as a remedy for disease and as a means of relieving pain after operations. In the former case it is a very valuable medicine under many circumstances, one of the most valuable we possess. But in the latter case there is no disease; the pain is merely a natural consequence of the physical injury which has been sustained, or of the local changes which are taking place as a consequence of the injury. The condition will generally subside after a short time; and it is much better to allow it to do so, unless there is some special reason for interference.—It is well to suspend the scrotum, especially in elderly persons, so that it does not become moistened with the urine, to keep the parts as dry as possible, and to defend them by the occasional use of oil. I do not remember any instance in my practice in which the wound failed to heal soundly within a few weeks, or in which any unpleasant symptom, except slight difficulty in holding the water, followed; or any case in which the rectum was wounded, or in which any troublesome local symptom remained.

Clinical Lectures

ON

INFANTILE PARALYSIS AND ITS RESULTING DEFORMITIES.

By Richard Barwell, F.R.C.S.,

Surgeon to Charing-Cross Hospital.

LECTURE III.

However valuable it may be to learn that infantile paralysis is, in its early stages, an affection essentially curable by the two forms of galvanism, yet it must be acknowledged that cases very rarely come under proper treatment until this easier phase of curability has passed away. A better knowledge and insight into the natural history of the malady may cause it to be more generally attacked in time; but it is too much the prevalent habit to believe in a temporary paralysis on the one side, or simply in incurability on the other—to begin by buoying up delusive hopes, and then to foster a vague despair, and, while doing nothing material, to allow all the best hours of action to pass away.

With the lapse of time the limb loses power, not merely of movement, but also of circulation and nutrition; it becomes permanently cold and bluish in tint; it stops growing, and soon is shorter as well as thinner than the sound one. If the whole leg, for instance, be affected, it has a singular knack of outward rotation in standing, or in sitting of rolling outwards from the nurse's lap to such an extent that the inside of the thigh presents forward, and often the sole of the foot, the toes being pointed, is visible from the front.* In this stage, though certain muscles may retain some irritability, deformities are arising with great rapidity, and permanent lameness already has commenced to assert itself. Yet even now cure is quite possible, but will require longer persistence of treatment, and the peculiar use of strychnine, after the manner whose introduction to the profession I will shortly describe.

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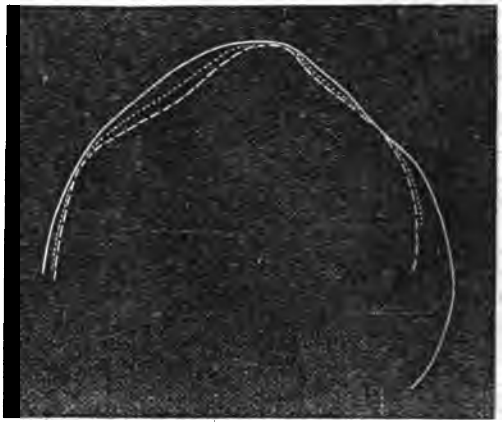
* This is the "jambe de Polichinelle" of French authors.

Being strongly convinced of the peripheral origin of infantile paralysis properly so called, yet finding that in cases of some little standing no progress was effected by galvanism, neither was the internal use of strychnine followed by any benefit, I determined to employ the drug locally. For this purpose I injected into the moveless muscles or their neighborhood the Pharmacopœial solution of strychnia. Here, too, I was disappointed; the topical effects were nil, even while on the verge of constitutional symptoms. It appeared to me that I must retard absorption, and thus, while keeping the drug longer in the part to be influenced, avoid its action on nerve centres. I therefore procured a very dense solution—viz., two per cent.,—and began a series of cautious experiments. I used a syringe with a piston graduated into half-minims, over which runs a screw stop; each turn and a half of the screw corresponds to a half-minim, hence each such quantity can easily be divided into six parts, and, with more care, into twelve; as each half-minim of solution contains 1–100 gr., subdivision is rendered easy and all precautions facile. In my experiments, however, I very soon discovered that, thus largely dissolved, strychnine might be safely injected in much larger quantities than had been imagined, and that I could fearlessly begin by giving even children five times the dose hitherto considered as the maximum—namely, five half-minims, or one-twentieth of a grain. But it was not alone with children that I thus worked: all local paralyses appeared to me proper subjects for these experiments. The results are singularly confirmatory of the idea which I had formed, that eccentric paralyses would be very amenable to the local action of strychnine. Thus, several cases of facial paralysis, several of paralysis of the wrist (independent of syphilis or of lead-poisoning), and others, have yielded to a few injections, after months of galvanism, internal administration of strychnine, and other remedies have failed. Several of these cases have been published; * here I will give only two short *résumés* in illustration of the power of the drug thus employed.

J. G. M——, aged thirty-eight, consulted me on the 14th January, 1872, on account of lameness from paralysis in both legs. In the early part of 1865 he had, at Malta, Mediterranean fever, which appears to be a variety of gastric fever. This was followed by acute rheumatism without heart affection. The recovery was slow, with many relapses, He does not know what muscles or limbs may have been paralysed when he was in bed; but when he began to get about, all the muscles in front of the tibiæ and all those in the ball of the thumb had lost power, except those inside the long flexor tendon, and even these were very much weakened. General wasting also of these muscles had taken place. His gait was very awkard and insecure; and he came to ask me about the desirability of instruments to assist him in walking, having found galvanism almost constantly employed utterly useless.

I explained the above-described use of strychnine, and proposed that it should at all events have a trial. He consented, and I began with the right leg, which was rather less wasted than the other. The dates and quantities of injection were as follows:—Jan. 14th, seven half-minims; 17th, eight; 19th, ten; 21st, twelve half-minims. From the time of the first injection the limb began to improve, both in power and in size. I then took the left leg—the more wasted of the two; but as the last injection had caused a slight sense of uneasiness I used a less quantity—namely, on Jan. 24th, eight half-minims; 26th, eight; 29th, eight; 31st, eight; Feb. 2nd, ten; 5th, ten half-minims. Here also improvement commenced with the first injection, as evidenced by the outlines here given. The broken line shows the condition on Jan. 24th; the dotted line, the condition on Jan. 31st; and the plain line, that on Feb. 10th. Each outline was made by cutting out a piece of stiff card-board to fit on the limb 10½ above the inner malleolus so accurately as to leave nowhere any interval between its edge and the surface, yet nowhere to mark or impress the skin. The forms thus obtained were then accurately reduced to scale.



In equal degree with the restoration to shape did voluntary power return. The limbs are not yet as strong as normal members, but this is a mere matter of time.

M. E. B——, aged forty, came under my care on the 17th of October, 1872, with paralysis of the right side of the face. On the 31st of August she, in washing her face, put her finger twice into the right eye. To this she attributes the paralysis; it was doubtless a consequence. In the evening her husband saw the "mouth go round." She has been since the 1st of September under treatment, taking tonics and strychnia, and using galvanism.

Oct. 17th.—The right side of the face is perfectly immobile; the mouth much drawn to the left; the right eye wide open. I tested the irritability of the muscles with a rather sharp galvanic current. There was some action in the platysma, but nowhere else. I injected seven half-minims of the 2 per cent. solution of strychnia; no constitutional effects.

27th.—This patient remained away ten days, as she was afraid of what she called "the operation." To-day, however, I found improvement, which she said had immediately followed the injection, but had not continued after the first three days; hence she returned, and I injected eight half-minims. On Oct. 29th I injected ten half-minims, on Nov. 1st ten, on Nov. 3rd ten, on Nov. 5th twelve, and on Nov. 7th twelve.

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* Clinical Society's Reports, vol. ii.

I have noted here nothing beyond the mere injections, as steady improvement is the history of the case. After ten injections she was quite well, the mouth remained straight, and she could close the eyes perfectly. I saw her only a week or two ago, and she remained quite well.

It is to be observed that in this and in other cases of adult eccentric paralysis voluntary motion has been perfectly restored, while galvanic irritability has remained absent for a considerable time. In other cases a response to the continuous current has early arisen, but only after voluntary movement became markedly improved. The behavior of infantile paralysis is herein different, and response to the electric current from one or the other apparatus precedes any return to voluntary movement; but this also comes after a little while. My method of proceeding with infants is as follows: To begin by galvanising each muscle separately, and to note the amount (if any) of electric irritability, or its entire absence. The former organs may almost take care of themselves, a little galvanism being passed through them occasionally. The others should be galvanised two or three times, and closely watched to detect any sign of twitching. Now during all this time it is most essential not to frighten the child, or give more pain than necessary. It is impossible to make much out about the paralysed muscles of a limb while the part is being jerked from above by violent movements of the trunk. Although we do not expect electric mobility to come into the muscles which have long been paralysed, it is well thoroughly to test them. And now, having determined to use strychnia, I begin with from three to six half-minims, according to my patient's age; and, selecting an opponent to that muscle which is most contracted, I inject the solution into its substance, or sometimes over the place of entry of its nerve. I continue such injections two or three times a week for a certain period, and then try the muscles again. They nearly always respond first to the constant current, and usually soon after this a small amount of voluntary power becomes perceptible. The injections must not be discontinued as soon as the galvanic or nerve irritability is established; brisk response to the former should at least be secured.

L. C——, aged six years and six months, was paralysed in the left leg sixteen months ago, there having been neither previously nor afterwards any severe illness. For the first ten months nothing was done except tonics and occasional rubbing. During the last six months galvanism and the internal use of strychnia have both been extensively used; indeed, on one occasion the drug was pushed so far as to produce tetanic spasms.

She came to me on the 6th of June, 1870. The left leg was shrivelled and utterly useless. When she was supported by her nurse on the sound leg, the other hung impotently down. By a peculiar jerk of the body, common to all these children, she could make it swing; and the psoas being intact, or nearly so, enabled her to hitch it a little forward. The limb was always cold—blue in places, pink in others. The muscles were so wasted that the limb looked barely thicker than the bone should be. There was no response to even strong galvanic currents. Three minims of strychnine solution were injected.

I injected this child with strychnia twice a week for three months before I got any response to galvanism. When the response began I left off the injections and the irritability decreased markedly. I then, in October, recommenced the use of strychnia, combining it with galvanism, and this treatment was continued for five months. In March, 1871, I discontinued the injection. Considerable voluntary irritability of muscles had now been established, and very easy response to the continued current. In May, the induced current began to tell. The rest of the case is a mere question of time. The patient is still improving rapidly.

J. R——, aged seven years and three months, was brought to me on February 3rd, 1871, with a deformity of the right foot, which I have named *pes cavus*, arising, as I have shown, from paralysis of the gastrocnemius, and frequently also of the muscles in front of the tibia. These paralyses were present in this case. The leg was a good deal wasted, but not livid. It easily lost temperature on cold days, but if kept well covered remained warm. The paralysis came on when she was two years old without illness of any sort; the deformity formed itself gradually. She has been treated, among other methods, with galvanism and internal doses of strychnine.

I injected five half-minims to begin with, and twice a week I continued this method, increasing to eight half-minims. At the end of seven weeks the muscles became amenable to the continued current. The use of galvanism was combined with the injection. At that time—viz., three lunar months—the muscles were voluntarily mobile, and the size of the leg was very much increased. The child walked very fairly well. The deformity was treated in a manner to be described in the sequel.

These two cases mark strongly a difference to be found in paralysed limbs—a difference which is of the very greatest value for prognostic purposes. In the one the extreme wasting and the great loss of temperature marked almost the utmost limit of nutritive disorder; in the other, although paralysis had lasted as long, or even longer, and though the limb was greatly wasted, yet the skin was not so cold nor so livid; the muscles gave, when pinched, some idea of resiliency, and were not like either soft paste in a sausage skin, or like bundles of cord, as in the former case. Now in these less livid cases, as it is certain that muscular degeneration has not gone so far, the chances of recovery within a moderate time are very considerable, however long the malady may have lasted. Of the other class I would speak with all caution. I have met with other cases besides that of L. C—— just quoted, which leads me to believe them curable, if sufficient care be employed for a long period. But we have to do not merely with malady. Three other conditions may, and do, often interfere—viz., convenience, patience, and money—to one or the other, sometimes to all of these, the best devised therapeutics will be forced to yield.

ABSTRACT OF

Two Lectures

ON

THE DIAGNOSIS OF PELVIC SWELLINGS.

Delivered at the Hospital for Women.

By Heywood Smith, M.A., M.D. Oxon.,

Physician to the Hospital for Women, and to the British Lying-in Hospital.

After a few introductory remarks on diagnosis, arising out of Hippocrates' aphorism, "the coming-to-a-conclusion is difficult," the lecturer sketched the method he considered the best for eliciting the correct history of a case, and then, going at once to the subject, said: "I make no apology for using the word 'swelling' in preference to the ordinary Latin word 'tumor.' My reason for doing so is this:—In some way or other the word *tumour*, while including all the signification of swelling, has added to it in the public mind the idea, not merely of the swelling of a part, but of a growth more or less separate or separable, and generally of a dangerous character. This idea of an entity independent of surrounding structures is often fixed so indelibly in the patient's mind that the hope of a remedy prompts the frequent question, 'Can I have it dispersed, or must it be cut out?' It seems so important to do away entirely with this erroneous impression from the minds, not only of our patients, but also of ourselves, that I have used this word 'swelling' as conveying a meaning more akin to the truth. With regard to the limitation 'pelvic,' by it I would confine our present investigation to those swellings that lie wholly, or have their greater part situate, within the boundaries of the pelvis."

The exercise of the various senses as means of diagnosis was then fully explained, and the various methods usefully tabulated; but there is only room here for their partial enumeration. The sense of *touch* was divided into *immediate* and *intermediate*. Under the head of immediate touch were placed: (1) The simple digital touch, (*a*) of the vagina, (*b*) of the rectum; (2) the "double touch"—bi-digital touch, or recto-vaginal examination; (3) the conjoined examination; and (4) abdominal palpation. Intermediate touch was explained to be that by the sound, whether uterine or vesical, and also by the thermometer. The sense of sight is exercised usually through specula of various devices directly on the object, or on fluids withdrawn by fine trocars, or by the microscope. Hearing is called into play in immediate or intermediate auscultation, and in discriminating between the often slight variety of note or pitch elicited from immediate or intermediate percussion. In speaking of immediate touch, Dr. Heywood Smith said: "It may here be remarked that, as a rule, the sensation of touch, as perceived by an internal examination, is such as to convey to our minds the presence of a body larger than really exists. The following explanation is submitted as not altogether improbable:—The apprehension by the mind of degrees of comparison stands in relation to brain-work as given out in nervous energy. Now our ordinary measurements by the sight need but little movement of the eyes; also, on calling in touch to our aid, an object is usually grasped by several fingers at once, necessitating but little motion; but, in internal examination, it is necessary that the point of the finger should journey round a tumour in order to comprehend it, and the mind, taking cognisance of the size by the amount of motion required, builds up an idea of comparative greatness so much larger in proportion to the nervous energy expended in acquiring the information."

The lecturer then, before dividing the subject-matter under its various heads, made some remarks on the morals of vaginal examinations, in the course of which he said: "If a patient applied to a physician for diseases of the chest, he would be deemed to have gone into the case but superficially unless he had made a most thorough examination by means of every appliance that could be brought to bear on the subject. In the same way, and similarly as a matter of course, the pelvis should be examined where there are indications of its necessity. The indelicacy lies not in the examination itself, but in making much, or indeed anything, of it; the necessity arises out of the very nature of the case, and it should never be introduced with a preface, as if it were out of the usual order of things, but undertaken in the ordinary investigation of the case with the same unaffected ease with which a sight of the tongue would be requested."

The system of classification chosen as that considered the best under which to arrange the large amount of material was the anatomical; and the first main division was into (1) normal and (2) abnormal pelvic swellings.

1. The normal pelvic swellings may be thus enumerated—(*a*) the bladder full; (*b*) the uterus unimpregnated; (*c*) the uterus impregnated; and (*d*) the rectum loaded with fæces.

2. Abnormal pelvic swellings were considered in relation to the various organs and tissues involved, and embraced swellings connected with the bladder, the uterus (fundus and cervix), the ovaries, the oviducts, the broad ligaments, the vagina, the rectum, and, finally, any swelling within the pelvis that had not been included in the above list.

Among swellings connected with the bladder were mentioned and illustrated calculus, abscess, cancer, cystocele, foreign bodies, &c. In diseases of the uterus the cervix uteri is the portion which most often shows itself as a pelvic swelling, especially in its state of cancer of various forms, chronic inflammation with deposit, engorgement with elongation of the cervix uteri, polypi of various kinds, and the occasional malformation of a double uterus. Cancer of the cervix uteri in its early stage has, in the form of scirrhus, to be differentiated from indurated (benignant) deposit in the labia uteri, and in the form of epithelioma to be separated from rodent ulcer, syphilis, and granular inflammation seated upon a hard basis in the cervix.

For the diagnosis of these several maladies, it is not merely requisite that the different morbid appearances should be most carefully observed, but also that the histories of the cases should be most accurately gone into and traced, so as, if possible, to make the diagnosis tolerably certain by the very history, and almost independently of any examination. The cases of induration of the cervix uteri present a hardness often circumscribed, occasionally involving the whole of one lip. In these cases the submucous hardness seems to lie at a somewhat greater depth below the membrane than does the hard tissue of carcinoma.

Sometimes diseases affecting only the body or fundus uteri present themselves per vaginam as pelvic swellings. Cancer of the fundus unassociated with, or, in fact, secondary to, carcinoma cervicis is rare; yet when it does occur it may be mistaken in diagnosis for fibrous tumour. The cervix high up or the body is felt to be bulging and hard, not very tender; the os uteri not patent; no discharge; in fact, the case may lack the characteristics of cancer. The history, too, may not be much help, for women will often live for several years before they find out that they are the subjects of fibrous tumours; and in cancer, in some cases, the disease may advance some way with scarcely any pain and without discharge. In such a case the passage of the sound will generally reveal the nature of the swelling; for when it touches a cancerous mass some bleeding is usually set up, and doubtless, at the same time, some pain too. Should, however, the diagnosis be not even then cleared up, the lips of the womb should be divided freely bilaterally, when cancer would reveal itself by being forced through the os uteri. A fibrous tumour might also thus, under certain circumstances, be forced through, or rendered amenable for exploration; and in that case division might enable further remedial measures to be taken. In the case, however, of fibrous tumours the history should be taken into consideration, as these may exist without much evidence of their presence for many a year. While speaking of uterine fundal swellings, it may not be out of place here to mention chronic inversion of the uterus: a condition liable, without due care, to be mistaken for a polypus; but the passage of the sound, disclosing an invagination of cervical tissue, determines the solution of the difficulty.

The lecturer then came to consider the various versions and flexions of the uterus, inasmuch as they produced pelvic swellings which it was absolutely necessary should be differentiated from each other and from other morbid conditions for the purposes of diagnosis. In considering *anteversion* it must be borne in mind that to constitute an abnormality the uterus must lie nearly horizontally; the natural position of the organ being at right angles to the plane of the pelvic brim, and so anteverted as regards the perpendicular. In *retroversion* attention is called to some difficulty in defecation with backache among other things, and the simple touch generally reveals the nature of the dislocation. In *retroflexion*, however, while these symptoms are generally exaggerated, there is also a post-cervical swelling separated from the cervix by a sulcus which is not found in retroversion. That this swelling is the flexed fundus is only to be demonstrated by the introduction of the uterine sound, for the proper use of which minute directions were given. *Anteflexion*, on the other hand, was described as producing more severe subjective symptoms, and for its proof requiring the careful use of the sound. These flexions were also referred to as occurring in the impregnated uterus. Other organs, some of whose morbid conditions should be ranged under the head of pelvic swellings, were then considered—namely, the ovaries as felt in chronic and acute ovaritis; also all the ovarian cyst formations, whether unilocular, multilocular, or histoid of various kinds. After referring to diseases of the oviducts, the pelvic swellings that are connected with the broad ligament claimed attention; these arise from cyst, pelvic cellulitis, abscess, periuterine serous effusion, and parauterine, or extra-peritoneal areolar hæmatocele. Cysts are scarcely distinguishable from those of the ovary save by their contents. Cellulitis is differentiated by its history from hæmatocele, and both from fibrous tumours by the absence of constitutional symptoms with the latter and by the use of the simple touch of the uterine sound. Pelvic swellings confined to the vagina may be caused by abscess in its wall, cancer, cicatrices, fibrous tumour, syphilis, condylomata, cysts, polypi, and foreign bodies. Cancer of the rectum may produce a pelvic swelling felt per vaginam, as also rectocele.

Dr. Heywood Smith then drew attention to certain swellings that might be present in the pelvis, and which had not been included in the above classification, as increased projection of the promontory of the sacrum, exostosis, cancer of the bone of the pelvis, abscess of the kidney, extra-uterine fœtation, enterocele, hydatids, aneurism, and post-uterine intra-peritoneal hæmatocele. The bony diseases are recognised by their hardness, which, in osteo-sarcoma, is varied by portions of friability. Pyonephrosis may extend into the pelvis and produce a swelling. The diagnosis of extra-uterine fœtation was carefully gone into, and its differentiation from fibrous tumour and ovarian cyst indicated. The rare accident of enterocele was explained, and hydatids and aneurism alluded to to render the list complete; and the last pelvic swelling considered was intra-peritoneal hæmatocele. This was explained to be more a symptom, though a very grave one, of some mischief, happening generally suddenly, in connexion with some previously diseased pelvic organ. A table was shown exhibiting, in parallel columns, the differentiating signs and symptoms between it and pelvic cellulitis, extra-uterine pregnancy, ovarian cyst, fibrous tumours, retroflexion of the uterus, and extra-peritoneal hæmatocele respectively.

The lecturer concluded with these words:—"In order to diagnose pelvic swellings with accuracy, it is necessary that we should habituate ourselves to an orderly, deliberate, and logical investigation of the question before us; and failing, as we so often do, to be able to grasp special pathognomonic signs, let us give our careful attention to the following: (1) to any deviation from the normal state of any function, not only of the organs, affected with disease, but also of neighboring organs, or organs functionally connected; (2) to all the absolute physical changes in any part under observation which may be discovered by any means in our power. To these ends we should bring to bear all the knowledge and appliances that modern medicine has furnished us with. And when we have done all this conscientiously, and have come to a conclusion which we have every reason to believe is a right one, how humiliating to have to confess that that labor has only given us the knowledge of the existence and character of the disease, and that on arriving at a correct diagnosis we have scarcely even entered the solemn portals of our art."

SUBSTANCE OF
Clinical Remarks
ON A RECENT
CASE OF LITHOTOMY
At University College Hospital,
IN WHICH THE OPERATION WAS PERFORMED FOR THE PURPOSE OF REMOVING A PIECE OF BONE FROM THE BLADDER.

By Sir Henry Thompson, F.R.C.S.,

Surgeon and Professor of Clinical Surgery to the Hospital.

The case before us is so unusual that I shall briefly recapitulate the history, and illustrate it by referring to one other very similar case on which I operated in this ward some years since.

You will remember that I performed the lateral operation on this lad three or four weeks ago, and you see that he is now making a good but rather slow recovery. He was sent to me by Mr. H. Evans, of Hempstead, and he was admitted here on the 14th of February last.

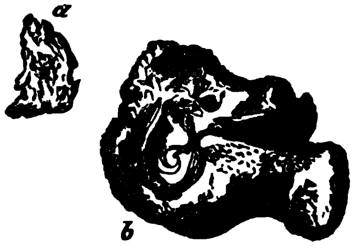
W. D——, aged fifteen, a weak-looking lad, much reduced by long and severe sufferings, from symptoms of vesical calculus of at least three years' standing. I sounded him on the 23rd, having been prevented from making my usual visit sooner; and, at once striking a large stone, observed that I would next introduce a lithotrite to measure it and ascertain whether it might possibly be within the compass of lithotrity, as he had a good urethra for his age. I did so, and grasping it, remarked to the students around that it was unlike "in feel" to stone, that in turning it over I felt a portion projecting, and would remove a little bit in the blades of the lithotrite to see what it was composed of. I did so, and withdrew the instrument with a small piece of bone between the blades. It was handed round, and we all saw that there was no question as to its being bone. I then learned the following history, with which I had previously been unacquainted. It was carefully taken by Mr. Lang, the ward clerk, whose words I now quote:—

"Four years ago the lad was run over by a cart, a wheel of which passed over his pelvis on a level with the superior spine of the ilium. Considerable bleeding from the urethra followed, and for more than a month after the injury he was unable to pass water by his own efforts, and a catheter was used to draw it off. Soon afterwards he felt pain during and after voiding urine, which was occasionally mixed with blood. During three years past he has also occasionally passed 'grit'. Once, about three years ago, he says he passed a piece of bone."

The usual symptoms of stone were present, but not in a very marked degree. I then determined, on account of the size of the foreign body, the irregularity of its form, and the certainty that it was at all events in part composed of bone, to perform lithotomy, and eventually decided on the lateral operation, after considering the claims of the high operation, which could not be overlooked in a case of this kind.

On the 6th of March I performed the operation in the usual manner making the incisions rather free, and removed several loose phosphatic masses, and then a larger one, measuring about an inch and a half in length, which proved to be bone highly encrusted with phosphatic deposit. (See Fig. 1.)

Fig. 1.



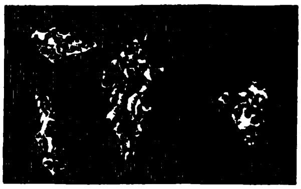
a, The piece of bone withdrawn by the lithotrite at the examination. *b*, The bone surrounded by phosphatic matter removed by lithotomy. A number of large fragments of phosphatic calculus were also removed, but are not represented. The above are shown of the actual size.

I need make no detailed remarks on his progress, which has been an uninterrupted convalescence, except one occasion by a large deposit of phosphates on the surface of the wound, which was removed and has not yet reappeared. He now passes water mainly by the urethra, and a small quantity only continues to flow through the wound, which has not at present entirely healed.

Now it must appear to you, as it really is, a very remarkable circumstance that a portion of bone detached by fracture from the pelvis should find its way into the bladder, become the nucleus of a stone, and four years after be removed by lithotomy. But, as I said before, the process which Nature instituted here may be illustrated by another case, equally remarkable which I will briefly relate.

During June, 1865, I found, at one of my hospital visits in this very ward, a man aged forty, sent to me by the late Dr. R. Uvedale West, of Alford, Lincolnshire. I sounded him, and then, as usual, introduced the lithotrite to ascertain the precise size of the calculus; and, having done so, remarked to the by standers, as in this case, that the mass did not feel like stone, and that I would withdraw a small portion as a specimen. I did so, and at once produced a small piece of bone for their examination. But in this case the mass was comparatively small, and I resolved to perform lithotrity. I did so on June 27th and 30th, crushing the bony matter and phosphate matter together; and on July 3rd I removed a portion of bone with the forceps which had become impacted in the urethra. On the 15th of July he was discharged perfectly cured. The bony portions removed shown in Fig. 2, and are drawn from the original now in my posses-

Fig. 2.



sion. One of the smaller pieces was removed by the lithotrite; the largest is that which became impacted as described. As before, the loose phosphatic matter is not represented.

Now what was the history in this case? Of course, as soon as I had withdrawn the bit of bone at the first examination I cross-examined him as to his history, and learned that during seventeen years he had had several attacks of pain in the right hip-joint, followed by abscess on three occasions. The last occurred two years before, soon after which he had symptoms of stone in the bladder. On stripping him, there were the cicatrices, and there also was the resulting deformity on the right side. At that time I presented this case, believing it to be unique, to the Medico-Chirurgical Society, with the bone and phosphatic fragments removed. I stated that the origin of the stone had, without doubt, been a portion of necrosed bone from the os innominatum, which, instead of being extruded externally, in the usual manner, had found its way, by slow ulcerative absorption of the tissues which lay between it and the bladder, into the cavity of that organ. The bone here soon acquired a phosphatic coating, and produced the symptoms which led to its detection and removal.

In the case before us no doubt the same process took place. I do not imagine for an instant that a portion of bone was forced by mechanical violence into the bladder at the time of the accident. The organ was injured at the moment of fracture—as we know by the hæmaturia, and by the loss of ability to expel urine by the natural efforts,—and almost certainly by a portion of bone which became detached, but which probably was only partially driven through the coats of the bladder at first. Subsequently, no doubt, it became free in the cavity and acquired a coating of phosphates, and produced the symptoms of a foreign body there.

I shall briefly refer to the only cases I know on record in which bone is the nucleus of a calculus. The first is in the museum of the Royal College of Surgeons, London — a large phosphatic calculus removed from the female bladder; in the centre of this formation is a piece of bone, but no history whatever of the specimen exists. The second is to be found in the American "Report of Surgical Cases in the Army," Circular No. 8, page 259, in which lithotomy was performed for a large phosphatic calculus in 1865; in its centre was found a small piece of bone; and, as the man was wounded in 1862 at the battle of M'Dowell by a ball which passed through his pelvis, shattering the horizontal ramus of the left pubic bone, there can be no doubt as to the nucleus having resulted from the gunshot injury.

I have only one more remark to make relative to the two interesting and instructive cases which form the subject of our remarks to-day. It is to recommend you always—in accordance with the rule I have so constantly given and insisted on when lecturing on stone in the bladder—to make a careful and accurate diagnosis of the case, before entering upon any operative procedure, in every case of stone. For this purpose a rather small and light flat-bladed lithotrite is one of the most useful instruments. Were there no such process as lithotrity, the lithotrite would still be an invaluable instrument in this respect. By its means you see how easy a thing it is to determine the size, form, and composition of a foreign body before deciding the question, often so momentous, as to how it must be removed. For this purpose the old, clumsy, heavy instrument is wholly inadequate. Extreme lightness and facility for being moved are the essential requisites for such a tool; and the more I see of calculous disease, the more I am impressed with the value of such a one for the purpose of acquiring an exact notion of the body on which I am called upon to operate before I decide on the method to be adopted.

ABSTRACT OF
Lecture
ON
CLINICAL STUDY.

Delivered at the Liverpool Royal Infirmary.

By A. T. H. Waters, M.D., F.R.C.P.,

Physician to the Infirmary.

Gentlemen,—I wish to call your attention today, not specially to any of the cases in the hospital, but to the subject of clinical study. It will be my duty, during the next few months, to give you a course of clinical lectures independently of clinical instruction in the wards. I am anxious therefore that this, the first occasion of our meeting, should be devoted to an endeavor on my part to enlist your interest in this portion of your work, and to point out to you its paramount importance. All the other studies in which you are or have been engaged are but introductory to that of disease at the bedside, and must be made subservient to it. It is in the wards and in the clinical lecture room that you will see the living examples of the various affections which are treated of in your theoretical discourses on medicine and surgery, that you will learn the practical application of the principles of diagnosis, and that you will be able to observe the manner in which disease or accident becomes amenable to treatment or baffles the skill of the physician or surgeon. And let me tell you that if you wish to become successful practitioners of medicine you must begin early to observe the phenomena of disease, to familiarise yourselves with its various aspects, and to learn the manner in which the examination of a patient should be conducted.

I do not think that the student of the present day is overtasked as regards attendance on theoretical lectures, nor are the requirements in this direction such as to prevent his daily visit to the wards. There has been, in fact, a tendency of late to curtail very largely the more formal lectures, and to give a practical character to our teaching. Indeed, the examinations of our licensing boards are now of a nature to test, much more than was formerly the case, the practical knowledge which a man possesses; and if the system of making candidates for a diploma examine cases of disease, and perform some of the manipulations of surgery, become more general, it cannot fail to have a beneficial influence on the education in our schools of medicine. A step in the right direction has recently been taken by the College of Surgeons, for which it deserves all credit, in reference to the study of practical surgery and practical physiology.

Your clinical studies, gentlemen, should be carried on in a carefully regulated manner, and let me now give you a few words of advice in reference to this subject. In the wards of this hospital you will have ample opportunities of seeing almost every variety of disease, but I would warn you against the notion that you can learn to practise your profession by hurriedly looking at a large number of cases. It is not by a rapid survey of numerous patients that you will learn to estimate aright the nature of their symptoms, or the treatment which is applicable to them, but by a careful study of a few; and the plan which you ought, in my opinion, to adopt is, to take in the first place those cases which are well marked instances of their kind; to note accurately and regularly from day to day all the details of each case, and record them at the time in your note-book. Do not attempt too much at once. Take a few cases of disease of some organ, or of some general malady, as acute rheumatism, and make them for the time the subjects of your special study. Let nothing interfere with your daily visit to these cases, and especially make a point of being present when the physician under whose care the patients are placed goes his rounds. You will gain from him his opinion of the nature of the cases, and the progress which they are making. You can compare the opinion which you have formed with his, and have your views corrected or confirmed by his more matured judgment. Moreover, if you are in doubt as to any particular symptoms he will be quite ready to assist you in reference to them. It is not to be expected that, during the early period of your attendance on hospital practice, you should give an undivided attention to your clinical work, or be able to appreciate, as you will after you have been well grounded in your anatomical, physiological, and other studies, the features of disease or the nature of surgical lesions. But let not these circumstances prevent you from regularly attending, from the very commencement of your hospital career, the clinical lectures, and the clinical instruction in the wards. And with reference to the latter part of your time here, when you have passed your preliminary examination, a very large share of your attention should be given to clinical work. It is then that you should take the office of clinical clerk, and devote yourselves assiduously to the duties which it involves. The appointment of clinical clerk is one of great value, but it depends upon yourselves how far you profit by it. Some, if not all, of the examining boards require that a student shall have filled this office before he can be admitted to examination for a diploma. This is a wise regulation. But before you undertake the duties of a clinical clerk you should have prepared yourselves for the work by taking your own notes of cases; you will thus not only render yourselves much more useful to your physician, but you will derive far more benefit from your clerkship than you otherwise could.

One word with reference to the attendance on post-mortem examinations. You should never fail to be present at these examinations. If you have watched the progress of a patient during life you will gain information of the most valuable kind from a study of the appearances which the autopsy reveals. But of what avail will these be to you if you see the case for the first time in the dead-house, or if you do not carry there with you a well-grounded knowledge of healthy structure.

And with regard to case-taking. This is the most important part of your clinical work. It is that which, after the primary difficulties are overcome, will be the most interesting to you. It is impossible for you to estimate the nature or progress of cases without carefully placing them on record in language of your own. The cases of which you preserve notes will dwell in your memory in after-years, and will serve as references in the treatment of cases of a similar character. Many of you, when you have once passed from your school and obtained your qualification to practise, will have no opportunities of clinical study except such as your private practice may afford, and you will then find the value of the accurate knowledge of clinical facts which you have acquired by systematic case-taking. But case-taking is not only important in impressing on the mind the features of disease; it is also a most valuable training. It leads to clearness of thought, exactitude of language, and habits of regularity—all of the utmost value in reference to your future career as practitioners. By it you learn how to question a patient, what are the special points to insist on in making a diagnosis, and the most ready way of arriving at a correct opinion of the nature of a case. Three months' work as clinical clerk, three months of careful daily case-taking, are far more useful than as many years of desultory, irregular, uncertain attendance in the wards.

Let me add a few words in reference to the subject of diagnosis. It may seem to you an easy matter to make out the nature of a case. A man of experience will put a few leading questions, make perhaps only a slight physical examination, and at once pronounce a correct opinion of the existing ailment; and you may imagine that you will readily be able to do the same. But do not be mistaken. This power of rapid diagnosis has been the result of long observation and great painstaking; and you will find even that the man of the most matured judgment will often spend a long time over a case—will examine carefully all its details—before he will venture on an opinion of its nature. If you wish to attain to the power of accurate diagnosis—of rapid diagnosis in ordinary cases,—you must begin by examining with the greatest care every detail of a series of cases which are the best-marked instances of their kind. The study of these will prepare you to understand the varieties and complications which so constantly present themselves at the bedside.

In conclusion, I would therefore impress upon you the importance—first, of a regular attendance in the wards, at the clinical lectures, and at post-mortem examinations; and, secondly, of careful case-taking. Bear in mind that when you have to undertake the management of cases on your own responsibility, you will have to watch and treat the symptoms which arise from day to day. Your attendance will not then be of an uncertain and irregular character; for if it be, your patients will soon dismiss you. As, then, you would wish to be able to estimate the daily progress of your future patients, watch carefully the daily progress of those whose treatment you witness here. Record their cases in your note-books, that they may become the more firmly impressed on your memory.

Original Papers.

EXPERIMENTAL OBSERVATIONS ON THE APPEARANCE OF THE LOWER FORMS OF LIFE.

By Robert Garner, F.L.S., F.R.C.S.,

Surgeon Extraordinary to the North Staffordshire Infirmary.

Without regarding with much complacency the following experiments and observations, or supposing that they can clear up a very dubious question —in fact, without feeling that the experiments have resolved all the doubts in the mind of the writer himself, he still thinks that they may be considered, and that irrespectively of any particular theory, as worthy of record. The first experiments (1), no doubt similar to others which have been often tried, were instituted for the purpose of ascertaining whether suitable animal or vegetable fluids, after being deprived, as was till lately supposed, of all living organisms and also of organic germs by boiling, and afterwards being simply cut off from the access of air, will present, after standing a proper time, any trace of the organisms which would in general have plentifully appeared if they had been unheated and freely exposed to the air. Other experiments (2) follow, in which a portion of air may or must have been present, but, if so, it must have been pretty strongly heated. In the next experiments (3) the liquid was also, it is believed, in a part of them, entirely shut out from the atmosphere, and in another part a portion of oxygen substituted for air, whilst in both the liquid used was previously submitted to prolonged boiling. In the fourth set of experiments (4) air was admitted, but only through a tube partly filled with asbestos saturated with sulphuric acid. Lastly, a few experiments (5) follow of a more miscellaneous character, and to be particularised further on in this paper. In some of the later experiments it is hoped that the materials chosen for the infusions, including the water used, were such as to render it unlikely that any germs or forms of life existed therein.

In the first experiments the fluid was boiled in a large test-tube for about fifteen minutes, then, the tube being a little inclined and the violence of the ebullition allowed to subside somewhat, olive oil strongly heated was cautiously allowed to run down by the side of the tube, till it formed a stratum of one or two inches on the top of the animal or vegetable infusion used. By this plan explosion was avoided, and it was supposed that the access of the atmosphere was prevented. In eight such experiments organisms appeared; in five, none. Yet it should be premised that these experiments, with another set to be mentioned further on, constituted the strongest presumption the writer met with for the doctrine that the life observed in such experiments is derived from the atmosphere, for the germs of it might, as we see with flies and other light bodies— as vegetable powders, for instance,—have sunk through the oil and so given origin to the life. When the oil was thickened with wax or resin life appeared less commonly, though it did appear in several of the experiments, as will be again alluded to. It may be concluded, however, that life is developed scantily in such closed vessels with entire absence of air, and, in fact, we found that an infusion of hay already abounding in vibrios lost the greater part of its life when so treated. The careful housekeeper, in making her preserves and syrups, is aware of the use of such a sealing by lard or suet, as well as of strong boiling and concentration. The animal life found in these tubes consisted of bacteria, often in the form of dumb-bells or of the figure of 8, and about the 4000th of an inch in length; vibrios of different kinds and sizes, linear, jointed, or spiral, and from the 2000th to the 50th of an inch in length in an infusion of bruised oats, and so long as to stretch across the field of the microscope and also very active in a decoction made from a kidney; and monads and active molecules, of which little can be said and little account has been made. The fluids operated upon were infusions or decoctions of hay, bread, oats, flesh, &c. The vegetable life consisted of interwoven, colorless, jointed filaments, or of pellicles which at first looked like chemical or mechanical formations, but which, after washing in water and ether, were evidently vegetable growths. The filaments in this case were hyaline, tubular, branched, jointed, and sometimes club-shaped at the end, and they were also accompanied by small and a few larger oval corpuscles, sometimes motile. Upon the whole, the lower vegetable forms seemed to appear more freely in liquids so treated than the animal.

The above experiments were mostly made in September, 1870, the liquids being allowed to stand seldom more than two or three weeks before they were examined with a power of from 500 to 1000. Lately we have found that infusion of hay or of flesh, the latter fresh—that is, living and contractile,—made with distilled water from the boiler of an engine working at considerable pressure, or with common, fresh-boiled, distilled water, the glass tube being heated, the operations accomplished in the heat of a strong oven, and, finally, the infusions covered with oil of turpentine, which seems to follow carbolic or sulphurous acid as a destroyer of the forms of life in question,—we have found such infusions to present undoubtedly bacteria and vibriones. The experiments with wax have also been repeated. Thus in a long tube or preparation glass, previously cleaned with sulphuric acid and liquor potassæ, and made hot, was dropped a little juice of meat, some heated sugar, and albumen from a newly laid egg broken for the purpose. The glass was filled with distilled water microscopically pure, and which did not contain animal forms higher than molecules even after being exposed for some days on the warm mantel-piece; finally, hot oil with wax was poured over the liquid, taking care that no opening was formed by the contraction of the wax. It was not long before turbidity and flocculent matter, with the usual animal forms, appeared; and so with a large test-tube, the circumstances being similar, but the relative amount of life less. In a tube containing a solution of acetate of potash and carbonate of ammonia, treated the same way, except that the solution was boiled, the result was the appearance of a few vibrios and of a beaded alga or fungus.

Other experiments (2), allowing we suppose of the access of a little strongly heated air, were made in 1870 and 1871, by merely boiling the fluids in suitable test-tubes, which had been previously drawn out and bent near the middle. In each case the fluid was boiled from ten to fifteen minutes, and the ebullition made to reach to the mouth of the tube; then the spirit-lamp was gradually raised to the narrow part, and its flame kept there till the glass became so softened that the lower cell containing the fluid could be sealed up. On breaking off the ends of these tubes there was sometimes a little report from the rush of air. In two-thirds of the experiments it was found that the fluids, after standing two or three weeks, presented either animal organisms (so called), monads, bacteria, and vibrios, or vegetable torulæ and filaments; but certainly more sparingly than when portions of the same fluid had, *cæteris paribus*, been exposed to the air, or when it had been attempted to exclude foreign bodies by merely plugging the tubes with cotton. Occasionally the liquids used could not be discovered to contain organisms after standing, even when they had not been submitted to heat nor excluded from the air; other circumstances, we suppose, not being favorable—the strength of solution, degree of temperature, amount of light, nature or quantity of fluid, length of time of standing, &c. In a tin of preserved haddock, and in another of Labrador salmon, we could discover no vibrios or bacteria; but in three tins of beef-tea, perfectly good in taste, both were present in the motile state.

In the following experiments (3) there was very severe boiling, and, we think, the exclusion of the atmosphere. In October, 1871, an animal decoction was made, and boiled for four hours in a pneumatic trough; the bottles to be filled, together with their stoppers, being also placed in the trough. Six of the phials were filled with the hot liquid, and various proportions of it displaced by pure oxygen which had been passed over quick-lime; the phials were then stoppered under the liquid and cemented. Four phials were quite filled and then stoppered; a very small vacuum in each soon appearing, but apparently not from the admission of air. Two phials had some air purposely admitted, being afterwards stoppered and cemented; in one the air was allowed to have free access. Two large test-tubes were also partially filled, and then very strongly plugged with cotton-wool, the liquid again boiled for ten and fifteen minutes, the ebullition rising nearly to the plug, and strongly heating it. Upon examination of these two tubes, after standing a week, it was found that in one of them, the liquid having been boiled down to a small quantity, there were only moving molecules or monads, of which little can be said—the liquid had apparently become too concentrated; in the other tube, which gave off a more fetid smell, were bacteria in abundance and various vibrios, some of them peculiar, being very long and sinuous. The phials into which the air had been admitted, more or less freely, also abounded in life. The full bottles contained no vibrios or bacteria, but only moving molecules, constituting the second circumstance above alluded to, from which some would conclude that the germs of the life found in the other experiments were derived from the atmosphere solely. The writer, however, is disposed to believe that their very frequent, though not constant, absence in such a case depends upon the organic matter present in the liquids being prevented from undergoing that chemical change, attended with the absorption of oxygen and the evolution of carbonic acid, which is necessary for putrefaction or fermentation, and for the formation and existence of life, especially of animal life. These full bottles were under similar relations to the tubes containing liquids covered with oil or wax, and therefore the experiments might have been arranged with the first set (1), in which, however, these tubes, which had their contained liquids covered with oil simply, were by no means devoid of life, either from the cause above alluded to, or from the boiled oil acting as a gas-transmitter. We found upon a second trial of the same experiments (3) that of two phials, which were filled, stoppered, and cemented, one contained vibrios, though they were dead or still, and the other a pellicle at the top, consisting of molecules having movement and all globular under a high power. In five of the phials with oxygen there were undoubted vibrios and bacteria generally abundant; in two, active particles or molecules only. The phials generally gave off a fetid smell; in one or two the oxygen still retained its properties, in the others a match was extinguished. They were generally not decidedly acid or alkaline. The contents were examined at from seven to twenty days. It should also be noted that in these experiments the gas was received immediately from a small retort without using the quick-lime; it was therefore less chemically pure, but more certainly free from atmospheric air. Tubes stopped with plugs of baked cotton corresponded in their products to the last, and to the phials more or less exposed to the influence of the air, one of them presenting beautiful torulæ, like long slender strings of beads.

But if the experiments so far should be considered to show that the access of air is not absolutely necessary for the appearance of organisms as a carrier of animalcules or germs, the question remains—Did not the organisms, or at least their germs, exist as such in the fluids or in one of their constituents from the first, and was the boiling of ten or fifteen minutes, or the boiling of four or five hours in the last experiments, sufficient to destroy them? Respecting germs we can say nothing; but we must concede that moderate boiling is insufficient to disorganise, though it may still or render motionless, when actually present, monads, bacteria, and vibrios: thus bacteria resist boiling of half-an-hours length. In common bilious diarrhœa it will be found that the dejectamenta swarm with the above minute motile forms, and that by boiling little effect is produced on this motility. The same may be said as to the effects of other strong agents—as galvanism, solution of chloroform, dilute sulphuric acid (1 to 12), solution of common salt, dilute carbolic acid (1 to 60), and dilute sulphurous acid (1 to 60).

At the risk of being guilty of digression, the writer may mention, further, that many years ago he experimented on the powerful effects of common salt upon fresh-water animals, especially in stopping the action of their cilia, and *vice versâ* of fresh water upon salt-water animals. It has, as just observed, little effect on the minute form of life in question, though higher fresh-water infusoria—Paramecium, Glaucoma, Stylonichia, or Vorticellæ—are immediately killed in a weak solution of it, as well as by the agents named in the last paragraph. In 1867 he described the effects of permanganate of potash, carbolic acid, and creasote on the higher infusoria; also of galvanism as obtained from a small pile of copper, zinc, and paper discs, having platinum electrodes connected with them, and pasted on the microscopical slide,—the galvanic fluid destroying the infusoria and stopping the ciliary action. These agents destroy the movements also of those Navicellæ so abundant in the green film (Colletonema) which forms where water is constantly trickling, as at drinking-fountains, or in impure sewer-water after keeping it some time in bottles. The movements of the remarkable spermatozoa and spermatothecæ of Paludina are destroyed by the vapor of chloroform—and, in fact, by water itself; but in the latter case from endosmosis. Large vibrois are far more destructible than small ones or than bacteria. When chloroform destroys infusoria, it does not appear to disorganise them much, and therefore its application might be useful in their study. We have noticed that when a raceme of barberry flowers is held over a phial of chloroform, the stamens lose their natural motility; and in its vapor, too, the leaflets of the sensitive plant close, the leaves drop, and small plants are even killed by it.

But, to go back a little to the principle subject matter of this paper, it appears that minute organisms are not proof against the action of the agents in a more concentrated state—of carbolic and sulphurous acids, for instance; nor against severe boiling for four or five hours, as shown by Professor Wyman; nor concentration by boiling; and their germs, if they have true germs, would probably be destroyed in the same way. But Professor Wyman further maintains that when a fluid or substance is so cleared of actual or latent life, life cannot again appear. This last conclusion, however, we doubt.

The next experiments (4) were as follows:—

Fresh, or rather living and contractile, muscular fibre, cut from the centre of a piece of flesh with a bright scalpel, was placed in an equally clean bottle heated in an oven, in which also the apparatus was adjusted; then was added distilled water, obtained from a steam-pipe of an engine working at a pressure of from 20 lb. to 30 lb. to the inch, which neither contained life nor generated it after several days' exposure. After boiling some time a cork was adjusted, through which a tube formed like the letter N was thrust. Asbestos soaked in sulphuric acid was packed in the tube, and the whole heated before being inserted. Immediately after the insertion the bottle was withdrawn, and the cork &c. well cemented with sealing wax. At first the vapor from the bottle escaped outwards through the tube, but the void was shortly refilled by the air passing inwards in a gradual manner through the asbestos. Of these bottles, five in number, the usual forms of life were afterwards found in four, though the fluid was in one or two of the bottles strongly sour, from the acid having gained access.

Two glass-stoppered phials were cleaned, heated, and half-filled with distilled water which contained nothing visible under a high power of microscope: in one was inserted a bit of liver from an animal just killed; in the other, a bit of muscle, still contractile, from the biceps of an arm just amputated for a railway accident. Both bottles had been kept boiling for some time before adding the ingredients, and were stoppered whilst boiling, and the stoppers cemented. In both, after standing, motile bacteria-like particles appeared. Or the phials may be stoppered by fine baked corks, probably transmitting but at the same time filtering the air. Infusion of hay and of belladonna so treated presented abundance of animal forms; solution of morphia none, but only a fine vegetable growth.

A piece of fresh flesh cut from an animal just killed was placed, with due precautions, at the bottom of a tube about a foot long, previously cleaned with caustic fluids, and which was then filled with distilled water, and covered in one experiment with wax, and in the other with oil of turpentine. Life appeared in both cases, and at the bottom of the tube rather than above.

Recently some of Liebig's extract of meat was boiled and semi-carbonised in a teaspoon in the flame of a spirit lamp, then boiling distilled water poured upon it in three tubes, the top being covered with heated oil and wax. After standing about ten days, bacteria, true monads, a few vibrios, and some higher infusoria were found in all three more or less. In the tube where the solution was weakest and the bacteria scarcest, there seemed an abundance of molecules—the first stage of the latter?

The writer, though he cannot offer it, presumes that an *experimentum crucis* would be one where the animal or vegetable substance used could not be supposed, either from strong heating or othewise, to contain any extraneous germs; the water of solution likewise being devoid of them, by being produced, for instance, from steam at a high temperature, or by some other effective way; and the atmosphere being free from any suspicion of its being the medium, say by its being artificially formed from its constituent gases. The difficulties, including those of manipulation, scarcely appear to be of an insurmountable kind, to one more accustomed to chemical operations than the writer. Under the above circumstances forms of life ought, if heterogeny be true, to appear as freely as in ordinary air, which certainly was not the case in most of our experiments. Yet we may have now made it appear that we are inclined to believe that in such experiments neither the air nor the water furnishes the organisms, if organisms they be, but the organic substance used, and especially in a state of change. Further, that germs, ova, or spores do not precede their appearance. They are the constant accompaniments of decomposition, and so associated with the last change or death of organic matter; at any rate, as much this as what is grandiloquently called the "origin of life." In a short time they pass away, and higher infusoria and green algæ and Pediastreæ, as Chlamydococcus, Colletonema, Euglenæ, Uvellæ, and Scenodesmi, appear. With the income of bacteria in a fluid there is presented an appearance, such as has been often noticed, very like a simultaneous generation from dead organic matter, rather than an irregular birth from germs received from the air, though the pellicle certainly often forms at the surface. In this zooglæa myriads of elongated atoms are arranged, either the precursors or the residue of bacteria; and at a later date, as remarked by Dr. Bastian, differentiated vesicular portions are seen, which, when freed by their vibratile force, enlarge and change somewhat in form, and thus become ordinary oval infusoria, such as Glaucoma.

In October, 1865, four experiments (5) were made with simple infusions of bread-crumb, bruised oats, secale cornutum ground, and chopped hay, the vessels containing the liquids being all alike, and simply placed to stand side by side. Though the organisms which appeared were mostly similar to those already named (as to genus), yet they varied a good deal in size and shape in the different vessels, and especially in the date at which they made their appearance, as well as their departure.

In the infusion of secale peculiar monads tardily appeared, and continued for some time, vibrios scarcely appearing at all. If glasses of solution of gelatine, albumen, sugar, and flesh be placed with the same surroundings, similar facts will be noticed: life will appear in the last very early, much later in the first three solutions, or in a compound of all three.

We lately placed three wide-mouthed phials containing distilled water, solution of the white of egg, and solution of flesh, in a dish, on which was poured a stratum of very putrid animal infusion swarming with bacteria. The phials were covered with a bell-glass raised a little above the putrid liquid. Three other phials were placed under another bell-glass with the same surrounding circumstances, but without the putrid liquid. Three others were entirely exposed to the atmosphere. No bacteria or germs could be detected in the dew collected from the interior of the first bell-glass. After some days there was an abundance of life in the solutions of flesh, some appearance of it in the solutions of albumen, none definite (as long as the experiment lasted) in the water. The genesis of the forms was about contemporaneous in all the sets, though the fully exposed solution of flesh appeared to undergo its changes more freely. Such a variation in advent, increase, shape, and disappearance of these forms of life, as seen in the experiments recorded in the preceding paragraph, and their non-variation in these, must be an argument against their origin from the incumbent atmosphere, to say nothing of the difficulty of supposing the spores or eggs of aquatic organisms to exist there. Though we have found that a microscopical slide placed over fermenting beer, or fading diabetic urine, exhibited the spherules of the yeast-plant, even in sequence, as well as minute molecules, yet tufts of developed and fructifying fungi seen on the surface of the liquids accounted for this; and other slides, placed over putrid fluids, presented no bacteria or vibrios, even if the zooglœa was present. We, like others, have occasionally revived these forms of life, after being dried up, by applying moisture, but have as often failed. It must be conceded, however, that the lower vegetable forms, as fungi, algæ, and lichens, originate in suitable localities from spores falling from the air; and these air-borne spores must be of different species, sufficient to produce all the objects of our observation, in cases of low terrestrial vegetation, or else we should have to suppose that different species of organisms arise from *one* kind of germ or germinal matter, which doctrine would be but a small remove from that of the heterogenist.

It must be allowed, though the appearance of these vibrios and bacteria in fluids that have been but moderately boiled and sealed from the air, may not satisfactorily prove that they have been generated from dead organic matter, it being possible that they or their germs, notwithstanding the boiling, may have been bodily present all along; yet, if it is true that they are often to be detected after such boiling and sealing, then a doubt is thrown upon certain experiments—those of Pasteur, for instance,—justifying the serious distrust that has been expressed by Dr. Child and others, not of his manipulations, but of his microscopical examination of the products of these experiments.

The use of ferments to produce, or rather hasten, certain effects in otherwise suitable substances, may be an argument against the opinion which the writer is disposed to hold; and the opinion also appears contrary to our experience with respect to higher organisms. Yet on the whole, we see no want of philosophy in believing that these very lowest forms of life, whether they are only animal constituents or animals themselves, may arise *de novo* out of suitable nitrogenous matter, without ova or germs of any kind, yet not, we imagine, from the operation of any physical agent or agents, as heat (though, of all physical agents, heat seems most correlative with life), but rather by a force, *sui generis*, advocated by the school of Hunter and, we think, unless we misunderstand him, by Dr. Beale amongst modern microscopists, though such a doctrine is far from popular. Further, we think that the advocates of the so-called *spontaneous generation* should not be set down as heterodox on that score, for they may think that such views necessitate something more than physical agents to act on organic matter in order to produce the manifestation of life; and though this may be every moment occurring, still it is *in the beginning*, if not in time, yet in animal existence at its very start—and, indeed, it is the view which the literalist might probably take of life—an aquatic genesis or *bringing forth*.

At present there may be no proof that these low organisms have any relation to the causation of general zymotic diseases—as, for instance, of Asiatic cholera. What we have said, however, respecting the *dejectamenta* in ordinary diarrhœa may be recollected. Whether microzoa are a cause or a product of disease, or whether their generation is noxious, or the reverse, may not be certain. We may mention a few experiments, if experiments they may be called, which we have occasionally tried. With respect to small-pox, we have found that glass slides, moistened or not with glycerine, placed in different positions in a ward which was rather crowded with small-pox patients at various stages of the disease, presented, upon examination under the microscope, irregular brownish scales, with granular matter attached to them, evidently from the diseased skin, also loose granules or molecules, and, of course, hairs, epithelium, and foreign matter. In such an instance the *materies morbi* must readily be inhaled by an attendant in a pretty substantial form. The breath of a patient with malignant scarlet fever was received and condensed on a slide, and when examined it presented some elongated monads or bacteria, evincing an evident spontaneous movement. Granular particles, answering to the small molecules found, as mentioned above, on the slide suspended above the decaying diabetic urine, seemed also to exist on glasses placed for a few hours over the skin in small-pox and scarlatina, and in the latter disease apparently in the renal and nasal secretions. Such atoms may be worthy or not of further notice; at any rate, the subject would require the examination of an adept furnished with the *best of instruments*. In two or three trials I could discern no trace of life in pus from diseased joints, &c.; whilst, on the contrary, the muco-purulent secretion from a patient with diseased bladder presented moving bacteria of a somewhat peculiar appearance. In a case of diphtheria the expectorated membrane exhibited a somewhat fibrous stroma, with oval corpuscles rather distant, but apparently regularly disposed in it; but in a long fibrous cast, lately brought up in a fatal case of the same disease, occurring in an adult, the structure was of a more granular appearance.

Stoke-upon-Trent, May, 1872.

AN IMPROVED PERIMETER.

(FOR MEASURING THE FIELD OF VISION.)

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IN many of the diseases of the eye that superficially appear to be chiefly local in their character, and in numerous cases also in which defects of sight are manifestly due to the reaction of central nerve lesion, the power to measure the field of vision with accuracy and readiness is of the greatest importance to the practitioner. In some instances the extent and shape of the field will at once determine a diagnosis that must otherwise have remained doubtful for an uncertain period; and in many the same data will afford the only trustworthy evidence with regard to improvement or retrogression. On these grounds the practice of perimetry has long been cultivated by ophthalmologists, and has lately been recognised as essential by physicians engaged in the study and treatment of diseases of the nervous system.

The early attempts at measuring the field of vision were of a rude and primitive character. A small white cross was marked on the centre of a black board, which was hung against a wall. The patient was placed with the eye under examination level with this cross, and at a measured distance of from eight to twelve inches away from it. He was directed to look steadily at the centre of the cross, which was called the "fixing point," and the other eye was closed or covered. The operator, armed with a black wand having a white tip, and with a piece of chalk, stood behind or at the side of the patient, and drew the white tip of the wand slowly along the board, from its margin towards its centre, in successive directions, usually first in a vertical line, next in a horizontal, and then in two lines intermediate between the former ones. The patient was directed to speak as soon as the white tip came into view, and at this point a chalk dot was made on the board. The patient was directed to speak again as soon as the white tip, in its onward movement towards the fixing point, became *distinctly* visible, and here a second dot was made. When the examination was completed there were two chalk dots on each meridian of movement. A line uniting all the external dots would show the absolute boundaries of the field of visual perception, and a line uniting the internal dots would show the boundaries of the field of distinct vision.

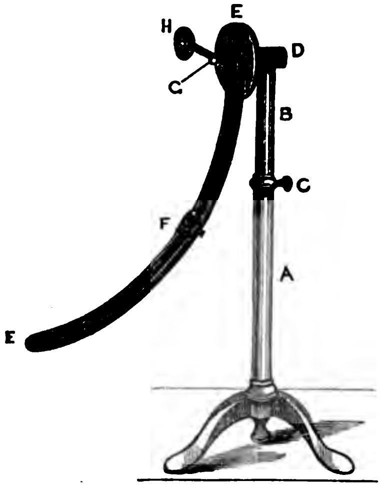
The maps afforded by this process were of great value, especially in the earlier studies of glaucoma and of nerve atrophy; but the process was open to the objection that the eye of the patient, which was concealed from the view of the operator, was apt to wander from the fixing point towards the moving object, and thus to vitiate the whole result. To meet this difficulty, Professor Donders made a hole in the centre of the board, and placed it in a vertical frame adapted to stand upon a table. The patient was directed to look through the hole at the eye of a second person suitably placed upon the other side of the board; and this second person gave notice if the eye under observation wandered. It is manifest that a certain amount of care and accuracy would be required from the second observer, and that the method, however occasionally useful, would render perimetry dependent upon the presence of at least a moderately skilled assistant. The flat board system, moreover, is open to the objection that the central fixing point is nearer the eye than any lateral portion of the surface.

To obviate these inconveniences several contrivances have from time to time been suggested; but that which has found most general acceptance is the perimeter of Professor Förster, of Breslau. It consists of a strong wooden platform, measuring fifteen inches by nineteen, supporting at one end a double chin-rest, and having at the other a stout, upright iron pillar. This pillar carries a semicircular iron band, two inches wide and of twelve inches radius, placed with its concavity towards the chin-rest, and turning freely at its middle point on a horizontal axis, so that it can be placed vertically, horizontally, or in any intermediate position. A movable slide carrying a white spot, travels freely to and fro over the concave face of the band, and is worked by an arrangement of cords and pulleys set in motion by a winch-handle. A rod proceeding from the base of the chin-rest carries a slender stem which is placed close to the band, which turns freely right or left, and on which slides a small ivory ball. The chin of the patient is placed on the chin-rest, on the proper side for the eye under examination, and (the other eye being closed) the ivory ball is used as a fixing-point, and is adjusted at such a distance on the nasal side of the eye examined as to make the middle of the band correspond with the blind spot. The same fixation being maintained, the band is placed in successive meridians, and the appearance and disappearance of the moving spot as it travels round the arc are recorded on a diagram prepared for the purpose. The operator stands behind the arc, opposite to the patient, and is thus able to watch the direction of the eye, and to see that it does not deviate from the fixing-point. The face of the arc is divided into degrees, and its axis carries a circle similarly divided, so that a map of great accuracy can in this way be obtained. The eccentric fixation-point has the advantage of rendering the optic disc, instead of the macula lutea, the centre of the map, which thus corresponds to the ordinary course of ophthalmoscopic examination.

The principle of Professor Förster's instrument is perfectly sound; but its details are excessively faulty. It is very large, heavy, cumbrous, and costly; its strings are constantly becoming loose, or twisted, or otherwise out of gear; its chin-rest is too low, the instrument being on a table, for the comfort of the great majority of patients; and the superfluous half of its semicircular band is perpetually getting in the way. Like other people, I had borne these inconveniences with more or less grumbling, until Mr. Hawksley, of Blenheim-street, one day called upon me, wishing to examine the instrument on the part of a gentleman who desired to have one. I pointed out the faults to which I have referred, and made some suggestions for overcoming them; and the result has been the construction of the perimeter I am now about to describe.

It consists of a simple tripod, supporting a hollow stem (Fig. 1, A), in which a second stem (B) moves up and down, and can be fixed at any desired height by the screw (C). At the top of the stem (B) is a short horizontal axis (D), carrying the quadrant (E, E′), which turns in a complete circle, and moves with just stiffness enough to remain wherever it is placed. On the quadrant is a travelling slide (F), with a white spot; and a second independent axis is inserted in the axis of the quadrant at G, and carries a short tube, in which may be placed a stem to support the fixing point. The second or inner axis makes a complete revolution without affecting the position of the quadrant, and without being affected by it. At its attached extremity the quadrant terminates in a circular disc (E′), which is graduated into degrees at the back, and a fixed index allows the exact position of the quadrant to be read off. The quadrant is also graduated from ten degrees to ninety, on its concave face, so as to show the exact position of the slide. The fixing-point may be either an ivory knob at the end of a wire, or, what is for most purposes better, a small disc with a central perforation, as shown at H, *through* which the patient looks at an object on the other side of the room, and obtains fixation without exercise of the accomodation and consequent fatigue to the eye. The travelling slide (F) may be made to carry a spot of any color or size that is desired, and it is furnished with a ring at the back, by which it may be moved by means of a hook set in a handle, so that its position may not be indicated by that of the hand of the operator. For the purpose of taking exact measurements of the blind spot, the quadrant is graduated at the back from eight degrees to twenty-five, in degrees and sixths of a degree; and a white spot is placed on the centre of the axis (G), to serve as a fixing-point for this particular purpose.

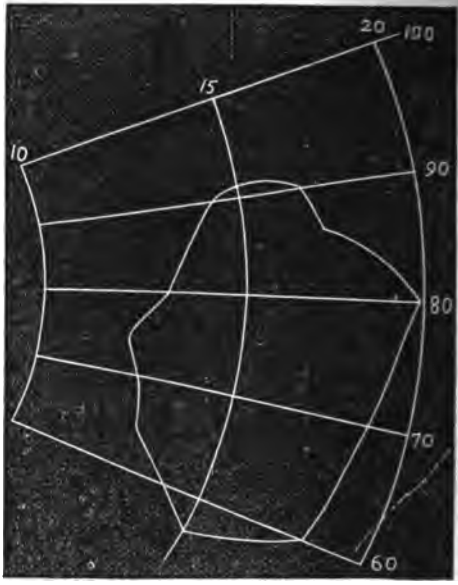
FIG. 1.



In order to use the instrument, the patient is comfortably seated in front of a table, with his chin supported by the rest of a demonstrating ophthalmoscope. The perimeter is then set at such a height as to have the centre of its axis on a level with his eyes, and is placed with this centre exactly in front of the eye to be examined (i.e., an inch and a quarter to the right or left of the median line, according to the eye), and exactly twelve inches distant from it. The patient closes the eye not under examination, and covers it with the hand, resting his elbow upon the table for the sake of greater steadiness.

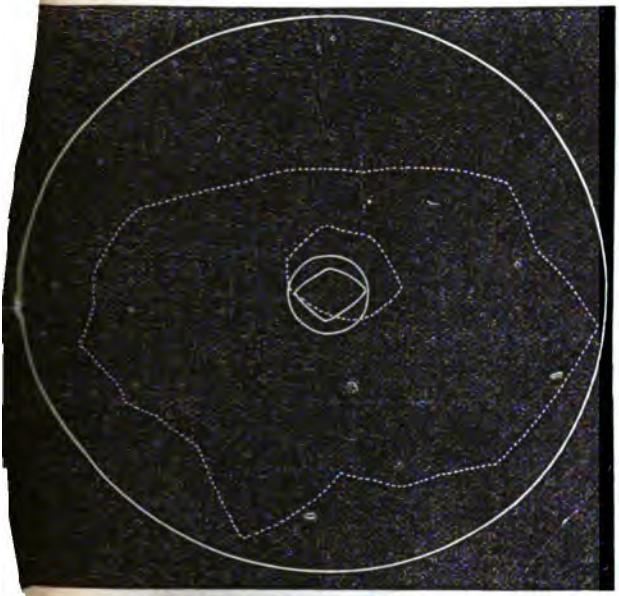
In order to measure the blind spot, the smallest white object is fixed to the slide, and the quadrant raised until it is nearly horizontal on the temporal side of the eye under examination. The patient is directed to fix the central point, and the slide is moved from outwards towards the centre, until its white spot disappears, which it will usually do at about 17°. Continuing the same movement, it reappears, say at about 14°; thus giving the horizontal width of the blind spot in that meridian. Placing the white spot half-way between the two points, at 15½°, the vertical measurement may be taken by raising and lowering the quadrant until the spot reappears in each direction, usually at the meridians of 90° and 58°. The horizontal and vertical measurements may thus be taken in as many meridians as we desire; and an exact outline of the part of the field corresponding to the blind spot may be traced upon a chart. I have designed the foregoing chart (Fig. 2) for this purpose, and have traced upon it the gap in the field of my own right eye for an illustration.

FIG. 2.



In order to measure the field, the perforated disc must be used for fixation, and must be set so that when the patient looks through the hole at a distant point the centre of the axis corresponds to the centre of the blind spot. For this purpose, in a normal eye, the centre of the perforation should be 10° above the horizontal meridian and 17½° from the axis on the nasal side of the eye under examination. The patient is then instructed to look through the hole, the instrument being placed as before, and the eye not under examination closed. The operator moves the slide from without inwards along successive meridians until he has completed the circle, and marks the limits of perception and clear vision on a chart as he proceeds. The following diagram presents two continuous line *circles*, as data, the outer having a radius of 70°, the inner a radius of 10°. The outer dotted line shows

FIG. 3.



the limits of visual perception of my own right eye. The continuous line within the inner circle shows the limits within which I can read an unknown letter of Snellen's No. 8 test types; and the inner dotted line, which shows the entire field of vision of a patient with pigmentary retinitis, may serve to illustrate some of the clinical uses of the perimeter. The last two outlines are drawn from the fixing point as a centre.

In mapping the field one may of course proceed in any order; but there are some advantages in a regular method. I usually commence from zero, with the quadrant vertically downwards, and move it upwards towards the temporal side of the eye under examination. We may of course take a chart of the field by placing our marks on meridians corresponding to the positions of the quadrant (e. g., below the centre when the quadrant is downwards); or a chart of the percipient portion of the retina by placing the marks in contrary positions (e. g., above the centre when the quadrant is downwards). To render the charts universally intelligible the same plan should be adopted by all observers. Of the two named, the former is clearly the best: first, because it is the more simple; secondly, because the chart so drawn will correspond with the more commonly employed inverted ophthalmoscopic image.

Since the woodcut has been taken I have had two small additions made to the instrument. The first is a little box beneath the tripod, which holds spots of various sizes and colors for the slide; and also a white knob on a stem, to be substituted for the perforated disc when it is desired to measure in the meridian of the fixing point, and hence to place the quadrant behind the perforation. The second is a screw on the axis D, by which the quadrant can be fixed or rendered easily movable at pleasure. By slackening the screw the quadrant can be turned to another meridian, without risk of displacing the general position of the instrument.

Wimpole-street, June, 1872.

CLINICAL REMARKS

ON A

CASE OF ANEURISM OF THE ASCENDING PORTION OF THE ARCH OF THE AORTA,

PRESENTING ANOMALOUS SYMPTOMS.

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I AM indebted to the kindness of Dr. Lionel Beale, who was good enough to place one of his beds at my disposal, for the opportunity of following the clinical history of this instructive case to its fatal issue. Its chief interest arises from the difficulty that was experienced, during the life of the patient, in determining the precise morbid condition which gave rise to the distressing symptoms from which he suffered, and which, in no long time, brought his existence to a most painful termination. It was one of those cases in which a large aneurism existed close to the walls of the chest, without giving rise to any of those local signs which are regarded as characteristic of aneurismal tumours.

Before this man came under my care in King's College Hospital he had been a patient at two other metropolitan hospitals; and he presented himself with a written diagnosis of his case as one of "mediastinal tumour." This was the conclusion that had been arrived at by the physicians who had already examined him, and we arrived at a similar one. My colleagues, Dr. Beale, Dr. Duffin, and Dr. Baxter, examined the case carefully with me, and we were quite agreed as to the inferences to be drawn from the symptoms we observed. To what extent the previous diagnosis may, unconsciously, have influenced our own it is not possible to discover; but that such unconscious influences do occasionally guide the current of our thoughts, and determine the nature of our conclusions, is not to be doubted.

But before I make any further remarks on this case, I will briefly relate its history and the symptoms which attended its course.

The patient was a man forty-four years of age, and for twenty-two years had served in the British army. He appears to have been a steady, sober, well-conducted man, and was a sergeant when he left the service and joined the Corps of Commissionaires four years ago. With the exception of three attacks of intermittent fever in India, he had had no previous illnesses, and he had never suffered from syphilis. His father and mother were living and healthy, so were all his brothers and sisters; moreover he was married and had four healthy children.

When first he came under my observation in June last, he stated that about six months before he began to complain of slight difficulty of breathing, accompanied with a dry, tickling cough, both of which gradually became worse. About a month later *the glands on both sides of the neck became enlarged, as well as those in the right axilla*, and he could not stoop down without feeling giddy. At that time he became first an out-patient and afterwards an in-patient of the Metropolitan Hospital, and whilst there the presence of a tumour in the anterior mediastinum was diagnosed. The enlargement of the cervical and axillary glands continued for about six weeks, and then subsided. He stated that the superficial veins on both sides of the thorax and of both arms became prominent and distended during the time the glands in the neck and axilla were enlarged. Up to this time his general health had been good; he had a good appetite, and did not lose flesh; he complained, however, of the troublesome tickling cough, of a constant pain between the shoulders, and of an inability to lie down in bed with comfort.

About the end of last June—that is, about six months after the first appearance of his cough—he became an out-patient at the Brompton Hospital, and subsequently an in-patient in King's College Hospital. At this time the following notes of his condition were made:—

"He complains of severe cough and dyspnœa, with pain in the right subclavicular region, extending to the adjacent part of the sternum. The cough is of a curious ringing and incomplete character, the paroxysm being suddenly arrested or cut short; it is immediately brought on by depressing the chin, or assuming the recumbent position, or even by pressure of the stethoscope on the back of the chest between the scapulæ. He has no difficulty in swallowing, and never has had any. On visual examination, there is a distinct and uniform bulging to be observed anteriorly, over the upper part of the right side of the thorax, between the clavicle and the nipple. There is almost absolute dulness on percussion over this region, and the dulness extends horizontally across the upper part of the sternum, and invades the left infraclavicular region, as far as, or rather beyond, the inner third of the left clavicle. Moreover, on the right side the dulness extends vertically downwards, with little variation in intensity to that of the upper margin of the liver. Over this dull area the respiratory sound is distant and feeble. In the right supra- and infra-clavicular regions a loud and harsh grating sound is heard with the breathing, while over the right apex posteriorly loud harsh bronchial râles are heard, which extend (probably conducted) over the whole of the right lung. The resonance behind is equal on both sides, and the respiratory murmur at both bases is feeble. There is an absence of vocal fremitus on the right side in front. The heart-sounds are heard feebly over the dull area; they are normal, and the organ is not displaced. Over a circumscribed spot corresponding to the heart's apex there is heard a somewhat indistinct double friction-sound synchronous with the heart's action; immediately external to the limits of the præcordia it adopts the rhythm of the respiratory movements, and is very distinct as a double pleuritic rub around the base of the left lung, as far as the left scapular angle. The superficial abdominal veins are enormously distended, and their anastomoses with the thoracic veins distinctly manifest; but the veins of both upper extremities are normal, as well as the two jugulars. The venous distension is limited chiefly to the veins below the nipple. The radial pulse on the right side cannot be distinctly made out; on the left side it beats 120 in the minute; temperature 102°. The face is livid, and wears an anxious expression; the pupils small but equal. There is no alteration of the voice. *No unusual impulse, no thrill, no abnormal cardiac or arterial murmurs, can anywhere be distinguished.* At present there is no enlargement of the cervical or axillary glands. On careful measurement it is noticed that the level of the nipple on the right side is half an inch lower than the left, and that the right half-circumference of the chest, on a level with the nipples, is two inches and a half greater than the left."

Soon after his admission into the hospital the external jugular veins and the veins of the upper extremities became again distended, and it was noticed that the jugular veins filled from below if they were obstructed by pressing upon them above. This was associated with extreme orthopnœa and lividity of the face, conditions pointing to great pulmonary obstruction. At the same time the epigastric veins were enormously dilated and tortuous, and the current in them appeared to flow from above downwards. The cough, at this period, became very troublesome, and was accompanied with abundant muco-purulent expectoration. Paroxysms of difficulty of breathing were frequent and very distressing. The hands and feet were cold and livid, but not œdematous. He could neither lie back nor bend forwards, since both of these positions caused violent coughing, so he sat upright in bed, with his hands on his knees, breathing with great difficulty.

He derived a good deal of temporary relief from a draught every four hours, containing morphia, ether, and carbonate of ammonia. For several days the breathing became easier, the cough less frequent, the efforts to expectorate less arduous, and he obtained some quiet sleep.

On the 6th of July the whole circumference of the chest exceeded the previous measurement on the 17th of June by an inch, and the right half-circumference by half an inch.

The improvement in the more urgent symptoms was not of long duration, and on one occasion, when the distress of breathing was extreme and suffocation appeared to be impending, Mr. Roche, the house-surgeon, opened one of the greatly distended jugular veins, the right, and withdrew about six ounces of blood. This proceeding was followed by much relief to the breathing, and the patient was enabled to obtain some sleep. A few days later (July 10th) another attempt was made to relieve the paroxysm of dyspnœa by letting blood from one of the brachial veins, but without any good result. The case was rapidly approaching its end. At this period the poor man could obtain no relief in any position; no relief from any drug; night and day were passed in an agony of distress—restless, sleepless,—and we could only stand by sad and helpless spectators of this strange unequal fate. He had not inherited disease; he had done nothing to acquire disease; for a man in his position he had led a singularly blameless life; yet we saw him wrestling, in the prime of life and the perfect vigor of his faculities, with a cruel, miserable, and inevitable death. It was one of those terrible, unaccountable destinies that we are occasionally called upon to contemplate. He died, exhausted with suffering, after he had been in the hospital about a fortnight.

The local signs remained the same throughout, except that the bulging of the upper part of the right side of the chest slightly increased. But there was never at any time any abnormal vascular or cardiac murmur, thrill, or impulse to be heard, felt, or seen.

The following is the account of the post-mortem examination, which has been furnished me by Dr. Kelly:—On opening the thorax, the right lung was seen to be partially collapsed, and the pleura over it was covered with recent lymph. There were also some old adhesions between the right lung, chest-wall, and pericardium. The left lung was non-adherent, but there was some soft recent lymph on the surface of the lower lobe. Lying behind the top of the sternum, and bounded on either side by the lungs, lay a nearly oval tumour, which proved to be an aneurismal sac springing from the front part of the ascending arch of the aorta. The sac was about 3 in. by 2, with rather thin walls, and partly filled by a pale, fibrinous clot. The aneurism was so situated that the left innominate vein ran above its upper surface, and joined the right innominate, to form the vena cava superior which ran along the right border of the tumour. The aneurism communicated with the aorta by an oval opening near the junction of the ascending and transverse portions of the arch; it then came forward, *and spread nearly uniformly in all directions*, so that it lay quite in front of the aorta and innominate. The heart was rather larger than usual; apex displaced a little downwards, valves healthy, walls flabby. The aorta was roughened and dilated just above the valves, and in several parts of its course was thickened with atheromatous deposit. The innominate, left carotid, and left subclavian arteries were healthy. The radial arteries on either side were healthy, and of the usual calibre; the right, however, was deeper seated than usual. The aneurismal sac pressed on the trachea and bronchi, and the mucous membrane corresponding seemed rather inflamed. Muco-purulent fluid exuded from the bronchi on section. The other organs were healthy.

This case, the history of which I have now related, was one of those where the diagnosis between the existence of an aortic aneurism and that of a solid mediastinal tumour becomes extremely difficult, if not altogether impossible. There were, doubtless, in this case many of the symptoms which accompany thoracic aneurism—I allude especially to the signs of intrathoracic pressure; the obstructed return of blood evidenced by the enlargement and tortuosity of the superficial veins; the presence of a hoarse clanging cough, the sense of suffocation, the orthopnœa;—but then all these pressure signs might be equally well produced by a mediastinal tumour which was not aneurismal, while in favor of the presumption that such was really the nature of the disease we had none of those local signs which in the majority of cases accompany a large aneurism close to the anterior chest-wall. There was no impulse over the prominent portion of the thorax, no thrill, no abnormal cardiac or vascular sound whatever. Then, again, the prominence of the chest-wall, though considerable, was more uniform and diffused, and not pointed, as is usually the case with aneurisms in this situation. We also found a most unusual extent of percussion dulness, extending uninterruptedly far beyond the course and situation of the aortic arch, invading both sides of the chest, and extending on the right side to the upper level of hepatic dulness. We had further the history of enlarged cervical and axillary glands, and we had occasional rises of temperature to 102°, a circumstance which has been observed in connexion with intrathoracic growths, but upon which no importance can be placed, as it is commonly caused, as in this case, by intercurrent attacks of pleuritic inflammation.

Taking all these circumstances into consideration, we were led to prefer the opinion that this patient was suffering from a large and rapidly-increasing intrathoracic growth, occupying chiefly the anterior mediastinum—an opinion which the sequel of the case proved to be incorrect. The case was, however, altogether an anomalous one, and the deviation from the symptoms usually observed in such cases may be, to some extent, accounted for by the spreading character of the aneurismal tumour, and by the fact that it was nearly filled with fibrinous clot, while pressure on the trachea and right bronchus had led to collapse and condensation of a great portion of the right lung, and produced a corresponding amount of percussion dulness.

I may just allude, in conclusion, to the bold, and to my thinking perfectly justifiable, expedient which our house-surgeon, Mr. Roche, had recourse to, of opening one of the jugular veins when the symptoms of pulmonary obstruction became very threatening—a proceeding which, for a time, gave the patient much relief.

St. James's-street, May, 1872.

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TWO CASES OF LIGATURE OF LARGE ARTERIES.

Performed at the Royal Naval Hospital, Plymouth,

By Robert Bernard, M.D.,

Deputy Inspector-General.

(*Communicated by* The Director-General of the Medical Department of the Navy.)

I.—LIGATURE OF THE EXTERNAL ILIAC ARTERY FOR INGUINAL ANEURISM.

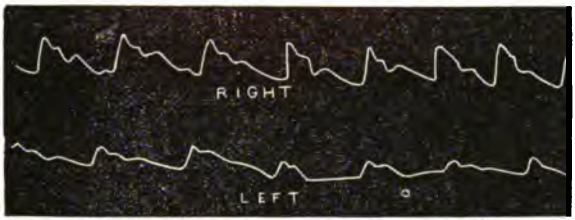
W. J——, aged thirty-nine, private marine, admitted from H.M.S. *Indus* on the 6th February, 1872. Has been twenty years in the service, and enjoyed fair health. Served for four years in India and China, where he suffered from syphilis. Has been accustomed to drink freely. His attention was called to his present affection a month since. First felt pain and cramp in walking; then observed a varicose state of the veins of the affected limb; and still later a swelling in the upper part of the thigh. This latter increased rapidly, but more particularly within the last forty-eight hours. He is not conscious of having sustained any injury.

On examination, a large fusiform aneurismal tumour is found to fill the upper two-thirds of Scarpa's triangle on the right side, and extending a little above Poupart's ligament. This pulsates strongly, the pulsation being readily commanded by pressure on the external iliac artery. No abnormal sign of disease can be detected by the ear in the heart or other arteries, but the sphygmograph shows a considerable difference between the radial pulses. (See tracing on next page.)

He was ordered a bladder of ice and salt to the tumour; a castor-oil draught; and the administration of a large enema, by a long tube, in the morning.

Feb. 7th.—The large intestines having been well emptied by the enema, he was placed under chloroform this morning, and the operation for securing the external iliac artery proceeded with.

Operation.—An incision was made extending from one inch above and to the inner side of the anterior superior spine of the ilium, downwards and inwards as far as the external abdominal ring, laying bare the external oblique muscle, which was then slit up to the same extent. The internal oblique and transversalis were carefully divided, the transversalis fascia pinched up, cut horizontally, and separated by the finger from the subjacent peritoneum, which latter uninjured was readily raised from the iliac fossa, drawn upwards and inwards, and retained by wooden retractors. The artery came now into view with unusual distinctness, and appeared large but otherwise healthy; its investing fascia was torn through by two pairs of long forceps, and a thick catgut ligature (carbolised) passed with ease round it. This was secured by the ordinary knot, the ends cut short, and the wound brought together by six points of suture. There was considerable superficial venous congestion, and three arteries were secured during the operation. On the vessel being tied, which was done about two inches above Poupart's ligament, all pulsation in the tumour immediately ceased. The wound was dressed with carbolised oil, a pad and bandage.



8th.—Complained of numbness of foot during the night; slept fairly under the influence of opium. This morning there is slight constitutional disturbance. Pulse 108; temperature of affected thigh 99°, being 3° higher than that of the left.

9th.—Has complained a good deal of pain and uneasiness of ankle; much easier this morning, and with less pyrexia. Temperature in axilla, 101°; in right thigh, 97°; in right foot, 93°; in left thigh, 96°; in left foot, 95°. Aneurismal tumour less, feels harder and free from pulsation.

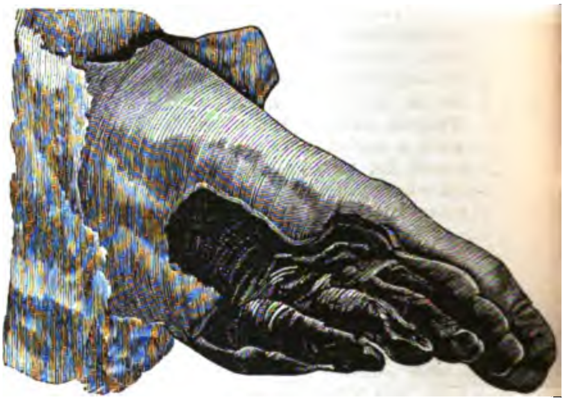
11th.—Wound was dressed and looks well. Carbolised oil dressing (1 to 50) reapplied. Has had some abdominal uneasiness, relieved by the bowels being opened by an enema; appetite improving.

13th.—General improvement until this evening, when he became almost suddenly delirious, fancying he saw insects and devils crawling round him; but with this no excitement of pulse, or tremor, or abnormal condition of urine. The wound was examined, and at its edges found to be in a highly inflamed state, almost sphaceloid. Opiate dressing was immediately substituted for the carbolic acid, and opium given freely during the night, also some wine and brandy. A restless, talkative delirium continued, but he fell asleep in the morning, and after a sound sleep awoke quite coherent; wound also looking much less angry.

19th.—Since last report the surgical wound has gone on favorably. The aneurismal tumour is smaller, more consolidated, and pulseless. There has been continued uneasiness and pain of foot. The little toe is cold, and begins to assume a dark hue. No pulsation to be felt anywhere below the ligature; but the temperature of the leg and thigh is normal, and the blood in the veins appears to circulate freely.

March 7th.—Has suffered much from pain of toes and foot, which was relieved by subcutaneous injections of morphia and the local application of belladonna. The three outer toes are now shrivelled and gangrenous (mummiform). Surgical wound doing well, but for a few days "grumous purulent" matter has been discharged, concomitant with which a rapid diminution of the tumour has taken place. The general health is fair and the appetite excellent, but a certain amount of constitutional disturbance is necessarily kept up by the pain of the foot and the effect of narcotics.

31st.—Since last report the surgical wound has rapidly contracted, and may now be said to be closed. Some slight thickening, indicating the position of the large aneurismal tumour. No return of pulsation below the seat of deligation. General health excellent. The pain in the foot is much less, but all the toes have become engaged, and the gangrene has extended a little on the outer half of the dorsum of the foot. This process has now, however, entirely ceased, and a line of demarcation has formed, isolating the disease. The accompanying photograph shows the exact state of the foot this day.



April 27th.—Going on well in every respect. Wound healed, and the gangrenous part rapidly separating, leaving healthy granulations beneath. At a future time partial amputation of the foot will be resorted to, in all probability Chopart's operation.

This case appears to possess several features of interest—first, the successful deligation of this large artery by carbolised catgut ligature, and cure of the aneurism; secondly, the delirium, no doubt caused by dressing the wound unprotected with carbolised oil; thirdly, the partial mummiform and gangrene which took place in the foot.

II.—DELIGATION OF THE LEFT COMMON CAROTID ARTERY FOR HÆMORRHAGE FROM FAUCES.

W. D——, aged twenty-five, A.B., admitted

February 5th, 1872, for phagedænic ulceration of the throat, supposed to be syphilitic. He is a married man; wife healthy; no children. He stated that he had primary syphilis nine years ago, and not since. Has never suffered to his knowledge from the disease in a constitutional form. He is a delicate, anæmic-looking man, who only presented himself yesterday to the surgeon of his ship; and states that he has suffered from his present symptoms (which he attributes to a cold) for the last month.

On examination there is no syphilitic eruption on the skin, but the velum palati is found to have disappeared from recent ulceration, and the left arch of the palate and back of the pharynx presents a sloughing mass, which appears to extend downwards, and to the left side, including the left tonsil. There is great difficulty of deglutition, and all solid food is rejected. There is no glandular enlargement, inguinal or cervical.

Feb. 26th.—Under the use of the mineral acids, mercurial vapor baths, and supporting nutriment, at first given in the form of enemata, the general health improved materially, and the throat assumed a more healthy action, several sloughs having been detached. This morning, however, after a slight fit of coughing, violent hæmorrhage suddenly came on, and he ejected from the throat a large quantity of arterial blood, which is described as having come "per saltum," and amounted to fully five pints. On being visited, he was found in a state of complete collapse, all bleeding having ceased. From this he rallied with great difficulty, and when reaction had fairly set in it was thought advisable to secure the left common carotid artery, the hæmorrhage having evidently come from a large artery on that side.

Operation.—An incision was made, commencing one inch below the cricoid cartilage, along the inner border of the left sterno-mastoid muscle, extending upwards to the extent of two inches. The sheath having been exposed was opened on its tracheal side, and the aneurismal needle, armed with a carbolised catgut ligature, readily passed round the vessel a little above the crossing of the omo-hyoid muscle. Neither jugular vein nor pneumogastric nerve was brought into view.

28th.—Has gone on very well since the operation, having been nourished by nutrient enemata. The only cerebral symptom observed was extreme drowsiness.

29th.—The throat continues to clean. Can swallow milk and beef-tea to-day.

March 18th.—Has not had any unfavorable symptom since the operation, gains strength, and takes his food well. Surgical wound healed except at one small point, through which, this day, a foreign body (evidently the knot of the ligature) was discharged.

April 27th.—General health excellent; throat nearly well; still some oozing from a speck in the centre of surgical wound. The arteries, above the ligatured vessel, show no return of pulsation.

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OPHTHALMIC NOTELETS.

By J. F. Streatfeild, F.R.C.S.

In the few little practical notes I now propose to issue, I have chosen those for publication which represent something, at least, which is original, and which may or may not seem to be worthy of more notice and of greater extension by myself hereafter or by others whom they may interest by-and by.

I. ON THE PRESERVATION OR DESTRUCTION OF THE CONJUNCTIVA IN CASES OF EXTIRPATION OF THE GLOBE.

When surgeons, not so very many years ago, used, ignorantly, very rarely to remove an eyeball, and never, probably, unless the case was supposed to be one of cancer, whether invading or not the surrounding orbital tissues, they took a long sharp-pointed knife, curved on the flat, and with a circular cut, including the eye, they removed it and a large portion of the neighboring soft parts: the conjunctiva, in the general confusion, was, for the most part, and irregularly, also excised. But when it was found, by the injurious effects of sympathetic irritation and inflammation, that very many lost eyes (certainly all that are blind, in the ophthalmic sense, and painful) should be removed, and also that seldom comparatively any more than just the eyeball itself need be removed, and that when, and though, it must be removed, as little as possible need be done, a mode of operating for this purpose was re-introduced, sometimes now, therefore, called "enucleation": the conjunctiva was henceforth all and entirely to be carefully preserved, not only for the sake of its preservation, and that only as much as need be should be done in the operation, but to give a regular space, as large as possible, and sufficient room for the wearing of an artificial eye of good size. A considerable cavity, called the "conjunctival sac," is left thus after the operation, now usually done as I have said. In the centre, at the bottom of this space, is to be felt a nodule with a cruciform indentation, where the four rectus tendons are lying involved in the central dense cicatricial tissue, and are seen to move it about in some degree when the other eye is moved. And this hard mass is to support the artificial eye, and by the muscles of the excised eye it is to get all the movement that can be now obtained for an artificial eye. And this is the best we can do; for "abscission" is an operation that must be avoided, and will be certainly soon altogether given up. All this I believe to be indisputably true, and the best line of practice of which we have at present any certain intimation in a general way. It holds good in our private practice in nearly all cases, because the patients will almost invariably desire, as they can well afford the expense and the trouble involved in, the wearing of an artificial eye. But in cases of some few of these well-to-do patients of a different mind, and in very many of those of our hospital patients, this trouble and expense, if well understood beforehand by the patients themselves, will not be entertained, or, if it is entertained, it is in the future found to be impracticable. Some patients, again, are met with, whether in private or in hospital practice, who either before or since the excision operation are quite incapable, and cannot by any surgical method be made capable, of wearing an artificial eye, even if they desire it; such are the cases of irregular cicatrisation within the lids, caused by wounds or burns, or caustics, or ill-surgery, and producing obstinate entropion, strong cicatricial bands in any direction (that cannot be isolated by a probe passed beneath them), or a narrowing of the whole conjunctival sac or of the palpebral aperture by any of the above causes. Once again, though the palpebral aperture be not narrowed nor the conjunctival sac contracted, there may be an obstinate ectropion, probably of the lower lid, which cannot be so well remedied by any surgical method as securely to retain an artificial eye.

I beg to propose now, instead of the unvarying rule of the preservation in all cases of the whole or as much as possible of the conjunctiva whenever the operation of extirpation of the globe is or has been done, its total destruction in the following cases.

1st. When the patient entertains an objection to the having to wear a glass eye, either before the excision operation or at any subsequent time, because of (*a*) the trouble it occasions, (*b*) the recurring expense, (*c*) the annoyance of conjunctival discharges, &c.; and therefore he, after a full explanation, gives up the idea of wearing an artificial eye for the future.

2nd. When the wearing of an artificial eye is impracticable, and cannot be made practicable by the arts of surgery, by even any one firm band whose anterior end is at or near the edge of either eyelid. A full explanation, again, of the nature of the case to the patient is here also necessary; or, at some time hereafter, though the mind of the operator is free of any blame, yet, if he adopt the operation I now propose, the patient may falsely say *he* made it impossible that an artificial eye should be worn.

3rd. When the bands are many or large, or the contraction of the conjunctival sac is such, or the lids so considerably though incompletely bound together, or so everted, that neither the surgeon nor patient can have any hope of an artificial eye being worn.

At present the surgeon in view of such a case thinks he has no more he can do, and, indeed, he thinks the same in cases I have classed under the first and second heads. He dismisses the cases of all three classes from his mind as of those with which he has no longer any active practical concern. But I believe he may yet relieve the patient of much annoyance and of some of the deformity, by finally curing all discharges by altogether permanently closing the eyelids. At present the patients of the three classes above are condemned for the future to go about with a small patch tied over the empty orbit, obliquely over the forehead and round the head. We constantly meet with them in the streets, and, if they are begging, they expose the hideous gap, bleared and running with matter by lodgment of foreign bodies in the sac, by the effect of changes of temperature, &c., on the sensitive mucous membrane, and, above all, by the irritation probably of the row or rows of eyelashes fallen in upon and brushing the mucous membrane, unsupported by an artificial eye. It is an improvement *in appearance*, as well as a comfort to the patient; in any case, if an artificial eye is not or cannot be worn, whether the palpebral aperture and conjunctival sac be full-sized or almost closed by cicatricial contraction. It is a disgrace to surgery not to close up the ugly chasm, and it is easily done.

I remember some time ago a man at University College Hospital for whom it was necessary that I should recommend a lost eye to be removed; hesitating and procrastinating, as patients in such cases, and not suffering much pain, will naturally do, finally consenting, and stipulating that the eyelids should be closed up over the wound, for he would not be troubled to wear an artificial eye. At this hospital there is a fund out of which any after-surgical appliance necessary for the good result of any operation can, on the certificate of the surgeon, be supplied. At the Moorfields Ophthalmic Hospital it is an old regulation that every patient for whom an eye has been removed, be supplied by the hospital authorities with a first artificial eye, and this regulation is well known to the patients and is constantly quoted by us in our endeavors to overcome the natural objections of patients to the removal of a lost eye; so that at this hospital our patients the sooner determine to have the necessary operation done, and accept, as a matter of course, the gift of the hospital. They think little of the saving of money that will be required of them to replace the artificial eye, which is, perhaps, soon broken, or is, in a year and a half or two years, worn out, as a remote event, and numbers of them come back to us sooner or later in one or the other case, lamenting their inability to procure for themselves the missing artificial organ. Then the money required at once is not forthcoming, and, in the absence of any competition in London in the making of the eyes to be worn, the expense is the more considerable. They wear the inevitable ugly patch in the empty orbit, or a handkerchief is tied over it, and, if they cannot afford an artificial eye, and it cannot be afforded for them, how much better off they would be with their two eyelids finally and totally closed together. I have lately at Moorfields had a patient who had had an eye either carelessly excised or previously injured in such a manner as to have ultimately necessitated its excision, and a hard, firm, broad band of cicatrisation as a consequence extended back from a part of the margin of the lower lid into the deeper parts of the orbit. It was impossible to remedy this state of things and to enable the patient to wear a glass eye. He was much disappointed in this, but, as it could not be, he assented to my proposal that the eyelids should be entirely closed up. Under anæsthetic influence I incised the conjunctiva and dissected off great part of it, beginning at the bottom of the irregular contracted sulcus between the lids. The bleeding of course was excessive, and not all of the conjunctiva was removed. When healing of the wound thus caused had taken place, of course the lids were almost closed, but little deep pits and irregular spaces between the lids remained here and there, where some conjunctiva had been left. I then finally destroyed all this remaining conjunctiva by use of the actual cautery, the iron, red hot, thrust to the bottom of the deep hollows, and drawn horizontally along the parts between the lids yet lined with conjunctiva, so as to draw them closely together in the next process of cicatrisation. It was perfectly successful, and the patient can have no more unpleasant conjunctival discharges from useless mucous membrane, and his deformity is much ameliorated, as much as it is possible.

But it is so difficult to dissect off the delicate conjunctiva from the irregular surface and deep-seated space in which it lies, when the eye has been removed at a previous operation, and the bleeding meanwhile is so great as to obscure very much the operation, that I would suggest it be destroyed by smearing over the conjunctiva, carefully preventing its transgressing the margins of the lids, a thin paste of chloride of zinc, to be left there long enough to destroy the conjunctiva, and then to be all thoroughly washed away, and the opposite surfaces allowed to heal. If any small spaces are then left between the lids of remaining conjunctiva they should be got rid of and closed up by use of a hot iron, and left to unite.

It will be so difficult to strip off or dissect away the whole conjunctival surface, leaving the subjunctival tissues, at the first operation, when an excision is done, that it will probably be found most convenient to excise the eye, as it has been done of late years, preserving the conjunctiva and allowing the deep wound to heal; when, if the patient prove to be indifferent to the appearance of having a second eye, or unwilling or unable to submit to the expense or the trouble of the substitute, at a second sitting the conjunctiva may be then, with the assent of the patient, by a secondary operation totally destroyed by producing a slough of all the mucous surface, and consequently a final closing of the eyelids.

Besides these cases, it is, I believe, a surgical duty in all cases of incurable contracted cicatrices within the lids, or of thickened everted lids, that, when the eye has been removed, the useless conjunctiva be not left to produce offensive discharges, and to disfigure the patient more than he need be disfigured, and that a similar operation with chloride-of-zinc paste supplemented with the hot iron be adopted, or that some other means to the same end be employed.

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A CASE OF PARAPLEGIA LASTING FIVE YEARS; ELECTRICAL ANÆSTHESIA.

QUESTION OF MALINGERING.

By W. Moxon, M.D.,

Assistant-Physician to Guy's Hospital.

A lad aged eighteen was admitted into Guy's Hospital (Astley Cooper ward), under my care. He had been sent up from Northamptom with the following history: He had fallen from a hay-loft nearly six years before admission, and in consequence of the fall was laid up with weakness of his lower extremities and of his back; he said that his water had been drawn off on account of his inability to pass it. The history which the lad gave was tolerably consistent. He said he had fallen from a height upon his back, and that paralysis of the bladder and lower extremities was the result of the fall, all of which was very likely.

Whenever a paralytic case comes under care one gets an advantage by inducing the person to put out all the effort he possibly can, urging and compelling him to do so. In this way one learns much of the nature of the case by the degree of willingness with which effort is put out, the freedom of the effort in the parts actually moved, as well as, of course, by the kind and degree of the imperfection in the usefulness of the paralysed part.

On making the boy get out of bed, he flopped on the floor, propping himself on his arms and resting on the side of his buttock, with his legs flexed at all the joints so as to be drawn up rather tightly. The legs were remarkably blue and cold, especially below the knees. The suspicious points about the lad's case were—first, the total paralysis he claimed to have; and, second, the defined line around his knees at and below which he declared the sensation to be lost. Under chloroform it was found that his legs, which he said had hurt him to straighten, would stretch to their length quite naturally; and when cautioned to be very careful, at the same time that he was forced to get upright—being held up in the air, in fact, by the arms,—his legs made very promising movements.

Testing the exact line limiting his anæsthesia in the way we employed led to still more doubt. We marked the professed line with ink, then made him shut his eyes, and tried his sensibility to touch above and below it. With his eyes shut he could not keep to his line as he had done most accurately when he was looking at it. It was remarkable, though, how near he kept to it.

The most interesting results were obtained from the electrical excitation of the paralysed parts. The lad was a miserable coward, and got into insufferable terror at the approach of the electrical battery. We did not calm his fears, but gave substance to them by applying the interrupted current so strong that I could scarcely bear it myself, which I endeavored to do for the sake of appearances. He fairly bellowed when the poles were applied to his arm, the stimulated muscles of which contracted very actively, but I was much surprised to find that when this strong current was applied to his legs below the knees, he gave not the slightest sign of feeling it, and said that he scarce felt it at all, while the muscles contracted so feebly that their reaction was often doubtful, and never more than very trifling. The same current applied in the same way to the legs of a man in the next bed, a courageous fellow who willingly underwent the experiment, made him hiss inwards through his teeth, and no effort of will on his part could prevent the muscles from strongly contracting, though he made a very energetic face in the endeavor to resist contraction, whereas the boy's countenance was perfectly placid while the electricity was applied to his legs. The boy had been a fortnight in the hospital before I made this examination by electricity, and had already made considerable progress; first he had stood up holding on to the back of a chair, then pushed the chair before him, and at last lifted the heavy chair, supporting it instead of having it to support him. As all this mending had occurred in a fortnight, and especially as I could not but think that the power required to lift a heavy chair, in the awkward stooping posture the boy always assumed, was far greater than compatible with his claim to be paralysed, I had come to regard him as a shammer. These results of electrical examination, however, made me at the time doubt whether it could be all sham, and led me to expect that perhaps a check in the progress of the case might be anticipated. I therefore adopted a more active treatment than that hitherto used, which had consisted of camphor-water and driving behind the chair. The fresh means consisted in the use of silver, administered, not internally, but in the form of sixpences, and accompanied with promises of more. Under all these therapeutical means his progress was wonderful. He left go the chair-back and got about on a broom-handle, then learnt to carry the broom. Five weeks after Christmas he ran a very good race with another boy in the hospital-park and won the shilling. He is now apprenticed to a tailor in the city of London, and described by his friends as doing well.

This issue of the case I suppose will be taken to show the boy's condition throughout was no other than deliberate deception. I can't help holding that opinion. Under these circumstances the state described in regard of electricity becomes of great interest.

Can we suppose that the prolonged disuse of the limbs had so brought down the sensitiveness of his legs and weakened their muscles that the usual painful contraction was partly not set up and partly not felt. It is difficult to suppose this, seeing that the power of the will found its ways into his legs so soon and thoroughly as the story shows. Nevertheless, I cannot suggest any more likely explanation of the difficulty, and we must remember that although recovery was quick it was gradual. I should, however, have said that in the latter few weeks the electrical current was used to the boy's legs; but the recovery was by that time complete. I ordered the electricity only for its moral effect; yet some may be disposed to think that that helped the cure.

But as my belief is that the lad was malingering, I think the chief interest of his case lies in its showing that want of absence of reaction to the electrical current does not prove a paralysis to be real; while the whole history shows the power over such cases which one may exercise by firm and persevering pressure upon the patient's will, keeping up a sufficient friendliness in the meantime.

Finsbury-circus, June, 1872.

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PURULENT PERICARDITIS.

A CASE IN WHICH THE PERICARDIUM WAS FILLED WITH PUS.

By James Grey Clover, M.D.

The following case is one of considerable rarity, and perhaps worthy of being recorded.

On the 12th of April I was called to see a little girl, Grace W——, aged twelve, who had complained occasionally, but not severely, of pain in her left side for two or three months, had been ill for two or three weeks, and under the care of a homœopathic practitioner. I was told that her illness began with a slight shivering. There was no rigor throughout. She was pale and emaciated, and had a higher temperature than normal; on the second day of my attendance it was 101°. The most noticeable complaint she made was that of excessive tenderness when touched anywhere, but especially about the chest. This made physical examination impossible. Her respiration was rather quick, and her nostrils were dilated more or less on breathing. She generally sat up in bed, with her shoulders much rounded. I could not make out the indications of tubercular disease, which I suspected, either in the lungs or the abdomen. The first day or two of my attendance there was sickness, vomiting of almost every thing taken, and a tendency to diarrhœa. The upper part of the abdomen was swollen and tender, and hard as far as could be ascertained. She became gradually weaker. She was clear and intelligent, but was occasionally excited in a semi-hysterical sort of way, and on the occasion of my visit the day before her death, went for a few minutes into a very curious trance-like condition, on coming out of which she sang in an Ophelia-like fashion. This nervous element in the case was very noticeable throughout, but I confess to being more impressed with the emaciation, the high temperature, the short breathing, &c. Though I had to content myself with a somewhat vague diagnosis, I had no difficulty in giving a very serious prognosis, which was verified by death in seven days. It is remarkable that two days before her death she managed to walk, after a fashion, a few feet from the sofa to her bed.

The treatment consisted in attempts to introduce nourishment, and, in the way of medicine, the administration of a drop or two of tincture of opium every three or four hours, which soothed and had a beneficial effect on the vomiting and diarrhœa.

At the post-mortem the above facts received the following pathological explanation:—*Thorax:* On percussing the chest and abdomen prior to opening them, extensive cardiac dulness, and dulness and hardness of the hepatic and epigastric region, were marked. On opening the chest the pericardium was found very much distended, as if with a large quantity of fluid. This fluid turned out to be very thick greenish-yellow pus—what the old pathologists would have called laudable pus,—which welled up out of a little opening made, till occasionally the opening became plugged with lymph. I collected a tumblerful of it, and much more remained. There must have been at least two tumblersful altogether. Much purulent lymph then appeared adhering to the inner surface of the pericardium. The substance of the heart seemed pale and soft. The endocardium and the valves were unaffected. The lungs were conjested and adherent to the side, but did not contain tubercle. There was a quantity of serum in the pleura, but not pus,—*Abdomen:* The liver was enlarged very much and universally congested. There was a good deal of straw-colored fluid in the abdomen; but no other morbid appearance was noted.

Remarks.—I never saw pus in the pericardium before, and from what I can gather from books and physicians and pathologists of authority, the case is one of rare occurrence. Clinically, it is curious to notice that such a quantity of pus can accumulate in the pericardium without more distress of the circulation, without more acute local symptoms, and without the coexistence of acute rheumatism.

Dr. Wilks kindly sends me the following comments on my written account of the case, which will be read with interest:—

"Your case is an interesting one; I have seen two or three of the kind, and in none was the pathology very clear—i.e., there was not the history, as one might have supposed, of rheumatic pericarditis, but the disease was insidious and undiagnosed. We have amongst our drawings in the museum an enormously distended pericardium of a case under the late Dr. Hughes. He thought the physical signs caused thereby indicated pleuritic effusion. The same thing occurred under Dr. Addison.

"In the Pathological Transactions for 1869 is a case of mine where a lad died with pericardial effusion. This was evidently the result of an acute affection, although the history extended over some months. His symptoms were due to impairment of the heart's action, and such as are met with in ordinary cardiac disease.

"I suppose the question is—whether such insidious and generally undiagnosed effusions are of a peculiar kind and have a pathology of their own, or whether they are simply the exceptional and accidental consequence of an ordinary pericarditis from some constitutional peculiarity on the part of the patient, just as a pleurisy will now and then end in an empyema, or a peritonitis in a purulent effusion in the abdomen. I apprehend that the chances are in favor of this explanation. Paracentesis of the pericardium has often been proposed, and even practised; but unfortunately, in the three cases which I have seen where it would have been feasible the diagnosis was not made."

Compton-terrace, N., June, 1872.

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ON THE SPIRAL SPRING EXTENDER;

BEING A NEW AND SIMPLE APPARATUS FOR MAKING EXTENSION IN DISEASED JOINTS, ETC.

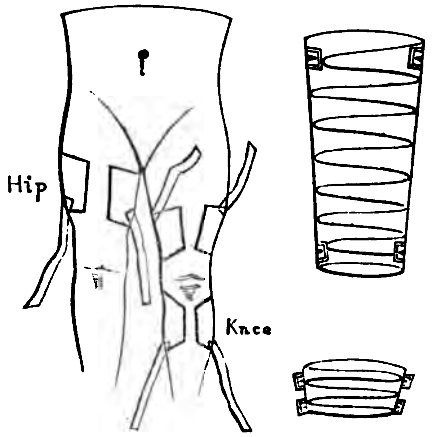
By C. Holthouse, F.R.C.S.,

Surgeon to the Westminster Hospital, etc.

I think it desirable to call the attention of the profession to a simple and inexpensive apparatus for making and keeping up any amount of traction on a limb or joint, and without necessarily confining the patient to bed or preventing him from taking exercise. The principle is old, and is illustrated in the Jack-in-the-box of our infant toys, in the spring seats of our chairs and couches, and in numerous weighing machines, &c. Although I designed this in 1868, I did not actually apply it to patients till the present year, being stimulated thereto by the success of the more costly but not more efficient instrument of Mr. Sayre of New York.

My apparatus consists of a simple spiral spring, covered with leather, flannel, or linen, and having two buckles at one or both extremities. When extended, it forms a cylinder closed at the sides but open at each end; and when the coils of the spring are pressed together it approaches in form to a discoid ring or a closed concertina. It need not, however, necessarily be cylindrical, but larger at one end than the other, in conformity with the tapering shape of the limb. It can be made of any length, size, or strength; and the traction-force exerted can be regulated with the utmost exactness; while its cost need not exceed five shillings.

Application of the instrument.—This is extremely simple. If the hip be the joint which it is wished to extend, two square pieces of strong diachylon plaster having a piece of boot-strapping, from six to eight inches in length, fixed to the centre of the lower border of each, should be applied to the upper half of the thigh, one on each side. The plasters should be sufficiently large to surround, or nearly so, the circumference of the limb, and should be applied evenly by their whole surface; for further security they may be surrounded by a roller bandage applied in the ordinary manner. Over the limb thus prepared, the cylinder, previously compressed, is slipped, the foot, of course, being extended as in pulling on a boot, and it is drawn up the limb till its larger end is supported by the perineum; this serves as its fulcrum. The tapes on the plaster being now buckled to the lower end of the cylinder, it will be obvious that any amount of traction on the hip-joint may be obtained, according as it is buckled tightly or loosely; the first will, of course, approximate the rings of the cylinder, and so increase the traction of the spring; while, in the latter, the rings being further apart, less force is necessarily brought to bear on the joint. The exact amount of traction-force may always be known by trials made with the cylinder previous to its application, and for convenience the tapes may be divided into inches. If the knee be the joint affected, two buckles must be fixed on the upper end of the cylinder as well as the lower, and the tapes which are attached to the plasters on the thigh must correspond with their upper instead of their lower borders. The plaster must, moreover, encircle the lower instead of the upper half of the thigh. On the leg the tapes must hang downwards, and the plaster be applied just below the knee (see figures). In some cases it may be advisable to substitute splints for plaster, in which case the two ends of the cylinder map be supported by buttons or hooks on the outer surface of the splints.



Although the spiral spring extender may be most frequently called into requisition in cases of joint disease, it may be employed with advantage in many fractures and dislocations, as well as in excision of joints, when it is desirable to keep the bones asunder. In amputations, especially of the thigh, it is pecularly eligible where the flaps have been made too short, or have become so by sloughing, the pulling forward of the integuments by this instrument taking off all tension from the sutures, much in the same way as Hainsby's instrument does in cases of hare-lip.

To sum up. The chief merits of this instrument, and its superiority over the weight and pulley, rack and pinion, and other mechanical contrivances for making extension, consist in its great simplicity, cheapness, lightness, and efficiency; the patients being in many cases able to take out-door exercise, and thus avoid the injurious effects of long confinement in bed.

The above have been made for me by Messrs. Mayer and Meltzer, of Great Portland-street.

May 23rd, 1872.

REMARKS ON HYDRATE OF CHLORAL; WITH CASES.

By Richard Wood, M.R.C.S.

At this time, when the properties of hydrate of chloral are being investigated and discussed, I think it must be interesting to give and receive all information that can be collected on the matter, more particularly when doses so small as forty grains will cause death in some cases, while doses as large as sixty and eighty grains may be given in others with impunity. We should endeavor, by one means or another, to tell beforehand what constitution will bear its administration and what will not. For chloroform, we have been taught to stethoscope the heart, and, if disease be detected, to be very guarded how we give it. May not the same caution be necessary before prescribing hydrate of chloral? In many cases where I have given it the patients have appeared confused and unable to answer questions intelligibly, even where doses of ten grains only have been taken. I have also observed great depression in the system, with feebleness of pulse. The valuable properties of the drug, however, are unmistakable, as the following cases will show.

Case 1.—On Feb. 5th, 1872, Mrs. G——, aged sixty-one years, presented all the symptoms of a severe attack of asthma, for which I prescribed an antispasmodic mixture of lobelia and ether; giving her at the same time a small bottle of drops, each dose containing one-eighth of a grain of morphia, to be taken when the paroxysms came on. Upon visiting her on the following day she immediately declared she could not take any more of the drops, as they had produced great oppression in the chest and much increased her difficulty of breathing, and she had been obliged to sit up in bed supported by pillows, fearing that every gasp for breath would be her last. I was perplexed what to do, when chloral occurred to me. On the following night, when the usual paroxysms threatened to come on, she took a draught containing one drachm of Ferris's syrup with five minims of tincture of belladonna. To her surprise, after waiting some time in full expectation of having to undergo the same struggle for life as she had on the previous night, she fell into a quiet comforting sleep, which lasted for two hours, at the expiration of which she repeated the dose, and slept again till morning—a thing she had not done for several weeks. This course of treatment was persevered in, gradually diminishing the dose, until, after the expiration of three or four weeks, she lost every sign of the disease.

Case 2.—On March 29th, 1871, being called to see Mr. W——, aged fifty-nine, I found him suffering acutely from gout in the stomach, with constant hiccough and vomiting, and excruciating pain over the epigastric region. He was ordered half a grain of morphia and a drachm of compound spirit of ammonia in an ounce of water, to be repeated if necessary; also a pill containing half a grain of the acetic extract of colchicum, to be taken every six hours. Upon my revisiting him in a short time he had vomited both the draught and the pill, which being repeated he brought up again. He was now ordered ice and effervescing draughts with hydrocyanic acid. This somewhat relieved him; but in the night I was again called to him, and I hypodermically injected one-third of a grain of morphia, which greatly lessened his sufferings. The vomiting, however, continued off and on until he was compelled to entirely cease taking anything by the mouth. He had, in consequence, beef-tea enemata injected into the bowel every fourth hour, to sustain his fast-failing vital powers, with occasionally the addition of half a drachm of tincture of opium. This plan was continued with success for a week or two, when at last even the opium ceased to give the relief it had at first afforded, and his nights were passed without sleep. He was now ordered two scruples of the chloral hydrate, to be given in the beef-tea enema at night. It acted like magic, and he obtained a quiet and painless night. This relief was felt throughout the rest of the illness, which terminated in the course of five or six weeks by a complete recovery.

Case 3.—On April 5th, 1872, Mrs. N——, aged seventy, called me in. She was suffering from acute bronchitis. Ordered a mixture of carbonate of ammonia; also a small bottle of drops, containing for each dose one-twelfth of a grain of morphia, to be taken occasionally at night to ease the cough. On the following morning she complained that expectoration was more difficult than it had been, although she had had more sleep. She also felt great discomfort from an eruption having appeared on the body, more particularly on the outside of the thighs and back, resembling urticaria, which caused intolerable itching. Not feeling certain that this was the result of the anodyne, I ordered her to continue it. The next day, the irritation being no better, I determined to change the morphia for chloral, of which I gave her a two-ounce mixture, containing six drachms of Ferris's syrup—two teaspoonfuls to be taken occasionally when the cough became troublesome. I was much pleased on my subsequent visit to hear her express herself considerably relieved and refreshed by a good night, the rash and itching having almost disappeared, and which were entirely gone in two or three days. She had completely recovered in the course of a fortnight.

Bromsgrove, May, 1872.

REPORT OF

A CASE OF PHTHISIS PULMONALIS,

ACCOMPANIED BY ULCERATION OF THE LARYNX AND PLASTIC EXUDATION INTO THE TRACHEA AND BRONCHI.

By G. H. Philipson, M.A., M.D. Cantab., M.R.C.P.L.,

Physician to the Newcastle-Upon-Tyne Infirmary, etc.

John S——, aged thirty, a Norwegian, was admitted into the Newcastle Infirmary, under my care, on the 18th of January, 1872. For nine months he had been employed as a fitter at one of the engineering works on the Tyne. For six months he had suffered from cough, shortness of breath, occasional pain in the chest, slight hæmoptysis, loss of flesh, and gradual diminution of strength.

At the time of his admission he was much emaciated, his voice was husky, his cough was almost incessant, and he was expectorating large quantities of nearly pure pus. His pulse was feeble (120), and his respirations were short and hurried (33). The chest anteriorly was flat, and was almost stationary, even in forced respiration. There was marked dulness upon percussion in both infra-clavicular regions, and in the left upper scapular region. The irritability of the muscles under percussion was well marked. The respiration was harsh, bronchial, and was accompanied with bubbling rhonchus, especially in the left infra-clavicular region. Upon examination with the laryngoscope, the mucous membrane of the larynx and chordæ vocales was seen to be ulcerated. A strong solution of nitrate of silver was applied to the larynx with the curved brush, and sedatives, nourishing food, and wine were administered.

Jan. 27th.—The respiration in the left infra-clavicular region was hollow, cavernous, and was accompanied with gurgling rhonchus. The voice was muffled, and at times was changed into a hoarse croaking whisper.

Feb. 10th.—After some hours of urgent dyspnœa, and during a violent paroxysm of coughing, the patient expectorated a piece of false membrane, about four inches in length, tubular at one extremity, and distinctly transversely marked and indented. From its size it was surmised that it had been dislodged from the lower portion of the trachea, or from one of the main bronchi.

On the evening of the 12th he died from exhaustion, but without having ejected any further mass.

Autopsy, fifty-seven hours after death.—The lungs were both adherent to the chest wall, the left more firmly than the right, especially near the apex. The upper lobe of the left lung was excavated with caverns, anfractuous, about the size of large nuts, communicating one with another, and with indurated lung between them. The upper half of the lower lobe was studded with yellow and grey tubercle, the yellow being in excess. The lower half of the lower lobe was darkly congested. The main bronchus and bronchial tubes were lined with false membrane, which at places was loose and easily raised. The pleural covering of the upper lobe was greatly thickened; that of the lower lobe was spotted with dark pigment. The upper lobe of the right lung was similar in appearance to that of the left, with the exception of the cavities being smaller, many not being larger than peas. The lower lobe was dotted with yellow and grey tubercle, the grey preponderating over the yellow. The main bronchus and bronchial tubes were lined with false membrane. The trachea was lined throughout with false membrane, which was separable in layers and easily raised from the mucous surface, which was tumid and of a dull-red color. The false membrane very closely resembled moistened parchment, and when examined under the microscope appeared to be made up of altered epithelial cells. The cavity of the larynx was covered with pus. The mucous surface presented an irregular-shaped loss of substance, with serrated ragged edges. The chordæ vocales could not be recognised. The heart, liver, and kidneys were healthy. The bronchial and mesenteric glands were not enlarged nor tuberculated.

The particular interest of this case is, the association of tubercle in the lungs and of tubercular laryngeal ulceration with the formation of false membrane in the air-passages, the ulcerative action being limited to the larynx, and the membranous croupous or aphthous inflammation to the trachea and bronchial ramifications, large and small; the two processes—that of the morbid condition of the blood giving rise to the deposition of tubercle and to the membranous inflammation affecting the same continuous membrane—being in close juxtaposition, and being apparently dissimilar, yet resembling each other in the lowness of organisation of the products. Possibly, the exudation upon the mucous membrane of the respiratory tract was only an extreme condition of that form of aphthous or diphtheritic stomatitis which is not uncommon towards the close in cases of chronic phthisis pulmonalis. The condition of the lungs very typically exemplified the anatomical characters of tubercle, and the changes which the tubercles undergo—namely, that tubercle in its early stage is grey and afterwards becomes yellow, and that yellow tubercle softens and finally gives rise to excavation.

Newcastle-on-Tyne, May, 1872.

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THE TREATMENT OF SMALL-POX BY VACCINATION.

By William Marshall, M.D.

Many times during the last five or six years I have had thoughts of sending a communication to The Lancet, to draw attention to what seemed to me a positive fact—viz., that vaccination was not only preventive but also curative in small-pox. I delayed doing so under the idea that if this were really true, what with the amount of vaccinating and revaccinating that was going on in the country, the same thing must have been observed by many others, and surely would have been published in some of the medical journals; and now that Mr. Furley has drawn attention to it, I feel it my duty to write in corroboration.

In estimating the value of vaccination as a cure for small-pox, it is necessary to be acquainted with the course of that disease in all its varieties, and to bear in mind that its incubative stage is thirteen days; and that vaccination, as far as my observation goes, is a local disease until some time between the eighth and eleventh days; it is only then that the constitution becomes affected, and it is so from absorption of the lymph in the matured vesicles. I come to this conclusion from observing that a person exposed to the infection of small-pox within eight days after being vaccinated may take the disease; but no matter how much exposed after the eleventh day, he will not take it. Also, if a person who has been vaccinated be revaccinated within seven or eight days, the revaccination will take in some modified way; but if done after the eleventh day, not at all. Hence, if an unvaccinated person who has been exposed to the infection of small-pox be vaccinated in time for the vesicles to rise, and the lymph to be absorbed before the symptoms of small-pox begin, he will either have no attack, or, what I think is more common, will have severe symptoms, ending with the appearance of a few spots, sometimes of none. But if the symptoms of small-pox commence before the vesicles have risen and begun to desiccate, he will have a severe attack, but modified in so far that there will be no secondary fever.

I shall not occupy space further than in giving the following history.

In a large family an unvaccinated child had small-pox and died. I only saw it when dying. I recommended all the others to be vaccinated. Four had been previously vaccinated; these were revaccinated; none of them took the disease. Three of the sons had never been vaccinated; one of them I vaccinated directly and satisfactorily, but just as the vesicles were desiccating, he showed symptoms of small-pox, and five spots appeared on his face, and a few on his body; he was not confined to bed a single day. The second on the very day he was vaccinated became ill with small-pox, and the third, who had never been vaccinated, was taken ill on the same day. Both of these had the disease severely, but the contrast between the two lying in the same room was most remarkable. In the second son the vaccination and the small-pox progressed, the one entirely unaffected by the other until the tenth day; the vaccine vesicles then desiccated and so did the small-pox ones, and the disease came to an end; he had *no secondary fever*, and was sitting up *quite well*, while his brother—the unvaccinated one—was lying in a high secondary fever, delirious, and battling for his life. These cases occurred six years ago, and this was the first instance I had seen of the curative effects of vaccine. Since then, however, I have seen many, and have always observed the same result, so that I think that I can have made no mistake. I was, therefore much surprised a few weeks since to see in the summary of the trial of some of the "Peculiar People" for the manslaughter of two children who had died of small-pox without medical aid, &c., that a third, who had been vaccinated, had small-pox directly after and died of it. I should be very much pleased if the doctor who attended the case would publish details of it, especially with regard to the date of the onset of the symptoms of small-pox after the vaccination, the date of the death, and the cause of it.

The great objection to the use of vaccination in the cure of small-pox is the necessity, in my experience, of its being done within a day or two of the onset of the disease, otherwise the vesicles have not time to rise and affect the system in a curative manner; but Mr. Furley's plan of injecting a large quantity of lymph under the skin seems to be exaçtly what is necessary, and may be done with hopes of immediate benefit at any stage of the disease.

Mortlake, S.W., May, 1872.

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A CASE OF MALIGNANT SMALL-POX.

By A. D. Ducat, M.D.

The following case may not be uninteresting, illustrating as it does the rapidly fatal and plague-like form sometimes assumed by small-pox, and the necessity of revaccinating before the age of twelve (the age at which successful revaccination is paid for under the regulations of the Lords of Her Majesty's Council), when the primary vaccination has not left behind a sufficient number of large and clearly defined cicatrices.

G. H——, a moderately healthy boy, aged twelve years and eight months, and residing at Highbury, was seized on Saturday, May 18th, with headache and sickness. As he had vomited his tea, and kept vomiting the cold water he very eagerly drank, his mother at 9 P.M. got a dose of magnesia and rhubarb for him from a chemist, which he also vomited. He was very restless through the night, and kept groaning and saying he had pains in his head, back, and legs. He seemed so ill that medical relief was sought early on Sunday morning from a neighboring dispensary, and some medicine obtained which the patient was able to keep on his stomach. Throughout Sunday he ate nothing save a little jam-tart, which, along with two cups of tea, he was able to retain. In the afternoon he got up and dressed himself, but was only able to stay up for an hour. He passed a slightly better night, and on Monday (Whit-Monday) morning his mother, thinking he was merely suffering from a bilious attack, left him at six o'clock to go down some distance into the country. The patient was thus left alone three or four hours, when the father coming in to look after him, found him covered with an eruption. About noon he asked me to see his child, which I did shortly after. The patient (who had not been seen by any medical man till then) complained of violent headache and backache. The sickness had somewhat abated, and he drank eagerly of cold water. His pulse was 130, and of fair strength; the bowels not confined. There was a little blood about the mouth, and the tongue was furred. He was quite conscious, and readily did what he was told. On examining the body hundreds of dark purpuric spots, perfectly distinct from one another, and varying in size from the diameter of a small pin's head to that of a large pea, were seen scattered here and there over trunk and limbs. There was not the slightest trace of the appearance of the ordinary characteristic eruption of small-pox. Occasionally one meets, in the more violent forms of small-pox, with a deep-red eruption (like that seen in severe scarlatina), covering portions of the body, and especially the abdomen; at other times in milder cases you have an eruption like measles, but here there was nothing beyond the purpuric spots I have mentioned, which were not raised above the level of the skin. I ascertained that the patient had been vaccinated when a baby, but the one or two marks which had been left were scarcely discernible. I ordered milk, strong beef-tea, and a mixture containing ammonia, spirit of chloroform, and compound spirit of lavender to be frequently administered, but gave no hope of the child's recovery. He died in less than twelve hours after, retaining his consciousness and power of speech up to the very last.

That this was a case of small-pox of intensely malignant type there can be no doubt. There have been some very severe cases of the disease in this part of my district, and this very lad had been playing about a house where a man lay severely ill with it. His fate may well serve as an additional warning for us not to neglect *the duty of early and thorough revaccination*, especially when we reflect how impossible it is in all cases securely to guard against the chances of infection.

Highbury, May 27th, 1872.

A Mirror

OF

HOSPITAL PRACTICE,

BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Procemium.

UNIVERSITY COLLEGE HOSPITAL.

INTESTINAL OBSTRUCTION; CAUSE OBSCURE; DEATH; OBTURATOR HERNIA DISCOVERED.

(Under the care of Dr. Roberts and Mr. Erichsen.)

Our readers will peruse with interest the following account. Cases of obturator hernia are rarely met with. The diagnosis is exceedingly difficult—by some considered impossible; and its occurrence has very rarely been determined during life. In most instances, as in the present, it has only been made out at the autopsy, the patient dying with symptoms of internal strangulation. We are indebted to Mr. W. Greenfield, physician's assistant, for the subjoined notes of the case.

S. H——, male, aged fifty, was admitted on April 10th, 1872, said to be suffering from intestinal obstruction. He is a tall, thin, spare man, much emaciated, and completely bald. Is married, and has several children. Has been a worker in brass. Habits regular. Has always been subject to cold and cough, and for the last ten years has had occasional hæmoptysis. Lately he has been losing flesh, and suffering from constant cough and loss of appetite. Had occasional severe diarrhœa some years ago, but of late the bowels have been regular. Has not been subject to pain in the abdomen. He states that he was quite well until Sunday last (April 7th). Had taken a light supper on Saturday night; and had his usual plain breakfast and dinner on Sunday, containing nothing indigestible. At about 4 P.M., as he was moving about the room, he felt a sudden sense of sinking at the epigastrium, followed by pain about the region of the umbilicus. He felt sick and faint, and sat down, but did not vomit. The pain rapidly grew worse, shooting down from the point of its commencement to the left groin and inner side of the left thigh. It did not affect the testicle, nor extend beyond the knee. He was obliged to flex the thigh, on account of the severity of the pain. The patient says he had not made any effort or strain before the pain came on. His daughter says he had just had a violent fit of coughing. Supposing the pain to be due to flatulence, he took some salts and castor oil; the latter he vomited at once, but repeated the dose in the evening, when the bowels were slightly opened. Had some difficulty in passing urine for a few hours. On April 8th the pain increased, but was more general in character. He had a small dark-colored motion of the bowels; but towards the afternoon vomiting came on, which in the course of a few hours became stercoraceous, and has continued almost incessantly till the present time. There has been no motion of bowels since the 8th. No diminution in the amount of urine has been noticed.

State on admission.—11 P.M.: Patient is extremely emaciated, almost a skeleton. He has an aged, anxious expression, but there is no appearance of acute suffering. Is very pale; eyes prominent and glistening; surface and extremities cold. He is quite sensible and intelligent, and answers questions readily. The breath has an unbearable sickly sweetish odor; tongue slightly furred, but moist; vomits frequently acid stercoraceous fluid; pulse 120, of good volume. The abdomen is slightly prominent, rather more so on the right side than left; veins on surface very distinct; abdominal walls thin and lax, stretched over distended coils of intestine, which can be distinguished through them. This is most marked on the left side, where the walls feel more tense. There is no marked general tenderness, but deep pressure causes pain, especially in the epigastric and right lumbar and iliac regions. He complains of a dull aching spontaneous pain in the same regions. There is no tumour or hardness to be felt anywhere in the abdomen, though, perhaps, rather more resistance over region of cæcum and ascending colon, but percussion note is perfectly clear everywhere. There is no drawing up of legs. On examination of the orifices the external abdominal rings are very large, and the finger can be passed in some distance; there is no fulness whatever in the canal. There is perhaps slightly greater fulness in the upper part of left thigh than right, but doubtful. No tenderness and no fulness can be felt in either of the regions for hernia. Digital examination of the rectum affords no information.

Was ordered half a grain of opium every four hours, and teaspoonfuls of iced beef-tea and brandy occasionally.

April 11th.—He has passed a restless night, without sleep, vomiting constantly; looks more prostrate; pulse softer, occasionally intermittent. Was seen by Dr. Roberts at 11 A.M., who ordered large enemata twice daily, and ice to the abdomen. Small nutrient enemata to be given, and other treatment to be continued. At 1 P.M. about two pints of arrowroot were injected and kept in for nearly half an hour. When passed it contained two or three small lumps of ordinary fæces. There was much pain at the time, but the vomiting ceased for four hours. At 6 P.M. it returned, and he was ordered half a grain of opium and one-third of a grain of the extract of belladonna every three hours. At 10 P.M. another large enema was given, more than three pints of arrowroot being injected gradually with a long tube. The abdomen became much distended, and patient complained of great pain and tenderness just above the umbilicus; the injection was therefore discontinued. He has complained to-day of some pain in the left lumbar region.

12th.—At about 8 A.M. the patient became altered in appearance; he looks even more emaciated; a slight flush on cheeks this morning; hands and feet cold, clammy, and purplish; body warm and moist; temperature normal. Was drowsy during night; but vomited constantly, and did not sleep. Has been straining at stool, but no motion. Passes water easily, and in good quantity (exact quantity unknown, some being passed with enemas). The abdomen is less tense, and outline of intestines less marked. Slight pain on pressure, but not otherwise. Has now frequent hiccough. At 2 P.M. he was seen by Mr. Erichsen, who explored the various positions for herniæ, inguinal, femoral, and obturator, and could find no evidence of the existence of one; nor any hardness or tumour in any part of the abdomen. Mr. Erichsen remarked that the symptoms pointed rather to some contraction or compression of the bowel than to acute strangulation or intussusception, as it was now the sixth day since the obstruction occurred, and there was not more prostration than would be accounted for by the vomiting, and no evidence of peritonitis; and intussusception would have been almost certainly accompanied by the passage of bloody mucus or the discovery of intestine per anum. There was no sufficiently definite symptom to warrant interference by operation, and he therefore advised large enemata of gruel with olive oil, and the internal administration of opium or belladonna.—10 P.M.: Patient seems much weaker; very drowsy, but conscious when spoken to; pupils normal; skin moist; temperature 98·4° Fahr.; hands cold; pulse 100, small, and rather weak; respirations 36 per minute. He is vomiting very frequently, but the odor is rather less offensive. Returns nutrient enemata immediately; no tenderness of abdomen.

13th.—2 A.M.: He is not vomiting so frequently, but strains often at stool. Hands and feet very cold; pulse small and intermittent; temperature in axilla 97·6°. He appears now much more collapsed; is very drowsy; wanders a little; no tenderness of abdomen; passes water well.—10 A.M.: Became speedily more restless and delirious, got out of bed several times, gradually sank, and died at about 10 A.M. The temperature in the rectum immediately after death was 103·3° Fahr.

Autopsy, four hours after death.—Weather warm; rigor mortis strong; great emaciation of the whole body. Abdomen not much distended; everywhere resonant; walls very thin; no subcutaneous fat. Upon opening the abdomen, the transverse colon was seen forming a sharp flexure in the left hypochondriac region; the great omentum drawn up to the left side; the remaining area below was occupied by three transverse portions of greatly distended small intestine, which were much congested, and presented fine capillary injection where in contact, but there was no lymph on the surface. On raising the lower piece of intestine, the bladder was seen rather distended, and to the left side, occupying the recto-vesical pouch, was a great portion of the small intestines, which were pale, very small, and firmly contracted. These appeared adherent at one point to the pelvic wall, opposite the left obturator foramen, the portion above being greatly distended and injected, that below pale and contracted. After dissecting from the front of the thigh, and cutting through and raising the pectineous muscle, some fatty and areolar tissue was removed from over the upper part of the thyroid foramen, and a small hernial sac was then seen occupying its upper part, but not projecting beyond the pubic bone. The portion of the sac seen was oval, measuring ¾ in. by ½ in., the longest diameter transverse; immediately below and to the outer side lay the obturator nerve and vessels. There was no fulness or swelling whatever over the region of the sac, and the muscles were firm and well developed. Only one side of the intestine was engaged in the sac, just sufficient to close the passage through the bowel, the other side being squeezed and bloodless. The part in the sac was rather deeply congested, but not at all gangrenous; there was no fluid in the sac. The portion of the small intestines above the hernia measured 7 ft. from the duodenum; they were greatly distended, injected, and almost empty; below firmly contracted and pale. Large intestine moderately distended, healthy-looking. Rectum firmly contracted. The mesenteric glands corresponding to upper part of intestine swollen and soft. The hernial sac was one inch and a quarter long; its orifice lay beneath and behind the left lateral ligament of the bladder, from this point passing at first directly forwards along the side of the bladder, and then a little inwards along the course of the nerve. It just admitted the tip of the little finger at aperture, and then contracted to the size of a quill, expanding again somewhat towards the end. The intestine was but loosely held, and was accidentally drawn out during the autopsy, scarcely any force being used.

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ST. BARTHOLOMEW'S HOSPITAL.

CASES OF STRANGULATED HERNIA.

(Under the care of Mr. Savory.)

For the notes of the following cases we are indebted to Mr. G. C. Parnell, house-surgeon.

1. M. S——, aged fifty-two, was admitted into the hospital on Dec. 13th, 1871. He has had a reducible inguinal hernia for the last twelve months, and has always worn a truss. Two days before admission he left off wearing his truss, on account of some tenderness about the seat of rupture; consequently the hernia came down, and symptoms of strangulation set in a few hours before admission. He was admitted with an oblique inguinal hernia of the right side, accompanied by well-marked symptoms of strangulation. Taxis was applied, the hernia reduced, and the patient taken into the ward.

Dec. 14th.—Little sleep during the night; much troubled with hiccough; vomited three times. Ordered milk, beef-tea, four ounces of brandy, linseed-meal poultice to the abdomen, and an enema of castor oil.

15th.—Bowels freely open; hiccough ceased; rupture allowed to come down, and immediately reduced. Temperature 99°; pulse fair volume.

16th.—Some good sleep after hypodermic injection of half a grain of morphia; sickness less violent. Ordered twelve ounces of brandy, enema of castor oil, ice, eggs, and five grains of calomel, which was repeated in the evening. Bowels not open.

17th.—Vomiting continued with marked violence through the night. Vomited matter slightly stercoraceous. Temperature 97·5°; pulse weaker; bowels not open. A consultation was held, and it was thought advisable to explore the seat of rupture.—Operation at 7.15 P.M.: Mr. Savory cut in the direction of the right inguinal canal, and opened the sac, which contained neither intestine nor omentum; but attached to the inner side of the canal, high up, was a firm band of fibres, which, being traced into the abdomen, was found to constrict a portion of the intestine. This was divided, and the wound closed.

16th.—Died, exhausted, twenty hours after the operation.

At a post-mortem examination the peritoneum was found in a state of diffuse but subacute inflammation, having a focus close to the internal abdominal ring on the right side; at this point the intestines were loosely adherent by a layer of recently effused lymph. The great omentum was healthy, and occupied its natural position. In the inguinal canal a firm, dense, fibrous band was found to have been divided close to the internal abdominal ring; the other end could not be found. It was suggested by Mr. Savory, Mr. Langton, and others, that it might have been attached to the appendix vermiformis. The intestine was found to be perfectly free, and the seat of stricture was three feet above the ileo-cæcal valve. The gut at this point was congested and inflamed, but no destructive changes had taken place either externally or about its mucous layer. The other organs were found to be natural.

2. T. O——, aged forty, was admitted into the hospital on Jan. 11th, 1872, with an indirect inguinal hernia of the right side. In March, 1871, he was operated upon by Mr. Savory for a strangulated inguinal hernia of the same side, which was returned after opening the sac. He made a good recovery. On the day of his admission, the hernia came down whilst he was in bed, during a fit of coughing; and, being unable to return his rupture, he came to the hospital two hours after. The taxis was tried in the surgery and afterwards in a hot bath without success. Two hours after admission he was placed on the operating table, and Mr. Savory cut in a line with the former incision. The tissues were very œdematous. The sac was opened, and a band of omentum was found strangulating the intestine. The omentum and intestine (large) were somewhat congested, and the former was adherent to the sac. The band was divided, and about twelve inches of intestine returned. On examination of the remaining omentum, a hole being found which appeared "as a trap for the intestine to be caught in again," a portion, about the size of the palm of the hand, was therefore removed, a few ligatures applied to the bleeding vessels, and the wound partially closed.

Jan. 12th.—Slept after hypodermic injection of one-sixth of a grain of morphia. Ordered milk and beef-tea. Was sick four times; had abdominal pain. Temperature 101°; pulse 76.

14th.—Sickness ceased; pain less. Temperature 101; bowels open after an injection.

16th.—Bowels open; temperature 100·6°; pulse 72.

18th.—Morphia injections discontinued.

20th.—Had a chop and a little porter. Wound nearly healed.

Feb. 6th.—Left the hospital wearing a truss.

3. A. B——, aged forty-eight, was admitted into the hospital March 6th, 1872. He had had a rupture for the last year, for which he had worn a truss. Whilst in bed, and without his truss, he was seized with a violent fit of coughing, and the rupture came down. A medical man tried taxis under chloroform, but without success, and he then sent him to the hospital.

On his admission, six hours afterwards, he had an inguinal hernia on the right side; the scrotum was very much enlarged, tender, dull to percussion, evidently containing a quantity of fluid, and symptoms of strangulation were well marked. Taxis was tried under chloroform whilst on the operating table for a few minutes, but failing, Mr. Savory operated, and it was found that the seat of stricture did not exist external to the sac. On opening the sac in the usual manner, prior to the introduction of the director, a quantity of dark turbid serum escaped from the small opening made, and the intestine was then returned without further division of the sac. The wound was closed with silver sutures, and a spica bandage put on. He had a grain of solid opium half an hour after the operation, and this was repeated at night. No sickness. Ordered ice to suck.

March 7th.—Ordered milk and beef-tea. Slept fairly: no distress from operation. Temperature 99·5° F.; pulse 72, fair volume. Bowels open freely in the evening.

March 8th.—Slept fairly. Temperature 99·2°; pulse 78. No sickness, but troubled with diarrhœa. Ordered arrow-root and twelve minims of tincture of opium in water, and an injection of acetate of morphia (one-sixth of a grain) at night.

9th.—Slept well. Bowels open; stools solid. Pulse 76; temperature 100°. Appetite good. Has a slight cough; tongue coated with a white fur.

13th.—Cough better. Bowels not having been open since the 9th, an enema of castor oil was ordered, together with a chop and four ounces of wine. Sutures removed, and a bread poultice applied to the wound.

14th.—Ordered full diet.

Since this date he progressed favorably, and was discharged, wearing a truss, on March 30th, 1872.

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ST. THOMAS'S HOSPITAL.

SOFTENING OF THE BRAIN FOLLOWING SYPHILIS; DEATH.

(Under the care of Dr. Bristowe.)

F. W. B——, aged twenty-three, was admitted into hospital April 16th, 1872. He had syphilis in October last, followed by sore-throat and general psoriasis. He is not an intemperate man, and has enjoyed good health till two weeks ago, when he began to suffer from headache, loss of appetite, constant nausea, and was sick after nearly every meal.

He is a strong, well-nourished man. Has still some spots of psoriasis on the face and arms. Can walk, but feels giddy. Complains of pains in the back of the head and down the neck; the pains are worse at night; reading increases the headache. He has continual singing in the ears, chiefly the left. The whole of the scalp is somewhat tender. Pupils contracted, especially the right; they do not dilate to light; a strong light makes his eyes ache. Tongue thickly coated. Has no appetite; has been sick three times this morning. Throat not sore now. No tenderness of abdomen; bowels confined. No particular pain after food; vomiting comes on about ten minutes after he has taken food. Heart and lungs healthy. Urine 1027; no albumen. Temperature 99°.

April 20th.—Complains much of headache, which is worse at nights. Appears to see with both eyes. Says he sees black spots. Sclerotics and conjunctivæ of both eyes congested. Still sick.

25th.—This morning it was noticed that the left side of the body was paralysed, the tongue deviated to the left, and the left side of the face did not move in speaking. No complaint of pain. He cannot move his left leg or arm, but seems to retain perfect sensation in them. Reflex movements easily excited in the left leg. Still complains of headache. Sees well, and eyes move perfectly.

29th.—He is getting drowsy and comatose. Can just reply "yes" or "no" indistinctly. Tongue deviates to the left. Left side of the body quite paralysed. Has no power to move the right eye outwards or inwards; the up and down movements are much impaired. Bowels open after croton oil.

He died comatose on the 3rd of May.

At the post-mortem examination the membranes of the brain were found thickened, and an increase of fluid at the base. There was marked thickening of the right posterior cerebral artery, which was surrounded by reddish fluid evidently the result of inflammatory action the interior of the artery for one inch of its length was occupied by a cylinder of adherent fibrine. There was softening of the anterior part of the left optic thalamus, extending down into the crus cerebri of that side. Another patch of softening was found in the outer wall of the right lateral ventricle at its posterior part. All the other organs were tolerably healthy.

TUBERCULAR MENINGITIS; DEATH.

(Under the care of Dr. Murchison.)

W. G——, aged twenty-nine, was admitted April 12th, 1872, in an almost unconscious state. He is said to have been "out of sorts" for the last month, and has had much to worry him lately. He had taken very little food for the last week. He has made no complaint of headache. No family history of phthisis. On Sunday afternoon, the 7th, he was found lying on the floor insensible. He appeared to be paralysed; he could not speak, only making noises at intervals. Up to this time he had been going about as usual. He had been sick several times on this day, and had been unable to eat any dinner. He is said to have remained in much the same state of insensibility since Sunday.

On admission at 8.30 P.M., he could move all his limbs, but the left side seemed weaker. He lies in bed moving restlessly, throwing his arms about, and apparently clutching at imaginary objects. He does not answer articulately, but mutters slightly. At times he seems to understand what is said; puts out his tongue when sharply spoken to. Conjunctiva congested; pulse 132; respiration 60; temperature 102·4°. Tongue dry, furred, protruded straight. No pneumonia detected. Right pupil sensitive to light, probably natural; the left, on sudden exposure, first contracts, and then expands and remains dilated. He was ordered a dose of croton oil and chloral.

April 13th. — Temperature 100·8°; pulse 128; bowels freely opened. He was much quieter after the chloral, but did not sleep. Appears to understand better; tongue brown, furred; can move both legs and arms; cannot give the left hand, but grasps fairly with it; pupils small, equal, sensitive; right eye much inflamed; cannot talk; heart-sounds healthy; no headache; evacuations passed under him; hands restless, but not tremulous; there is no strabismus, facial paralysis, or deviation of the tongue; no vomiting; no appearance of discharge from the ears; he is very restless, constantly clutching at the bed clothes. There is marked tenderness on pressure over the upper cervical spines, but none over the head. He died, exhausted, the next day.

Post-mortem examination.—In the upper lobe of the right lung, near the apex, was an indurated fibroid mass the size of a wallnut, with caseous softening in the centre; scattered through the lung a few small irregular masses. A similar mass was found in the upper lobe of the left lung. Bronchial glands crumbling, caseous, almost chalky. The brain weighed 2 lb. 14 oz. The veins on the surface were congested. The ventricles contained much clear serum. Membranes at base much congested. Along the fissure of Sylvius, on both sides, there were distinct tubercular nodules in abundance. In the gray matter adjacent to the right Sylvian fissure was a small abscess the size of a pea. On the under surface of the right anterior lobe, close to the median fissure, was a small extravasation of blood. The other organs were natural.

ST. GEORGE'S HOSPITAL.

THE ACTION OF SULPHATE OF QUININE UPON THE TEMPERATURE IN PULMONARY PHTHISIS.

(Under the care of Dr. Ogle.)

The following observations regarding the action of quinine upon the temperature of the body in pulmonary phthisis were recently made by Dr. Ogle. The patient, a young woman, was admitted into the hospital with symptoms of dyspepsia, attended by pain across the back, great debility, and mental despondency. At this time no febrile condition existed. Subsequently, whilst taking quinine and sulphuric acid, pains about the thorax came on, and also undoubted general and stethoscopic signs of deposit of tubercle. At this time the daily registering of the temperature of the body was undertaken. From April 27th to May 20th the temperature was 100° night and morning, except on three occasions, when it went up to 101° —once in the morning and twice in the evening, at intervals of three or four days.

During the above days—that is, from April 27th to May 20th—the patient was treated as follows: viz., by two-grain doses of quinine and a few drops of sulphuric acid in water every four hours, and by occasional turpentine stupes to the chest, until May 1st; she then took effervescing citrate of ammonia and carbonate of ammonia until the 6th of May; after which she returned to the quinine and acid, and took two drachms of cod-liver oil after dinner. This was continued (a blister having been applied beneath the left clavicle on the 18th) until the evening of the 20th, when she began to take ten-grain doses of quinine in a little dilute sulphuric acid and water three times a day. The temperature was then recorded as follows:—

	A.M.	P.M.		A.M.	P.M.
May 21st ...	100°...	100°	May 23rd ...	99°...	99°
22nd...	99 ...	99	24th ...	99 ...	99

On the 24th the quinine was increased to fifteen-grain doses until the 27th, the temperature during this time being registered as follows:—

	A.M.	P.M.		A.M.	P.M.
May 25th ...	98° ...	99°	May 26th ...	99° ...	99°

At this time the quinine produced such discomfort and pains in the head, with buzzing in the ears and dimness of sight, that the dose was again diminished to ten grains, and continued till May 31st;

and during this time the following temperatures were recorded:—

	A.M.	P.M.
May 27th,	100·4°	100·3°
28th,	100·3	100·2
29th,	100	100·2
May 30th	100°	100°
31st	100	—

At this period, as the uncomfortable feelings about the head had not subsided, Dr. Ogle omitted the quinine for some days altogether; the temperature during this time remaining at 100°. On June 3rd, as all discomfort had abated, Dr. Ogle at once put the patient on twenty-grain doses of quinine night and morning, which doses were continued until June 10th; and the following temperatures were during that period recorded:—

	A.M.	P.M.
June 4th	—	99°
5th	99°	99
6th	99	99
7th	100	99·2
June 8th	100·2°	100°
9th	99	99
10th	100	—

On the 10th the quinine was discontinued, and the patient a few days after, at her own desire, left the hospital, and thus observation of her case was broken off.

With regard to diet, the patient, during the thermometrical observation, was taking meat and potatoes, or fish with eggs and (at her own request) without butter, until June 3rd, when she had fish and beef-tea, occasionally eggs, but no meat. For the most part, three ounces of port wine were given daily.

On consideration of the tables the following points are noticeable:—

1. The high temperature and its uniform range in a case of active phthisis, the temperature during observation prior to the exhibition of the large doses of quinine being never lower than 100° F., and occasionally 101°.

2. The promptitude with which (see date May 22nd), on the exhibition of the ten-grain doses of quinine, the temperature sank to 99°, this range being maintained night and morning.

3. The same degree of temperature only being attained when the quinine was increased from ten- to fifteen-grain doses, except on one occasion (the morning of May 25th), when it fell to 98°.

4. The return of the high range of temperature on the disuse of the quinine, and even of a higher degree, as on three occasions it reached respectively 100·2°, 100·3°, and 100·4°.

5. The subsequent reduction of temperature to 99° on the quinine being given in twenty-grain doses night and morning, with only one or two exceptions, when it attained 100° and a little more.

During these fluctuations no proportionate modifications of respiration, of the pulse, or of the heart's beat, were found to exist.

MAISON MUNICIPAL DE SANTE, PARIS.

(Cases under the care of M. Demarquay.)

In the course of a visit which we paid a few days ago to the surgical wards of the above hospital, M. Demarquay kindly showed us, or related the details of, various cases of interest.

A CASE OF COMPLETE SELF-MUTILATION.

This patient was brought into the wards as we were going round the hospital. He was a man about twenty-eight years of age. The whole of the external genital organs had been cut off two or three hours before by means of a razor, and the wound was as smooth and perfect as if made by the most skilful surgical hand. The man had lost a considerable amount of blood. He was in a condition of semi-delirium due to alcoholic intoxication. To the questions which were addressed to him he would every now and then make some clear, direct answer, but evidently his mind was quite confused through the effects of drunkenness and loss of blood. On being asked as to the cause of his self-mutilation, he made an allusion to some love troubles, but it was found out eventually that the man had been long addicted to drinking, that he had been intoxicated for the preceding three days, and had committed self-mutilation in a fit of alcoholic delirium. A strong dose of opium was administered, and, notwithstanding the considerable loss of blood, some hopes were entertained of saving the patient's life; but he died the following morning. The interest of the case lies in the fact that self-mutilation was complete, the penis and scrotum having been cut off quite clean.

M. Demarquay mentioned that in the course of his surgical practice he had seen several cases of partial self-mutilation, but never a complete one like this.

TWO CASES OF SOFT CHANCRE OF THE FINGERS ACCOMPANIED WITH CHANCROUS BUBOES IN THE EPITROCHLEAR REGION.

The details of these cases were furnished by M. Demarquay, and his house surgeon, M. J. Tirard.

The first case was that of a man aged twenty-two. When first examined by Dr. Demarquay, the following lesions were noted:—On the index finger of one hand a deep, irregular, depressed cicatrix; immediately above the epitrochlea of the same arm a tumour, about the size of a hen's egg, with a soft fluctuating centre and hard œdematous edges. The patient said that the cicatrix on the finger was the result of a whitlow; and then the question arose whether the swelling in the epitrochlea might not be a mere glandular swelling, the result of the whitlow. On viewing, however, the peculiarly depressed and irregular character of the digital cicatrix and the appearance of the finger, M. Demarquay asserted that it must be a chancre. The tumour was opened, and there issued an abundant flow of sanious, non-homogeneous, brownish matter. The patient was then minutely questioned, and related that, in the preceding month of September (this was in February) he had been suffering from soft chancre on the penis; that he used to dress the chancre with aromatic wine; that one day he burnt his finger, did not attend to the burn, and continued to dress the chancre in the same way. The wound resulting from the burn became larger and deeper; but there had never been any pain. It had healed only in January; and then the tumour in the arm first made its appearance. The incision made into the tumour by M. Demarquay was dressed with soft poultices occasionally and aromatic wine. The patient left the wards on March 12th, the wound being then almost entirely cicatrised.

The second case was very much like the preceding one. It had been under M. Demarquay's care two years ago. This patient was a youth who also had chancres on the penis. One day he burnt his finger with some sulphur, and then the same consequences occurred: large and deep sore on the finger, which took a long time to heal; a tumour in the epitrochlea with sanious pus and hardened edges, which resisted for a long time the employment of all therapeutical means.

There is no doubt, said M. Demarquay, that the epitrochleal swellings were true chancrous buboes, due to the absorption of a special kind of pus. Such cases are rare, M. Ricord, in the course of his long special practice, having only observed one similar instance. Buboes in the armpit, through chancres on the fingers, are of more frequent occurrence. In M. Ricord's case the pus taken from the bubo was inoculated with success. M. Ricord had looked up statistical accounts of chancre, and found that the occurrence of hard chancre of the fingers was more frequent than soft chancre. Both, however, were very rare, since out of Ricord's statistical index of 651 cases, only two belong to the fingers. The question naturally suggests itself whether soft chancres of the finger are not sometimes overlooked and mistaken for other lesions—whitlow for instance. In the preceding cases the existence of the epitrochleal bubo was a clue to the discovery of the true character of the digital lesion.

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WORKHOUSE HOSPITAL, MANCHESTER.

EXOPHTHALMIC GOITRE; UNUSUAL SEVERITY OF SYMPTOMS; ULCER OF CORNEA; CURED.

(Under the care of Mr. William A. Patchett.)

Emily W——, aged twenty-seven, servant, unmarried, admitted March 1st, 1871. For the last five or six years she says she has been accustomed to very hard work, involving much loss of rest. Catamenia appeared at fifteen, have generally been scanty and irregular, and for two years have been completely stopped. Five years ago, after a severe day's exertion, she was suddenly seized with violent "throbbing" at the heart, pain, and dizziness in the head, which remained a few minutes and then disappeared. A few weeks afterwards the palpitation returned, and she has been troubled with it ever since—the least over-exertion or any excitement bringing on an attack. Two years ago her friends remarked that her eyes looked prominent, and that she had a very staring appearance. She also noticed that there was a fulness on the right side of the neck. From this time the eyes became more and more prominent, the swelling in the throat larger, and the palpitation more frequent. Two months ago the eyes became so painful and tender that she could not bear the light, and in consequence had to leave her situation.

State on admission.—Is very excited, pale, and emaciated; the eyeballs considerably protruded, the insertion of the recti muscles being visible; conjunctivæ highly injected. There is a small ulcer of right cornea, just as if punched out. Eyes are exceedingly sensitive and painful; cannot bear the light, and as the eyelids do not cover the eyes by ¼ in. she is obliged to wear a covering over them. Both eyeballs are equally prominent. By pressure with fingers on eyelids, which causes very great pain, the protrusion can be much diminished, but it returns on the removal of the pressure. When the eyes are uncovered she has a wild staring look. Heart's impulse is strong and frequent, and diffused over a wide space. The superficial cardiac dulness measures 5 in. across, deep cardiac dulness 6 in. by 4½ in. A loud blowing systolic murmur is heard over the base, also along the carotids, and over the axillary and brachial arteries a bruit is heard. There is a venous hum over the jugulars; pulse 120, weak. The thyroid gland is very prominent, the enlargement being confined to the right lobe; left lobe is not visibly enlarged, but a fulness can be felt over it. Complains of having very restless nights; both legs œdematous; urine free from albumen.

She was placed on generous diet, with two eggs and a pint of beef-tea daily, and ordered three-grain doses of valerinate of iron (in pill) three times a day. The ulcer of cornea to be painted twice a day with a solution of nitrate of silver, four grains to the ounce.

This was a typical case of Graves' disease, the three characteristic symptoms, exophthalmos, goitre, and excited heart, being so well marked. The patient remained in hospital fourteen weeks, and was discharged June 7th, 1871, perfectly cured. For the first two weeks there was very little alteration in the state of the patient. She had frequently most violent attacks of palpitation and breathlessness, and during these attacks the protrusion of the eye-balls was so great that she thought they would fall out of their "sockets." The notes of her daily progress are too lengthy to reproduce, but the intensity of the symptoms gradually abated. At the time of her discharge the thyroid enlargement had entirely disappeared, the condition of the eyes was perfectly normal, the heart's action quiet, and the sounds unassociated with any murmur. What is most extraordinary in this case is, that the cardiac dulness, which on admission measured 5 in. across, on her discharge measured only 3 in. by 2¼ in. The occurrence of ulcer of cornea is also interesting, and must be rare, as Trousseau says he never saw it occur in Graves' disease. This woman is now (April, 1872) quite well and strong.

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BALTIMORE (U.S.A.) EYE AND EAR INFIRMARY.

ACCIDENTAL IRIDEREMIA; OR, COMPLETE REMOVAL OF THE IRIS BY THE FINGER-NAIL OF AN ANTAGONIST.

(Under the care of Dr. Julian J. Chisolm.)

J. M——, aged thirty-seven, presented himself for treatment, suffering from chronic inflammation of the left cornea, with loss of transparency. Attention was attracted by the striking contrast which his two eyes exhibited. The left eye, for which he desired treatment, was of a light-blue color, whilst the entire corneal portion of the right eye was of a black color, with the exception of a white triangular scar, 1½ lines wide at its base, which rested upon the lower edge of the cornea, with apex extending up to the central pupillary region. To a casual observer the right eye presented the appearance of an old and severe injury, with anterior chamber filled with blood blackened by long exposure. Dr. Chisolm, however, was surprised, on throwing the light from an ophthalmoscopic mirror upon this black eye, to find it brighten with the true red reflex of a healthy choroidal fundus. The use of the objective lens indicated transparent media and a normal disc. Upon inquiry this black unpromising eye proved to be his good one, whilst the normal-looking one had had its usefulness much marred by the haziness of its cornea.

He furnished the following history of himself:—Formerly his eyes were both blue and strong, until three years since, when he came out of a fight with one eye black as it appears at present. During the fight his antagonist had gouged him in the right eye. Under excitement he suffered no pain, and was not aware at the time that his eye had been injured. The next day he discovered the change of color of his eye, and detected a shred protruding from a cut in the front of the eyeball. After one or two days this fragment, which was hanging out, came away, leaving a white scar over the sight. No severe inflammation had followed upon the injury, and so little inconvenience was experienced that he did not lose a day's work, nor did he think it neccessary to seek professional advice. A little cold water was the only local application used. Until the left eye became inflamed, one month since, both eyes had been in constant service, and were believed to be equally good.

A careful ophthalmoscopic examination revealed no trace of iris. When the thrust of the finger had driven the nail through the cornea, it had in some inexplicable manner entered the pupillary opening, and, passing between the capsule of the lens and the iris, had thrust this latter membrane against the inner face of the cornea. Then, acting as a hook, the nail had drawn the iris out of the wound, separating this membrane from the ciliary ligament, the surface of adhesion being weaker than the cohesion of iritic structure. The final escape of the protruding iris had been effected by the continued drawing action of the ever-winking lids. The finger-nail had in no way injured the lens, nor the accomodation of the eye. Although the corneal scar contained between two descending radii involved about one-tenth of the surface of the cornea, light could freely pass around this obstacle and make an image upon the fovea centralis retinæ. With Jaeger's test-types he could read No. 1 at four inches, and No. 20 at eight feet. With the optometer and a 10-inch convex lens, his range of accomodation for brilliant type was *pr* 1-5½, *pp* ½.

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WEST LONDON HOSPITAL.

RETENTION OF URINE FROM STRICTURE; EXTRAVASATION; RECOVERY.

(Under the care of Mr. TEEVAN.)

R. R——, aged twenty-nine, a groom, was admitted into the hospital on April 18th, 1872, with extravasation of urine. Two years ago the patient contracted his first and only attack of gonorrhœa, for which he had no treatment whatever. In four months it began to decline, and was succeeded by a gleet which lasted for many months. One year ago he began to suffer much pain in urinating, and noticed that the stream of urine became very fine and twisted. His health now declined, and he lost flesh. Three weeks ago the perineum commenced to swell, with much pain, attended with shivering fits. Two days before admission the penis became very œdematous.

When admitted, the face was ghastly, cheeks hollow, and eyes sunk. Patient tremulous, and sweating profusely; pulse 150, and very feeble. Hypogastric region full, and dull on percussion. Penis semi-erect from œdema; perineum greatly swollen, indurated, and of a dark-purplish color, limited behind by a line drawn from one tuberosity of the ischium to the other, but extending forwards on to the abdomen.

Mr. Teevan made an incision three inches long in the median line of the perineum, and from the wound there escaped some urine, followed by pure pus and some gangrenous shreds of tissue. The penis was also punctured in several places.

On the 4th of April urine passed freely through the wound, and some came by the penis. The cellulitis had extended upwards on each side to the crest of the ilium. On the next day Mr. Bridgman made free incisions into the penis and skin in each flank. He found that the temperature, which had been gradually increasing since the patient's admission, had to-day reached 104°. On the 8th, two large collections of pus in each flank were opened, as also an abscess in the abdominal wall. From this date the patient began to improve, the temperature descending till it reached 98·8° on the 16th. On April 27th Mr. Bridgman began to dilate the stricture with the smallest French bougies; and by May 18th he had got up to No. 14, when the patient left the hospital quite well, a little urine still coming through the wound for one week afterwards.

Mr. Teevan observed that the case illustrated the course and sequelæ of a gonorrhœa unchecked by any treatment whatsoever. When the patient was admitted, one of the finest filiform bougies was with ease passed into the bladder, after a vain attempt to introduce the smallest metallic catheter, to demonstrate the fact that the former would often succeed where the latter failed through its greater calibre. Now, inasmuch as the final cause of retention from stricture was the blocking up of the passage by a plug of mucus, it followed that cases could be successfully treated by the simple introduction of a bougie where the smallest catheter had failed to effect the desired object. Finally, it was only in cases similar to the present, where the urine was pent up and unable to escape, that it could do any harm. Infiltration of urine after lithotomy was a physical impossibility.

Editorial.

THE DIAGNOSIS OF SYPHILIS.

For some time past a sharp controversy has been sustained in Vienna upon the subject of the diagnosis of syphilis by the microscopical examination of the blood. Dr. Lostorfer now furnishes to Stricker's *Medizinische Jahrbucher* the facts which have led him to the position he has now taken up on the affirmative side of the question. He was supplied, it appears, by Stricker, who appends a note to the paper, with a number of test specimens; and arrived, on the whole, at very correct conclusions. A drop of blood was taken from the skin, and as quickly as possible placed upon a clean object-glass, and covered. Twelve glasses, with drops belonging both to syphilitic and non-syphilitic patients, and properly numbered for subsequent identification, were then placed in a moist chamber, and were examined from day to day with a Hartnack's immersion No. 10, ocular No. 3. During the first day or two, vibrios, bacteria, and the early stages of development of sarcina, were commonly met with. On the third, or still more frequently on the fourth or fifth day and occasionally even after twenty-four hours, small bright corpuscles made their appearance, which were partly at rest and partly in vibratory motion; some of them exhibited a small process. By the sixth day these corpuscles had increased in size and number. Many of these larger ones exhibited the above-mentioned process; and this then distinctly appeared as a bud. In a few it was so large that it was very little less than the parent cell. During the subsequent days the corpuscles continuously augmented in size; so that a few soon attained the magnitude of shrunken red corpuscles, or even exceeded them. At the same time smaller ones of all sizes were present. Gemmation was frequently perceived, many corpuscles presenting, not one bud only, but several, which were in some instances pedunculated, and in others directly applied to the parent cells. Cases also occurred where the secondary buds appeared upon the surface of the primary ones. Most of these gemmæ were of irregular form, but some were spherical. About the eighth or tenth day a vacuole formed within the larger corpuscles, which gradually increased in size till ultimately its wall appeared to consist only of a thin lamina, presenting a double contour under the microscope. At this period the corpuscles had attained their final development, and underwent no further changes even when the test could be preserved for a month, which of course was only of exceptional occurrence. Dr. Lostorfer also examined the characters of the corpuscles with different fluids, as with solution of sugar, distilled water, and Pasteur's fluid—namely, one-half per cent. of solution of common salt and one per cent. of solution of acetic acid. These fluids, notwithstanding their diversity, produced similar effects during the earlier period of the development of the corpuscles, causing them to shrivel, and preventing any further growth. Even at the sixth day some shrivelling occurred on the addition of these fluids and dilute solutions of sugar; but when distilled water was added at this period the vacuolæ enlarged rapidly. Gemmation took place in the form of long tubules, which closely resembled the germ tubes of fungi. The number of the corpuscles varied to a very great extent. In one instance he counted fifty of them in the field of the microscope at one time on the fourth day; but he is unable to say whether any proportion exists between their number and the severity of the symptoms of syphilis present. Dr. Lostorfer states that long before the discovery of these corpuscles he had carefully examined the blood of healthy persons without finding anything similar to them; and that during the last three months he has systematically subjected healthy blood specimens to the same treatment, as well as blood taken from those suffering from gonorrhœa, diphtheritic ulcers, lupus, and from typhus patients, without in any instance observing their development. He therefore thinks he is justified in terming them syphilis-corpuscles. He is of opinion that temperature is a matter of great importance affecting their development; for when the temperature fell below a certain point, not yet exactly ascertained, the results were negative. He gives the appearances in thirteen cases, which show that the corpuscles are not associated with any particular form or stage of syphilis, but manifest themselves both before and after the occurrence of the eruptions and in tertiary affections. He guards himself from saying they in any way constitute the carriers of the infection; and, as regards their origin, it is uncertain whether they are introduced with the syphilitic poison, or whether the blood is so altered in syphilis as to permit their growth when derived from other sources. In the note appended by Stricker to Lostorfer's paper, he says that he tested Dr. Lostorfer's ability to diagnose syphilitic blood under the microscope on six occasions. On the first he sent him twelve proof specimens, of which Nos. 8 and 9 were taken from healthy persons, and the remainder from three different patients suffering from syphilis. In a few days Lostorfer replied: "Nos. 8 and 9 non-syphilitic; two preparations spoilt; the remainder syphilitic." In the second list there were seven numbered proofs, and in the third nine proofs, and in each of these the syphilitic cases were correctly diagnosed. In the fourth list, however, no results were obtained; but this Lostorfer explained by stating that all the specimens had accidentally been exposed to a very low temperature (— 12° C.). The remaining tests, including one by Professor Hebra, seem to have been very fairly made out. Whatever may be the ultimate conclusion arrived at, enough has certainly been ascertained to lead to attention being paid to the subject; and, as the field of observation is unfortunately only too common, it is probable that positive results will soon be obtained. High powers, immersion lenses, and some experience with the moist chamber, are indispensable for pursuing the inquiry.

ATHEROMA OF ARTERIES.

Our knowledge of inflammatory processes in arterial tissues and their relation to atheroma and aneurism is of the most obscure and unsatisfactory kind. English pathologists, with very rare exceptions, regard the atheroma of arteries as chronic in its progress and degenerative in its character. It is generally attributed to some defective or impaired nutrition of the arterial tissues associated with age or with some abnormal state of the blood —from syphilis, alcohol, &c. On the other hand, the German pathologists, with a few writers in this country, consider that the atheromatous process in arteries is the result of some form of inflammation, commonly chronic in character. Dr. Moxon has lately called the attention of the profession to the subject in a very suggestive and able article in the sixteenth volume of Guy's Hospital Reports, "On the Nature of Atheroma in the Arteries." Dr. Moxon believes the continental view, which regards this process as truly of an inflammatory kind, to be the correct one. He thinks that an explanation of the misapprehension that prevails on the subject may be found in some such reasons as the following:—The superficial fatty degeneration of the arterial lining membrane, which is unconnected with any other changes in the remaining coats, is easily regarded as of the same nature as that which is more deep-seated and accompanied by such changes. It is forgotten, too, that fatty degeneration is, in some tissue-elements, a normal result of inflammation. The changes which always accompany the fatty deposits are also frequently overlooked or ignored; and, lastly, there are the theoretic considerations as to the non-occurrence of inflammation in non-vascular structures. The failure, says Dr. Moxon, to recognise the identity of the causative process of vivid and of smouldering inflammation has been a main cause of our hitherto overlooking the inflammatory nature of what is roughly called atheroma in this country. Changes of a kind that must be regarded as inflammatory are present in nearly all bad specimens of atheroma. In young subjects such inflammatory changes occur often without atheroma; and it is these that tend to produce aneurisms more than do the atheromatous patches. "The disease that leads to aneurism is the same disease as leads to atheroma"; they are, in fact, alternative results of this disease of the coats. If the sub-inflammation be acute, the arterial wall may rapidly soften and rupture, and either sudden death or a false aneurism be the result; if less active, the coats soften and yield before the chronic process that produces the atheromatous patch has had time to occur; and if still less acute, we get the products of a fatty, calcareous, or ossific degeneration superadded, as it were, to those which can be readily ascribed to inflammatory changes, such as thickening of the arterial coats.

The author follows up his subject by showing the connexion between inflammation of the arterial tunics, atheroma, and aneurism; and he dwells upon, and accounts for, the relatively greater frequency of the latter affection among soldiers. In fine, Dr. Moxon holds (1) that what is called atheroma of arteries is sub-inflammation of various degrees, of which the lower degrees end in fatty degeneration of the coats, along with the inflammatory products; and (2) that the determining cause of this change is mechanical strain, a general altered nutrition—such as obtains in gout, syphilis, &c.—being regarded in the light of a predisposing cause.

Medical Annotations.

"Ne quid nimis."

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At a meeting of this Society on the 27th May, Dr. P. W. Latham read a paper "On some of the Symptoms produced by Uræmic Poisoning in Chronic Disease of the Kidney." The object of this paper was to show that many of the symptoms as to the mode of production of which in chronic Bright's disease much discussion has hitherto arisen might reasonably be explained; that the factors involved were (1) the impeded passage of the blood through the minute arteries of the system, caused by excessive contraction and hypertrophy of the muscular walls of these vessels, as has been demonstrated by Dr. George Johnson; (2) the hypertrophy of the heart developed by the resistance offered to the circulation from the contraction of these small arteries; (3) the impoverished state of the blood, which is the necessary accompaniment of the disease.

The author first dwelt upon the occurrence of paroxysmal dyspnœa or asthma, and, after discussing the effects which would be produced if the minute branches of the pulmonary artery were suddenly contracted, and by what general symptoms and physical signs such an event would be accompanied, he showed by reference to cases recorded by other observers, and from instances which had come under his own observation, that this theory as to the production of the dyspnœa was supported by facts. In the next place he referred to epileptiform convulsions and uræmic coma, and pointed out why in some cases convulsions might occur and not in others, owing to the predominance of one or other of the factors above mentioned. He then went on to say that, although cerebral apoplexy not infrequently occurred where there was atheromatous degeneration of the arteries, yet that, independently of this, apoplexy might be caused by the velocity of the blood through the minute tubes being retarded by their contraction (the velocity through a tube varying as the square of the radius of the section) to such an extent as to allow of the formation of a small coagulum of fibrin or a thrombosis; there would then be complete obstruction, and consequently the greatest possible pressure would be brought to bear on the arterial wall, and result not improbably in rupture. This also, he contended, explained the production of pulmonary apoplexy, and minute apoplexies in the kidneys and spleen and hæmorrhagic infarctions occurring in chronic Bright's disease where no valvular mischief of the heart or endocardiac disease existed.

SUGAR IN NORMAL URINE.

Brucke was the first to state that the healthy urine of man contained sugar, since, on boiling with alkali, it became of a deep-yellow color, and reduced small quantities of the oxides of bismuth or copper. Bence Jones repeated Brücke's experiments; and, by another method enabling him to obtain carbonic acid and alcohol as the result of its fermentation, convinced himself of the presence of sugar in normal urine. Kühne estimated the quantity at 0·1 per cent. These statements are contested by Prof. J. Seegen in Pflüger's *Archiv* (Band V., Heft viii.) Starting from the observation that all the symptoms of diabetes are not unfrequently present when the percentage amount of sugar does not exceed that given by Kühne, he thought it highly improbable that such a discharge should be of normal occurrence; and he was induced to examine the whole subject methodically. He took large quantities of urine, and repeated the various methods for the detection of sugar given by Brücke and others; but with only negative or very unsatisfactory results, which were chiefly owing to the fact that all the sugar-tests at present known are not sufficiently precise. He therefore maintains that the excretion of sugar by the urine is not a physiological function of the kidneys, and that in the normal state none is present. He further observes that the persistent excretion of even very small quantities of sugar in the urine is accompanied by the general symptoms of diabetes.

FIREPROOF STARCH.

From the scion of royalty to the seamstress or the ballet-girl, more cases of sudden and violent death have been due to the combustion of wearing apparel than to any other cause. Now it is the Archduchess Mathilde of Austria—now the accomplished wife of the poet Longfellow—who falls a victim to her dress catching fire; far oftener, however, it is the poor *danseuse* or *cantatrice* before the footlights, or the still poorer heroine of the "Song of the Shirt." At the International Exhibition at South Kensington there is exhibited a starch which ought to remove all such calamities in future from the category of accident to that of design. Improving on the old and very partially adopted tungstate of soda, Mr. Donald Nicoll has produced a preparation which renders the finest cambrics and muslins, the material of window-blinds, mosquito-curtains—the finer fabrics of every kind, in short—not only uninflammable, but well-nigh incombustible. Henceforth we are entitled to expect that the verdict, hitherto so frequent at coroners' inquests, "Burnt to death by her clothes accidently taking fire," will reach its mimimum; and that the "curse of Prometheus" will, in great measure, be neutralised by this latest addition to the armory of preventive medicine.

THE ACTION OF QUININE ON THE COLORLESS BLOOD-CORPUSCLES.

The June number of the *Practitioner* contains an important paper on this subject, by Dr. Geltowsky, which derives additional interest from being the result of work done at the Brown Institution, and the first actual contribution to science thence proceeding. The author confirms the statements of previous observers that the addition of a minute quantity of sulphate or hydrochlorate of quinine to drawn blood arrests the movements of the colorless corpuscles, either instantaneously or after a time, according to the quantity used; and he establishes that the action is due to the salt of quinine itself, not to its solvents; and that it is not shared by methyl sulphate of strychnia, urea, or chloride of sodium. Proceeding further from these data, he estimated the quantity of quinine that would be required to affect the whole mass of the blood in a given animal, and injected this quantity into a vein. In every instance either narcotism or death was produced; and the corpuscles, notwithstanding, remained wholly unaffected. The original paper contains full details of the various experiments, and is well worthy of careful study. We trust it may be the forerunner of many others of equal merit.

Correspondence.

"Audi alteram partem."

PREVENTIVE AND CURATIVE TREATMENT OF VARIOLA.

To the Editor of The Lancet.

Sir,—Your excellent periodical has given publicity to the ravages of small-pox in England. Experience has taught me that these ravages might be considerably diminished by mercurial treatment; and I beg you will draw the attention of your numerous readers to this exceptionally valuable remedial agent, either as preventing variola or curing it.

Preventive Treatment.—During an epidemic of small-pox, doses of two grains of calomel (half for children) protects almost certainly. When the disease exceptionally breaks out, it is of a mild and discrete character, even when the patients had not been vaccinated. Four or five doses of calomel are generally sufficient to shield from the disease.

Since I used this treatment, which I thought I was the first to employ, I consulted many books, and found that the medical men of the eighteenth century had, like myself, observed the prophylactic action of mercury in variola. This certainly takes some credit from myself, but confirms my views.

[Here the writer of this letter quotes from Grassius, Ephem. Naturæ Curios., A. 72, obs. 57; Roussell, Researches on Small-pox, 1781; Dezoteux, Traité de l'Inoculation, an. 8, p. 159; Black's Letter on Inoculation, p. 19, 1771; Desessartz, Mém. de l'Inst., vol. ii., p. 229; Van Voensel, St. Petersburg, &c., in support of the preventive treatment.]

Curative Treatment.—During the incubation of variola, and even during the eruptive period, mercury should be used both internally and externally. By the mouth it may be given as stated above; but the doses should be repeated every twenty-four hours only. The forehead and face should also be rubbed with the strong mercurial ointment for five minutes at a time. From three to five frictions should be used, in proportion to the more or less advanced stage of the incubation. Salivation need not be dreaded; I have observed it only once. It would seem as if the variolous ferment were contrary to the advent of ptyalism.

When frictions have been used during the period of incubation, the eruption sometimes does not appear at all. When the breaking-out does occur, it presents only a few papules, which soon disappear.

When the rubbing is employed within the first two days of the eruption, the development of the pustules seems to be arrested; they become gradually smaller, and finally dwindle away. There is either slight desquamation or none at all. This retrocession of the pustules occurs even when suppuration has reached its greatest activity (viz., from the third to the sixth day of the eruption), but in such a case the retrocession takes place somewhat later, and desquamation is more marked.

In any case the face presents no swelling, the eyelids are unimpeded in their functions, and the nose remains free for the purpose of respiration. This retrograde course in the formation of pus is followed by two consequences of vast importance to the patients.

1. It prevents pitting on the forehead and face, which pitting almost always takes place in confluent small-pox, especially where no vaccination has been performed.

2. It forestalls congestion about the cerebrum, depending on the swelling of the face and parts underneath. This congestion is very dangerous, and has been declared as such by Sydenham.*

I was somewhat nervous at the beginning of my experiments, when favoring the arrest of the pustules of variola; but the absence of any unpleasant symptom soon dispelled the fears I might have experienced respecting the retrocession of the purulent matter.

The medical men of the eighteenth century had here also forestalled me, just as in the prophylactic treatment; whilst I, at one time, thought I had opened a new path. All their efforts were directed to diminish the number of pustules, and even to prevent suppuration altogether.

[The author here quotes Van Swieten, vol. v., paragraph 1388, p. 52; and also paragraph 1393, p. 62; Thomas Fowler, De Methodo Medendi Variolam, præcipue auxilio mercurii, 1778; Ballonii op. omnia Med., Genev., 1762; Lettsom's Med. Mem., sec. 5; Tomlinson's Comment., p. 9.]

I could easily multiply quotations which would show how highly valuable many authors considered the mercurial treatment of variola.

I remain, yours, &c.,
Gariel, M.D. of Paris.
Paris, May, 1872. Prizeman of the "Institut."

ON THE MEMBRANA DECIDUA IN TUBAL GESTATION.

To the Editor of The Lancet.

Sir,—On June 28th, 1872, Dr. Paul Munde, Master in Obstetrics of the University of Vienna, late assistant to Professor v. Scanzoni at Wurtsburg, Bavaria, U.S. America, called upon me, and requested to know whether in any of my papers on the membrana decidua which surrounds the ovum in cases of tubal gestation I had described the decidua reflexa. My impression was that I had done so; but I requested Dr. Munde not to state this as a fact until I had leisure to refresh my memory by referring to the Medico-Chirurgical Transactions, vol. xli., in which one of the papers was published. At page 145 is the following description of an ovum in which both decidua vera and reflexa and a decidual cavity were observed:—

"Case 6.—Mr. Jackson, of Wimpole-street, presented to me in 1856 the uterus and appendages of a woman who had died suddenly from internal uterine hæmorrhage at at early period of pregnancy. The preparation of the parts is now placed on the table of the Society. It will be seen that the uterus is enlarged, and the whole lining membrane is coated with a thick, irregular layer of a substance resembling the fibrine of the blood, of a red color in the upper part. This substance has been partially detached from the lining membrane of the uterus, which presents a natural appearance. When the preparation came into my possession the right Fallopian tube, about the middle, was as large as a walnut, or larger where its coats had burst, and a coagulum of blood was hanging through the irregular aperture. The tube was pervious from the corpus fimbriatum to the dilated part. On cutting open this expanded portion, a small embryo, enclosed in the amnion, was observed, and the vesicula umbilicalis, remarkably large, with its peduncle, came into view. All the cells of the placenta, and all villi of the chorion, were seen distended with coagulated blood, and surrounded with deciduous membrane, a great part of which had been separated from the inner surface of the tube. By a careful dissection subsequently made, the decidua was found to consist of placental decidua, decidua vera and reflexa, with a decidual cavity. The ovum in this case of tubal gestation was, therefore, perfect in all its structures, and similar in every respect to ova which had reached the cavity of the uterus, and been developed there until the end of the second month."

I am, Sir, your obedient servant,
Robert Lee.
Savile-row, June 28th, 1872.

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* Morbi periculum a pustalarum frequentia et numero in facie sola æstimandum. Sydenham, Processus, &c., vol. i., p. 503.

MECHANICAL RESTRAINT IN CASES OF INSANITY.

To the Editor of The Lancet.

Sir,—My own experience is decidedly in favor of the employment of restraint in suitable cases, which I admit are not frequent; but, when necessary, it is often the most humane as well as the most scientific means at our disposal of preventing patients inflicting injuries upon themselves and others. Doubtless there are other plans of treatment which ought first to be tried, and which generally succeed; but I do not myself look upon restraint as many do, as the opprobrium of alienists, or as a *dernier ressort*, to be held in abhorrence. For eighteen years I have seen it employed in certain cases with the best results; the last one was that of a man, a few years since, who attempted to tear out his tongue in a fit of excitement, and in whom restraint succeeded when full opiates and other means had failed. In fact, in such cases as this, it is far better, far more humane and scientific, to adopt the proper form of restraint at once, than to waste time and strength and patience in fruitless attempts to pour medicines down the throat, and in various devices intended to defeat the object of the patient. I may add that I have never experienced the least opposition or objection on the part of the Commissioners in Lunacy to the adoption of restraint whenever the nature of the case and the reasons for the treatment were fully explained to them.

I am, Sir, yours obediently,
William C. Hills, M.D.,
Medical Superintendent.
Norfolk County Asylum, May 31st, 1872.

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CANCER.

To the Editor of The Lancet.

Sir,—While search is being made for a *cure* for cancer, it will not be uninteresting to many of your readers to hear of a *palliative.* I have used as a lotion, with great palliative success, in a case of cancer of the breast, and in another case of cancer of the rectum, an American drug, "hydrastis Canadensis"; it was preferred by both patients to any other form of lotion, for its painlessness and for its power in keeping the surface in a more healthy condition and free from offensive odor. The strength in which it was used was one drachm of tincture to eight ounces of water. Finding it unknown to many practitioners, I have thought the mention of it in the pages of The Lancet may perhaps bring an improved temporary relief within the reach of some sufferers from this dire calamity.

I remain, Sir, yours truly,
Edwin Payne, M.D., M.R.C.P. Lond.
Selhurst-road, South Norwood, June, 1872.

M. Laboulbene, of Paris, has been enabled by means of an aspiring pump, to investigate the character of the liquid contained in the knee of a patient suffering from blennorrhagic arthritis. The liquid was withdrawn *in vacuo*, and therefore quite pure and undisturbed. It presented a deep yellow tint, and contained numerous purulent white globules. It was twice inoculated in the eye of a rabbit without result.

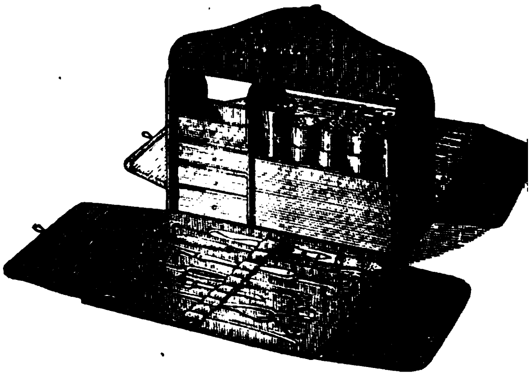
New Inventions

IN AID OF THE

PRACTICE OF MEDICINE AND SURGERY.

NEW GYNÆCOLOGICAL BAG.

Gynæcologists cannot be too grateful to the Messrs. Arnold and Son, of West Smithfield, for this neat and compact bag. It includes everything they are likely to want, and is calculated to save them much time and trouble by supplying them with a place for everything and everything in its place. The comfort and success of many uterine operations will be greatly furthered by the use of this bag. It is light, portable, compact, and strong, resembling, when closed, a neat secretary's writing-case. The framework is of strong material, covered externally with morocco leather, and is divided into an upright central compartment, with nests or divisions for specula and bottles, and a nest of four mahogany drawers, one fitted with four small bottles, and the remainder for pessaries, stems, &c. When unfastened the two sides of the bag fall apart, exhibiting at one view all the instruments therein contained, held in their respective places by bands of leather, from which they can be easily and speedily withdrawn. The cost of the Gynæcological Bag complete, with instruments, is twenty guineas; without instruments, three guineas.



Foreign Gleanings.

PERUVIAN BARK *versus* ERGOT OF RYE.

Dr. Angelo Monteverdi has published a book of 279 pages on this subject, and shows by facts that the former is superior to the latter, besides being devoid of any noxious effects. The cases and quotations are numerous, and embrace abortion, metrorrhagia, menorrhagia, amenorrhœa, puerperal fever, hysteria (in which the bark is noxious), affections of the intestinal canal, of the genito-urinary organs, &c. &c. We subjoin a few of the points which the author considers he has proved:—1. The action of the bark and its derivatives is principally exerted on the sympathetic nerve. 2. This action extends over the organs depending on the nerve, as the uterus, the bladder, intestinal canal, and bloodvessels. 3. The action is physiological and excitant in small doses, and pathological in large doses. 4. The bark acts powerfully on the gravid uterus, and should, in deficient pains, be preferred to the ergot. 5. To be used also in retention of the placenta, rather than have recourse to manual interference. 6. Especially in the same placental retention connected with premature birth. 7. The bark is to be employed instead of the ergot in hæmorrhage either during gestation or labour. 8. In amenorrhœa it stimulates the inertia of the uterus. 9. Puerperal fever is simply due to metro-paralysis; the bark in small doses will cure it. 10. The bark is contra-indicated in hysteria and hysteralgia. 11. It is useful in affections of the digestive apparatus, and the genito-urinary system in cases of atony. 12. As far as the bloodvessels are concerned, small doses accelerate circulation, and large ones determine tension of the vessels, which tension is followed by slow pulse. 13. Opium and morphia moderate the action of the bark.

The book will be read with much interest by all members of the profession.

FOREIGN BODIES IN THE EAR.

Dr. Löwenberg has published, in the *Berl. Woch.* (No. 9, 1872) three very instructive cases, accompanied by judicious remarks, illustrating the proper modes of removing foreign bodies from the external ear. The important fact made evident by the cases is the prejudicial nature of the rough or ill-directed attempts at removal which are generally instantly made when the foreign body is complained of. In fact, such attempts may give rise to inflammatory effusion, which forms a bulging ring around the foreign substance. Dr. Löwenberg also shows that the latter may remain in the outer ear for several weeks without occasioning great discomfort. Also that the foreign body, or the instruments used to remove it, sometimes perforate the membrana tympani; but that this membrane may eventually recover its integrity. We also notice that such perforation may be proved by the reappearance of water injected into the ear at the mouth and nose, or by forcible expiration when both the latter are closed. In the latter instance bubbles of air will be noticed in the water left in the meatus.

The author relies chiefly on injections of water, and, these failing, on the *agglutinating* method. To carry out the latter he uses a stem delicately surrounded with lint, the end of which, a little unravelled, is dipped in carpenter's glue. None of the glue should touch the sides of the stem. The latter is left from fifteen minutes to an hour, and the fastening becomes so firm that the foreign body can be extracted by careful management. A bead thus removed has remained attached to the stem for three years. Two cases, after all other means had failed, proved successful with the glue; and a third at once yielded to injections.

A strange circumstance was noticed in a boy of five years. A shirt button had been introduced into the left ear, and was eventually found there; but when the child came under Dr. Löwenberg's notice, the *right* ear was complained of. He saw nothing in it, and when, according to his custom, he explored the supposed sound ear, the button was seen lying against the membrane. The explanation was that, after the first attempts, the boy became so alarmed and confused that he presented to the first surgeon called the right ear, which, in consequence of manipulations, became inflamed, and was looked upon as containing the foreign body.

From the details of the cases it would appear that Dr. Löwenberg is in Paris quite a specialist a regards these extractions. He seems, however, to undervalue the delicate instruments which have been devised for the purpose. No doubt roughness or blind explorations are mischievous, but proper instruments, used with gentleness and with due regard to the anatomy of the part, should not be despised. The gluing method is very attractive through its simplicity and proved efficacy. The author especially directs attention to the position of the head when injections are used. It should be such as to favor the *vis a tergo* of the stream of water.

INOCULATION OF TUBERCULOUS MATTER ON THE HUMAN SUBJECT.

Messrs. Paraskeva and Zallonis, of Syra, in Greece, have published in the *Gaz. Méd. de Paris* (April 27th, 1872), an account of five experiments on rabbits, wherein tuberculous matter, either mixed with the food or inoculated, excited deposits of the same kind in the lungs, thus confirming Villemin's investigations. The authors attempted besides a bolder experiment, and inoculated a fisherman of fifty-five years with tuberculous matter on the upper part of the thigh. This man was suffering from gangrene of the great toe, in consequence of obliteration of the femoral artery. He steadily refused amputation, and the authors considered themselves justified in undertaking the experiment. The patient died thirty-seven days after the inoculation, and had never been ill before in his life. Seventeen tubercles in the first stage of development were discovered in the apex of the right lung; two were of the size of split peas, and the others as large as mustard-seeds. Two more tubercles were observed in the apex of the left lung. The liver looked healthy, but presented two tubercles on its convex surface of the size of peas. The authors conclude that they have proved their point, but it should be recollected that, in ordinary autopsies, tubercles are often found when their existence from the history could hardly have been suspected. As for animals, it may always be asked whether we can, in all cases, conclude that the phenomena observed upon them would be the same upon man.

SUPPOSED SCIATIC EPILEPSY.

A young Hungarian, going down stairs loaded with books and portfolios, fell on the seat. Severe pain and tumefaction ensued, for which appropriate means were used, but the front of the right thigh remained in a state of anæsthesia, and the back became over-sensitive. Severe convulsive movements now distressed the patient, and resisted all means directed against them. Billroth, of Vienna, who saw the patient, diagnosed irritation of the sciatic nerve. At this time pressure on the nerve caused severe spasms and convulsive movements of both legs. Even the arms contracted violently, and the fists struck against the chest. Billroth made an incision eight inches in length (?) along the sciatic nerve, between the tuber ischii and the great trochanter. The nerve was laid bare from the notch to the lower sacral foramen, and was, to the surgeon's astonishment, found in a perfectly normal state. The wound was closed, and the patient on waking had a fit. For two years and five months the convulsive movements recurred from time to time. At last severe onychia of the toes occurred, the nails came off, and Prof. Luminzer having incised the roots, the seizures never took place again.

CAUSES OF THE VARIATION IN THE AMOUNT OF OXYGEN AND CARBONIC ACID IN THE BLOOD.

Drs. Mathieu and Urbain give the results of their researches on the gases of the blood in the last part (May) of M. Brown-Séquard's *Archives de Physiologie.* They have ascertained the accuracy of the following law:—"That in warm-blooded animals the volume of oxygen absorbed by the blood varies inversely with the temperature of the air that they respire." In other words, with a warm external temperature the amount of oxygen circulating with the blood is diminished, with a cold external temperature it is augmented. Hence under the former condition all the processes of organic combustion are rendered more active, whilst they are retarded under the latter condition. Further, these experiments show that with diminished pressure there is diminished absorption both of oxygen and carbonic acid by the blood, and *vice versâ.*

HOUSTON'S OPERATION.

Hæmorrhoids, connected with prolapsus, were successfully operated upon by the application of nitric acid some thirty years ago by Dr. Houston, of Dublin. The method found great favor at the time, but has been somewhat displaced by the ligature, and the section followed by the actual cautery. Billroth, of Vienna, has of late revived Houston's operation, and reports excellent results, especially in those cases where frequent and dangerous hæmorrhage had occurred.

ABORTIVE TREATMENT OF GONORRHŒA.

M. Ledeganck maintains, in a pamphlet lately published at Brussels, that by cauterising the fossa navicularis on the third day the disease can be arrested, as the inflammation has not as yet extended further. Those who have much experience in the treatment of this complaint know that such a measure does not arrest the discharge, and that, by the application of nitrate of silver, abscesses on either side of the frenum are likely to appear.

THE INFLAMMATORY AGENTS OF PUS.

Dr. Chauveau, of Lyons, pursuing his researches on the general physiology of virulent fluids, has just published in *La Revue des Cours Scientifiques* an account of several experiments made with pus. The main result of these experiments (concludes M. Chauveau) has been to show clearly that putrid pus, which induces fatal results when injected into the tissues, becomes almost innocuous when it has been perfectly filtrated, thus giving evidence of the corpuscular state of the inflammatory agents of pus.

ADDISON'S DISEASE.

Prof. Béhier has delivered, at the Hôtel Dieu of Paris, a long and learned lecture on the above disease. He concludes that much obscurity still exists respecting the histology and pathology of the suprarenal capsules; and that it is not proved that in those bodies resides the power of developing the bronze skin. He would be inclined to say that the pigmentation just named may accompany every decided state of cachexia.

News Items, Medical Facts, &c.

A Hint about Feeding Patients Suffering from Wounds of the Throat.—An article in the *Indian Medical Gazette*, by Mr. J. B. Scriven, Civil Surgeon, Lahore, on Wounds of the Throat, refers to what seems to be a new fact that came under his observation. which may supply a useful hint in future cases of a similar kind. A young woman was admitted into the Medical School Hospital at Lahore in December last in consequence of a wound of the throat. She had been struck by an assassin with a sickle. This penetrated between the hyoid bone and thyroid cartilage, causing a wound of about three inches and a half in length. The glottis was visible, and a catheter could be passed over it into the pharynx and down into the stomach. With the view of anticipating the occurrence of œdema of the glottis, Mr. Scriven, acting on a lesson gleaned from previous experience of such cases, had recourse to the apparently paradoxical measure—which under such circumstances may turn out to be very good surgery nevertheless—of cutting his patient's throat again; in other words, he performed tracheotomy, and inserted a tracheal tube, through which the woman respired. An elastic catheter was passed through the original wound into the pharynx, and the patient was ordered to be fed through it by means of a syringe. Happily, however, she discovered that this was unnecessary; for on dipping the open end of the elastic gum catheter into any liquid, it traversed the tube, and passed into the stomach. Whilst calmly breathing through the tracheal tube she used "in this way to drink a pint of soup in about ten minutes at first, but afterwards in about three minutes." This was a new physiological phenomenon to Mr. Scriven and his colleagues, the explanation of which he could not positively determine. It could not have been effected by any suction power of the mouth, as the catheter entered through the wound directly into the pharynx. We are not told whether the patient could influence the movement of the fluid through the tube or not. We can only surmise that it might have acted as a syphon; but we do not know whether the arrangement admitted of such having been the case.

The Patent Felted Paper Curtains.—Amongst the novelties of the International Exhibition at South Kensington, not the least remarkable is the new felted papers of Messrs. Pavy, Pretto, and Company. All the windows and doors of the Exhibition are fitted up with curtains of this new material, which is a paper into which a certain quantity of animal fibre has been intimately felted. The effect is to give a remarkable toughness to the fabric, without any very great addition to the cost. Curtains, blinds, and bed furniture of the most beautiful patterns are produced at astonishingly low prices, and it is fully expected that the new material will replace leather in the covering of chairs, the binding of books, &c. The feature to which we desire to draw special attention is its superiority over cotton and woollen materials for the curtains of sick-rooms. It is non-absorbent, is easily brushed, and is not likely to retain contagion.

PRINTED AND PUBLISHED BY
WM. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 10. NEW YORK, OCTOBER, 1872.

Lectures

ON

THE SURGICAL TREATMENT OF ANEURISM

IN ITS VARIOUS FORMS.

Delivered at the Royal College of Surgeons, June, 1872.

By Timothy Holmes, M.A., F.R.C.S. Eng.,

Professor of Surgery and Pathology to the College.

LECTURE II.—Part I.

Mr. President and Gentlemen,—I endeavored to show in my last lecture that Brasdor's operation rests upon a sound pathological basis, so far as this—viz., that we have preparations in sufficient number to prove to us that impaction of clot in the distal end of the artery as it leaves an aneurismal sac is naturally (I do not say necessarily) followed by obliteration of that portion of the sac which is distended by the stream of blood formerly passing up the closed artery; that such obliteration, even when it stops short of the consolidation of the whole tumour, may suffice for the removal of all symptoms of the disease; and that precisely the same effect would follow naturally (but here again I do not say necessarily) on the distal ligature of the same vessel. This being granted, it follows that there are some cases of innominate aneurism which may be completely cured by Brasdor's operation.

The condition of an artery immediately after ligature varies much. In some cases an artery which, like the carotid, runs a long course without branches will be filled with clot from end to end (as in Dr. Wright's case); in others, as in Sir W. Fergusson's, no clot whatever will be contained in it. And the same thing is seen at a later period. Sometimes after division only a very small part of it is obliterated; at others the whole artery between the nearest considerable branches is converted into a solid cord. This must have its effect upon the obliteration produced in the tumour by the distal operation. If the blood is so indisposed to coagulate that after the ligature of the carotid no coagulum forms in the artery, it is probable that coagulation will go on but slowly or imperfectly in the sac. In those cases, on the other hand, where the artery is completely filled with coagulum, the blood in the sac will also show a strong tendency to coagulate: first the ordinary blood-clot will form, and this will subsequently become laminated, as happens after the ordinary operation.

It seems doubtful whether we can entertain the same expectation of complete obliteration of the sac from distal obliteration of the arteries when the tumour is partly aortic. An interesting case is here in point, communicated to the Pathological Society (Trans., vol. xx., p. 118) by Dr. Douglas Powell. In this case an aneurism, arising from the arch of the aorta, and involving that artery from above the origin of the innominate to below that of the left carotid, implicated the innominate in its whole extent, and it must have been impossible during life, as far as I can see by the recorded symptoms, to have diagnosed this from a purely innominate aneurism. After death it was found that the carotid artery had been converted into a solid cord from end to end. The right subclavian was also obliterated at its origin, though this, as I am informed by Dr. Powell, must have occurred very shortly before death, since pulsation in the right wrist, which had been absent during the presence of a considerable effusion into the pleural cavity, returned on its removal, and was perceptible till within a few days of death. Here, however, the circulation through the sac was kept up by the stream of blood traversing the aortic and left carotid orifices, and only a little laminated clot was found at its upper part. This deposit of clot was connected in all probability with the coagulation of blood in the carotid artery. And the analogy of the preparations of innominate aneurism above referred to justifies me, I think, in saying that, in an aortic aneurism also, the obliteration of any of the arteries, such as the right or the left carotid, which lead directly out of the sac, will be followed by consolidation in that part of the sac which is distended by the stream of blood formerly passing up the obliterated vessel.

In vol. ix., p. 83, of the same Transactions, a very interesting case is related by Mr. Mayo, where an aneurism of the arch of the aorta was found to be far advanced towards consolidation, in consequence of the impaction of clot in the main artery. The tube of the aorta was completely closed by this impacted clot, so that not even air would pass up the vessel. This impaction occurred at the usual situation of the congenital malformation; but the clot, though adherent, could still be detached from the wall of the vessel, showing that the obstruction did not arise from that cause. The aneurism was situated below the three great branches, and the enlarged condition of these arteries seemed to be a step in the formation of the collateral circulation. Death was caused by obstruction to respiration produced by pressure on the lung and trachea. Exposure to vicissitudes of temperature, and habits of intoxication, appear to have had a great share in rendering this obstruction fatal. If the man's habits had been different, Mr. Mayo conjectures, with reason, that the aneurism, which was found quite filled with firm coagulum, might have been spontaneously and permanently cured.

Many recorded cases show the severe symptoms which are produced by the distal impaction of clot in cases of aneurism, and the rapid and extensive coagulation which occurs in the aneurismal tumour. Let me call your attention, as an example of this, to a preparation and drawing (from the Irish College of Surgeons) from a case under the care of Dr. Staunus Hughes. I regret that time does not allow me to quote the whole of the very interesting notes which Dr. Hughes has sent me of this case, but I hope they will be fully published elsewhere. Suffice it to say that the patient was a gentleman past middle life, of a gouty habit, who had long labored under a pulsating tumour, the origin of which, whether from the aorta or one of its great branches, was obscure. He had much improved in health under treatment, and one day after breakfast was about to go out for a drive, having just said that he felt lighter and better in health than for years past, when suddenly he gave a loud cry, exclaiming that he had an excruciating pain in the chest, and felt so cold that he asked to be laid on the hearth-rug before the fire. Dr. Hughes happened to come in at the time, and found the patient in an agony of pain, pointing to the sternum as the seat of it. In a few minutes this pain disappeared, but he was immediately seized with terrific convulsions, in which he died in a few seconds. The dilated aorta and the left ventricle of the heart were found full of recent coagula. The valves were healthy.

We cannot hesitate to attribute these terrible symptoms to the sudden coagulation of the blood in the arterial dilatation, and the intense pain to the stretching of its coats and the resistance offered to the heart's impulse; and many other cases, to which I could give references if time permitted, show the acute symptoms produced by obliteration of a large vessel with impacted clot, and the rapid collection of blood coagulum which follows on such impaction.

There is, then, sufficient pathological evidence to show that an innominate aneurism may be obliterated by the obstruction of its two distal orifices, and to prove generally that the obstruction of an artery as it leaves an aneurismal sac will in all probability be followed by a deposition of firm laminated coagulum in the portion of the sac connected with the obstructed artery, whereby the growth of the tumour in that direction will be stopped. Now, we must always bear in mind that it is not usually the existence of an aneurism, however large, which is dangerous to life, but the growth of an aneurism, however small in some definite direction. To stop the growth of a small innominate aneurism towards the trachea, or of a small aortic aneurism towards the recurrent laryngeal nerve, would preserve the patient from a danger which cannot be over-estimated, and would be worth much more risk than is involved in tying the common carotid or the subclavian artery. Again, to line any portion of the sac of an aneurism where it is growing and threatening to burst, or compress important parts, is surely a reasonable object for an operation, even if the operation involves danger, and if the rest of the sac remains unconsolidated.

In combating Mr. Wardrop's proposal for reviving the distal operation, Mr. Guthrie endeavored to show, from the preparations preserved in our museum, that the distal obliteration of an artery would not cure an aneurism, and I think I ought not to pass over so important an argument, although the data on which it rests are not very extensive. It will be noticed that even in advancing this argument Mr. Guthrie is obliged to admit that such distal impaction is an effort of nature to cure the disease; that is to say, that the disease is partly cured by it, though he did not believe that it could be so entirely.

In treating of the spontaneous cure of aneurism, Mr. Guthrie speaks as follows:—"Two natural processes favoring the cure of aneurism by coagulation have been observed to take place. The first is the enlargement of the collateral branches to such an extent as to enable them to maintain the circulation if the main trunk be rendered impervious. The second is the effort made to close or shut up the lower openings from the aneurism, or those most distant from the heart—an effort which has been hitherto scarcely considered as a natural one, but which is frequently attempted, and often with success." In a further passage he says: "The Hunterian collection supplies several instances of all the openings into an aneurism save the upper one having been closed during life, and I think in sufficient number to establish the fact, that in aneurism of the extremities nature resorts to this method as a part of the curative process; and that although she succeeds in effecting it, still more is requisite to complete the cure by the filling up and obliteration of the tumour. These preparations also show, that when the lower end of the artery has been obliterated the aneurism has not ceased to increase. It is a step which, when once taken, favors the coagulation of the blood; and if a piece of coagulum be separated by accidental violence or other irregularity committed on it by the patient,* or by a state of syncope supervening, or by any other means, the coagulation of the whole of the fluid blood in the tumour may rapidly take place."

The preparations he refers to were numbered in the old catalogue 386, 392, and 397, and are at present numbered 1692, 1709, and 1703. Of the last preparation it will be sufficient to say that Mr. Guthrie clearly misunderstood the description which Hunter gave of it, in consequence of that description being, like so many of Hunter's, merely a short note or heading. His meaning was that the injection did not run through the sac into the artery in consequence of that part of the sac which lies next the artery being filled with clot. But a glance at the prepation shows that there is no obliteration of the lower end of the artery whatever. The bougie placed in it shows it opening freely into the dilatation which is part of the sac. I understand Hunter to have meant (as, indeed, the preparation shows to be the case) that cure had commenced by coagulum forming a barrier between the large sac and the small dilatation behind, which is part of the artery. No. 1709 (formerly 392) has been reexamined, and it is found that the artery, though nearly, is not quite obliterated. Thus, as the process is not complete, we can hardly speculate on its effect, even if the condition of the preparation was such as to admit of a decision on this point, which, however, it is not. The other preparation, No. 1692, in no respect answers to Mr. Guthrie's description as one in which all the openings into an aneurism save the upper one have been closed during life. A slight error is made in the old catalogue, where the obliterated artery is called the brachial, and the aneurism is described as axillary. In fact the artery is the axillary, and the aneurism affected the subclavian, commencing near the point at which the thyroid axis is given off. So that there would be a constant current in the aneurism through the large branches of the subclavian and axillary for anything that this preparation shows. Here, again, only a fragment of the aneurism has been preserved, so that it is impossible to say exactly what effect was produced on the tumour by the obliteration of the axillary; much clot has certainly been deposited in it. But while large branches are left (as was probably the case in this instance) opening out of the sac, it is likely enough that the impaction of clot in even the distal portion of the trunk itself will not cure the aneurism. Even so, however, it may lead to considerable deposition of clot, and may obliterate some part of the tumour, which otherwise would have grown, and possibly burst.

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* This is an interesting hint as to Sir W. Fergusson's proposal of manipulation.

Mr. Guthrie has endeavored to show that in the cases where Brasdor's operation has proved successful (and that it has been entirely successful in some cases he does not deny, nor that it has in others been partially so), this success has been produced by inflammation propagated from the seat of ligature to the artery, and thence to the walls of the sac. How far Mr. Guthrie would have modified his opinion had the phenomena of impaction of clot in vessels and its results been more fully known to him I cannot say; but I think that anyone who studies his lectures with attention will be of opinion that in this particular his judgment was somewhat misled. It is true that he brought forward the case of Hall, operated upon by Mr. Evans of Belper, and that of the negro operated on by Mr. Montgomery, and succeeded in showing that in both cases inflammation and suppuration in the tumour had followed on the operation. But in Hall's case this inflammation was caused clearly enough by imprudent exposure to accidents in riding and to intoxication after the immediate results of the operation had long passed away; and though the same cannot be said in Montgomery's case, yet suppuration in the sac follows occasionally after the Hunterian as well as after all other modes of treating aneurism, apparently as the result rather of imperfect coagulation within the aneurism than of inflammation of its parietes. Very probably this is more likely to happen after the distal than the proximal ligature, as it certainly seems to be after Anel's than after Hunter's method; but I confess that I fail to see the radical difference on which Mr. Guthrie so much insists. On the contrary, it seems clear that the coagulation in the sac has been more extensive and complete in cases where no inflammatory symptoms have been recorded.

I conclude therefore that there is *a priori* reason to believe that Brasdor's operation may be successfully applied to some cases of aneurism of the innominate artery, and that when applied to aortic aneurisms it may possibly be followed by partial consolidation of the tumour.

Let us now see what the results of distal ligature have been as applied to the treatment of innominate and aortic aneurisms. In order to exhibit the past experience of surgeons in this particular in an accessible form, and to indicate the sources from which information may be derived, I have constructed the accompanying table, which comprises all the cases I have met with. I have not separated the cases of innominate from those of aortic aneurism, since I am sensible that in those cases in which recovery ensued the separation would be merely conjectural.

I do not intend to use this table in a statistical manner. I do not deny that statistics have their useful application in many branches of inquiry, and even in some surgical questions. I would instance the very interesting statistics which a respected predecessor of mine, Mr. Hancock, produced to show the greater safety of resection in disease of the foot, as compared with amputation. This was a comparatively simple question, and the comparison implied was between cases in most respects perfectly similar. On the other hand, everyone knows, and all sound reasoners must have deplored, how statistics have recently been perverted in an attempt to support conclusions of the most doubtful character bearing upon the practice of our hospitals—statistics founded upon the comparison of things differing in so many and such important particulars that any discrepancies in the results of treatment might obviously bear many, and those quite opposite, explanations.

In order to use data such as those of this table statistically, we ought to be in possession of accurate facts both on one side and on the other. For it must never be forgotten that in all surgical questions the statistical method involves a comparison. In the present case, for instance, the comparison would be between treating an aneurism, assumed to be innominate or known to be partly aortic, by the distal operation in one or other of its forms, or by galvano-puncture, or by no treatment at all. For I repeat that the internal treatment should in all such cases have been tried and failed, unless the rapid advance of the tumour proves this to be useless. Therefore, in order to form a judgment, we must have definite data enabling us to compare cases exactly similar and treated in these three ways. But it is obvious that even the most accurate and complete collection of facts can hardly ever suffice for this. And even were there facts which could show us at all what is likely to happen if such tumours were treated by electricity, or what would be about the duration of life and what the urgency of the symptoms if they were left alone, yet the facts on which this table is founded are not, in truth, sufficiently uniform or sufficiently well digested to form the basis of any statistical reasoning. Such reasoning is only then trustworthy and accurate when all possible sources of error have been eliminated, and the facts reduced, as it were, to a common denominater. This is far from being the case in the table before us. Many of the operations were, evidently enough, performed haphazard, on imperfect diagnosis, or as a *pis-aller* in cases where there was no rational prospect of benefit. The results are unknown in some, and imperfectly stated in others; and in many of the fatal cases it remains uncertain whether death was caused by the operation or not, or, in cases of recovery from operation, whether the treatment was really beneficial.

DISTAL LIGATURE IN AORTIC AND INNOMINATE ANEURISM.

No.	Surgeon.	Artery affected.	Date.	Result.	REMARKS.
I.—DOUBLE CONSECUTIVE.					
1	Fearn	Innominate	1836, 1838	Cured	Preparation.
2	Wickham	Ditto	Sep., Dec., 1839	Died: second. hæmorrhage	
3	Malgaigne	Ditto	Apr., Oct., 1845	Died: erysipelas	
4	Bickersteth	Aorta and innominate	May, June, 1864	Died: suffocation	
II.—DOUBLE SIMULTANEOUS.					
5	Hobart	Aorta	1839	Died: second. hæmorrhage	Subclavian, 1st part.
6	Rossi	Innominate	1843	Died: operatn.	Subclavian, 1st part (?).
7	Heath	Aorta	1865	Recovered	Preparation.
8	Maunder	Ditto	1867	Died: operatn.	
9	Sands	Ditto	1868	Recovered	No benefit.
10	Durham	—	—	—	
11	J. Lane	Uncertain	1871	R.: operation	Ditto.
12	Holmes	Aorta and innominate	1871	Ditto	Ditto.
III.—CAROTID ONLY.					
13	A. Key	Innominate	1830	Died: operatn.	
14	Neumeister	Ditto	1830	D.: hemiplegia	
15	Scott	Ditto	—	Died: rupture	
16	Dohlkoff	Ditto	1837	Died: paralysis	
17	Hutton	Ditto	1846	Died: suppuration of sac	
18	Wright	Ditto	1855	Died: cerebral abscess	Aneurism consolidated.
19	Ordile	Ditto	1859 (?)	Died.	No particulars.
20	Broadbent	Ditto	1861	Died: second. hæmorrhage	No permanent benefit.
21	Hewson	Ditto	1867	Died: operatn.	
22	Nussbaum	Ditto	—	Died: rupture	
23	Nussbaum	Ditto	—	Ditto	
24	Evans	Innom. and carotid	1828	Cured	Lived 39 years.
25	Morrison	Ditto	1832	Cured	Lived 20 months.
26	Vilardebo or Rompani	Ditto	1844	Died: second. hæmorrhage	
27	Pirogoff	Ditto (?)	—	Recovered.	Imperfect.
28	Mott	Innom. and subclavian	1829	Recovered: temporary cure	Lived 7 months.
29	Fergusson	Ditto	1841	D.: pneumonia	Preparation.
30	Porta	Ditto	1842	D.: erysipelas	No aneurism.
31	O'Shaughnessy	Aorta	1843	Died: galv. puncture	
32	Campbell	Aorta and innominate	1845	Died: suffocation	
33	Knowles	Aorta	1867	D.: apoplexy	
34	Hutchinson, of Brooklyn	Aorta and innominate	1867	Died: suffocation	Double ligature attempted.
35	Rigen	Aorta	1829	Recovered	Left carotid tied.
36	Tillanus	Ditto	—	Recovered	Ditto.
37	Montgomery	Ditto	1839	Died: suppuration of sac	Ditto.
38	S. Lane	Left carotid and aorta	1852	Died: rupture into lung	Ditto.
39	Pirogoff	Ditto (?)	—	Recovered	Ditto.
40	Pirogoff	Aorta and innominate	—	Died: hemiplegia	Ditto.
41	Heath	Aorta	1872	Recovered: much benefit	Ditto.
IV.—SUBCLAVIAN ONLY.					
42	Wardrop	Innominate	1827	Recovered: much benefit	Lived 2 years.
43	Broca	Ditto	1862	Rec.: ditto.	Lived 7 months.

For such reasons I think the mere facts that the death-rate among these cases is enormous, and that the evidence of sound and permanent cure is wanting save in three instances out of the whole forty-three, undeniable as those facts are—and no one wishes less than I do to blink them or to extenuate their importance,—are not of themselves sufficient to found a trustworthy judgment on as to the future applicability of the distal ligature. The novelty of the proceeding, and the great difficulty of ascertaining the indications for operating, are of themselves quite sufficient to account for a record even gloomier than that which I place before you. Let us not forget the early history of ovariotomy. What reliable judgment should we have formed by the statistics (which means usually the percentage of deaths, irrespective of all consideration of any other particulars) of the first fifty tabulated operations of ovariotomy?

A better plan, I think, will be to select those cases from which I can produce reliable histories, trustworthy drawings, or preparations, and endeavor as shortly as possible to see what lesson they teach.

The first part of the table refers to four cases in which the carotid was tied first and the subclavian afterwards, the intervals being two years, six months, ten weeks, and seven weeks respectively.

In one case, the first, of which the preparation is in the museum of this College, I think there can be no doubt that the tumour was checked in its growth by the operation, and that a cure was effected. Being most anxious not to exaggerate the benefits of these operations I shall not hesitate to admit the possibility of a relapse in this instance. But the sac is seen to be solidly coated with coagulum in every part, and there is no reason to doubt that the patient might have lived to the ordinary term of life without any further trouble from the disease; nor is it possible to deny that life was saved by the distal operation. The preparation, however, supports the view which I have previously endeavored to maintain, that the ligature of the third part of the subclavian necessitates the continuance of circulation through the sac; and if so, the absolute obliteration of the tumour, its radical cure in the strictest sense of the term, making relapse impossible, cannot be obtained by these means. For an innominate aneurism, even when it is not strictly speaking of the tubular variety, but arises from expansion of a part of the wall of the vessel, has hardly ever, if ever, a definite opening or mouth communicating with the innominate, but shares freely in the general circulation through that vessel. Now the ligature of the third part of the subclavian necessitates the continuation of that circulation. In this case you will notice how large the lower portion (or what I call the subclavian portion) of the sac is, and how it has grown down to, displaced, and compressed the trachea, and you will also notice that the obliteration of the carotid artery following on the ligature is not complete, the root of the artery as it leaves the tumour being quite pervious.

Mr. Fearn's case, then, may be accepted as a proof that it is possible by double distal ligature to check the progress of an innominate aneurism and restore the patient to a state of health. I would also call attention to the fact that in every one of the four cases the ligature of the carotid was, in the judgment of the operator, productive of relief, especially when symptoms of pressure on the trachea existed.

It cannot be a fortuitous circumstance that the ligature of the carotid has always been the operation which was first selected. The direct connexion of the artery tied with the sac no doubt ensures much more certainty of producing an effect on the tumour than when the third part of the subclavian is operated on.

In the three later cases the operation was unsuccessful. In Malgaigne's case so much benefit was produced by the ligature of the carotid that it almost seems as if a cure might have resulted under more favorable circumstances; but the patient left the hospital and took to drinking and other debauchery. Death was the result of erysipelas after the second operation, a circumstance purely accidental. In Wickham's case the first operation was productive of much benefit, but the second seems to have had no effect on the disease, and the man died from the rupture of the aneurism. In Bickersteth's the progress of things was much the same, and the man died three months after the last operation from the progress of the disease.

In all these cases the aneurism was innominate, though in Mr. Bickersteth's the aorta was also affected.

The second part of the table contains eight cases, of which, however, the first two must be separated from the others, as being strictly on the principle of Brasdor's operation—i.e., distal ligature, with no branch between the sac and the ligature.

Such, at least, is the view usually taken of Rossi's case, though it must be allowed that some of the accounts speak of it as being a ligature of the third part of the subclavian. This is a matter of less importance that it would otherwise have been, since the operation proved fatal, and the accounts which have been preserved of the case are so meagre as to render it of little use for any practical purpose.

Hobart's case also unfortunately proved fatal, but it is a case of such unique interest that I must be excused for dwelling on it. "The case was supposed to be one of aneurism of the arteria innominata, and in order to give the patient (a female, aged twenty-five) a chance the distal operation was decided upon. A V-shaped incision was made, one leg of the V being parallel to each of the vessels, and the surgeon without much difficulty came down upon the arteries. The subclavian was tied between the innominata and where it gives off its first branches, and the carotid one inch above its origin. On the fourteenth day after the operation the ligature came away from the subclavian artery without any hæmorrhage, and everything promised a favorable result, especially as the pulsation in the tumour had quite disappeared. On the sixteenth day the patient, a woman of violent temperament, had a quarrel with the nurse, when she jumped out of bed, seized a pillow and some books, and threw them at her. While making these exertions hæmorrhage set in from the carotid, and the patient died shortly after. On post-mortem examination the arteria innominata was found healthy, and the circulation through it had not been stopped, but a pyriform tumour, which grew from the arch of the aorta to the left of the innominata, had overlapped, and to a certain extent had pressed upon, that vessel. It was found that *perfect union had taken place where the ligature had been applied on the subclavian artery*, but a small opening was found in the carotid, through which the hæmorrhage had occurred. The tumour was filled with a firm coagulum. The parts are preserved in the museum of the College buildings, Cork."*

The preparation has been kindly confided to me by the present Dr. Hobart of Cork, the nephew of the operator. Most unfortunately the subclavian artery has been entirely removed. In all other particulars the preparation verifies each detail of the description, and the facts as they are related can be vouched for by living eye-witnesses. But it would have been of singular interest to have seen a definite proof that the first part of the subclavian is susceptible of healthy union after division by a ligature, and that the common impression of the necessary and universal fatality of this operation is incorrect.

As these are the only two cases operated on strictly after Brasdor's method, I must admit that there is at present no instance of the cure of innominate aneurism by this operation; though I cannot entertain any doubt that the operation, if it could be successfully performed, would cure the disease.

LECTURE II.—PART II.

We now come to six cases in which the simultaneous double distal ligature has been performed according to Wardrop's method.

The first of these cases belongs to Mr. Heath, to whose enterprise and unwearied perseverance the profession owes the realisation of Wardrop's proposal—a step in surgery which I am confident will lead to much practical advance in the treatment of thoracic aneurisms. The operation has not as yet been permanently successful, but in Mr. Heath's case there was quite sufficient benefit to justify the operation and encourage its repetition. The preparation is in the museum of the College. The case is too well known to make it necessary to read it at length. I hardly think it possible to produce more distinct evidence of benefit from any operation than is furnished in this case by the relief to the dyspnœa, the diminution in the size of the tumour (so that it receded into the chest, leaving the hole in the thoracic parietes perceptible), and the prolongation of life so long as four years, under the most unfavorable circumstances. Had the patient kept quiet, and submitted after the operation to the regimen prescribed by Mr. Tufnell, it is possible that she might have been cured; and had it not been for the unfortunate administration of emetics it seems certain that the fatal growth of the tumour would not then have taken place. It must not be forgotten that in Mr. Heath's case the aorta was alone implicated, the sac projecting from the arch in front of the innominate artery, and having its mouth close to that vessel. The improvement must be referred to the derivation of the stream of blood into the lower part of the arch, thus relieving the first or convex portion of the vessel. But it is clear that in a pure innominate aneurism, such as Mr. Fearn's case, the effect would be much more direct and powerful.

The fourth case in the table, Mr. Maunder's, would in many respects be one of the most interesting if the account given of it had been adequate and the preparation carefully made and preserved. Death occurred on the sixth day after operation, and it would appear that it was attributed to the clotting of blood in the aneurismal sac, extending into and obstructing the aorta. The preparation, however, does not show this, for the clot has been removed or allowed to fall out. It must be evident how strong a bearing it would have had on the theory of this operation if we had really possessed tangible proof that so great an amount of coagulation could be produced in an aortic sac by the distal operation.

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* Flood "On the Arteries," edited by Dr. Power, Dublin, 1860, p. 134.

In Sands' case there was no very marked benefit (though it was thought there was relief to the dyspnœa) and no detriment from the operation. The paper which Dr. Sands published in the *New York Medical Record* for Feb. 1st, 1869, is a most interesting one, and will well repay perusal. In this case also the disease, as I learn by a letter just received from Dr. Sands, was purely aortic. The aneurism arose in front of the innominate artery, and had pushed that vessel back four inches from the surface of the body, but the innominate itself was not diseased. In fact, the state of parts must have much resembled that which existed in Mr. Heath's patient.

In Mr. James Lane's recent case the tumour extended so far up the neck that there could be little doubt the innominate artery was involved in the disease, though it was impossible to ascertain whether the aorta were affected or not. However, the case proved fatal some weeks after the discharge of the patient from the hospital, in consequence of the continued growth of the tumour and its rupture externally; and as no autopsy was permitted, the value of the case for our present purpose is much diminished. But we must not forget to note that in this case the operation, although, as far as can be judged by the notes, as clearly indicated as it usually is, proved entirely useless in checking the growth of the aneurism. I conjecture that in this case the tumour, though it extended far up the neck and simulated an innominate aneurism, really had its origin in the aorta.

Mr. Durham's case is also incomplete, for the patient died on the sixth day after the operation, apparently from the shock. No lesion was found which accounted for death. The tumour was extremely large, and extended up the neck so far that the carotid was only accessible on a level with the upper border of the thyroid cartilage. Still the innominate was the only artery actually implicated in the aneurism, although the sac had grown for so great a distance along the course of the carotid; but the origin of the innominate in the aorta was greatly dilated. In tying the arteries it was noticed that the ligature of the subclavian (the first artery tied) was followed by a very marked diminution in the force and fulness of the pulsation; that of the carotid caused no further change. During the short time he survived, the aneurism was noticed to become smaller and more solid.

The case on which I operated was in many respects an interesting and important one; but I will only briefly indicate its leading features, from a very good account of it drawn up by my clinical clerk, Mr. Stirling. The patient was a man aged fifty, who was transferred to my care at St. George's Hospital by Dr. Wadham. The tumour was situated chiefly behind the first bone of the sternum, which was much absorbed, and it also extended above the sterno-clavicular articulation for a short distance. Its aneurismal nature admitted of no doubt; but there was no bruit, and no very striking difference in the pulsation of the right and left arteries, for though some difference in the pulse at the two wrists was thought to be perceived by the finger, this was negatived by the sphygmograph. The internal treatment of the aneurism, by absolute rest and moderate diet without stimulants, was sedulously followed from April 28th, the date of his admission, to Nov. 1st. The temporary improvement which always ensues in the commencement of this treatment, as the pulse becomes regular and slow, soon ceased. Efforts were made to stop the growth of the tumour by the injection of ergotine into the neighborhood, according to Langenbeck's suggestion, by acetate of lead long continued so as to produce its characteristic symptoms, and by local cold and compression; but it became evident that the aneurism was slowly extending through the thoracic parietes. It was decided, therefore, that the operation of distal ligature was justifiable, and I noticed and pointed out to my colleagues that on compressing both the subclavian and carotid arteries firmly a remarkable diminution occurred in the force of the tumour's pulsation and in its rate, and that, on testing the effect of compressing either artery singly, it was found that the effect was much greater when the subclavian alone was held than when the carotid was compressed. If it had been the reverse, I should have recommended the ligature of the carotid only; but as I have no confidence in the ligature of the third part of the subclavian alone for this disease, and yet it seemed that the stoppage of the circulation through that vessel checked the pulsation of the aneurism, I proposed the simultaneous ligature of both arteries. This was done on Nov. 16th, the vessels being tied tightly with a stout ligature of carbolised catgut, the ends cut close to the knot. I had previously ascertained on the dead subject that this ligature tied tightly on the subclavian artery divided the two internal coats. In each operation some small superficial vessels were tied, and it was found afterwards that by inadvertence silk had been used for this purpose in the subclavian wound, and possibly in the carotid also. The wounds were united with the continuous suture (also of catgut) and dressed according to the plan recommended by Mr. Lister. The patient, however, was restless, occasionally delirious, at night, and it was found next day that he had disturbed all the dressings, so that the attempt to treat the case on the "antiseptic" plan was abandoned. The carotid wound suppurated a little, but was finally closed on the thirteenth day. The subclavian wound soon contracted to a short sinus, and so remained till the patient's death. A day or two before the fatal issue the cause of this became apparent by the projection out of the sinus of one of the truncated silk ligatures, and after death another small knot of silk was found loose and lying close under the little opening. The tumour at first diminished slightly in its pulsations, but soon returned to about what it had been before the operation, and it was quite evident that the ligature had had no curative effect. In the last few days of the year it began to grow rapidly and to produce great dyspnœa, the skin also became somewhat thin and livid. Rupture of the sac being obviously impending, it was resolved to try galvano-puncture. Four needles were inserted parallel to each other and about an inch apart. Dr. Althaus applied the constant current of from ten to twenty-five cells of Smee's battery (Foucaux's arrangement). The positive pole was first applied to each needle, the negative being in the first place applied to the skin; then the positive pole was applied to the next needle and the negative to the one from which the positive had just been shifted, the changes being made every five minutes, so that the whole process lasted twenty-five minutes. He complained much of pain, particularly when the changes were made. Arterial blood jetted freely from the punctures as the needles were withdrawn, but the hæmorrhage was at once checked by pressure. For the first two days the tumour decreased in size, but afterwards it increased both in size and pulsation; redness and œdema extended around it in all directions, and he died on Jan. 10th, at 9 A.M. On the previous night, seeing that the skin was whitish and soft, and apprehending that the tumour might be suppurating, I punctured it deeply with a thin knife. A little froth of blood and a gush of air alone issued from the puncture. At the autopsy the whole of the cellular tissue around the tumour was found loaded with lymph and much indurated. This diffuse inflammation extended the whole way up the neck, rendering the dissection extremely difficult. The aneurismal sac involved the whole of the innominate artery, and opened into the aorta by a very large orifice. It was almost filled with blood-coagula, which were firmly adherent to the sac, but had not assumed any regular concentric lamination. The aorta was greatly dilated and very atheromatous. The heart was healthy. The arteries opening out of the tumour seemed healthy; their continuity was not interrupted at the site of ligature. On the carotid, lying between it and the vein, there was a lump, something like a small gland, and the artery was constricted. When laid open its calibre was found to be interrupted merely by a thin stratum of partly decolorised clot; a distinct ridge or transverse mark indicated the position of the ligature, and below this (between it and the heart), at about one-eighth of an inch distant, were two very minute apertures in the internal coat. One did not lead through the walls of the vessel; the other led into the small lump above mentioned, which was a mass of cellular tissue and débris of blood-clot, in which no trace of the ligature could be found, though carefully looked for, nor was there a trace of the ligature in any other part. In the subclavian also there were no remains of the catgut ligature. This artery was not interrupted in its continuity at the place where it had been tied, but was completely closed by a diaphragm less than a quarter of an inch thick.

The case appeared to me to point to the following conclusions:—That the double distal ligature will not produce consolidation in an innominate aneurism if it is partly aortic—i. e., if it opens into the aorta by a greatly dilated orifice. That galvano-puncture may be employed in such a tumour with the effect of producing great consolidation; but that if this consolidation is very rapid there is risk of inflammation. That arteries may be tied as securely with the carbolised catgut ligature as with silk. That such ligatures melt away in the wound without being discharged from it. That an artery under such circumstances may preserve its continuity, whilst its tube is obliterated at the part tied; and thus that the chief risk of secondary hæmorrhage after the ligature is obviated.

In the *Boston Medical and Surgical Journal* for Aug. 6th, 1868, a case is related by Dr. Hodges in which these arteries were tied for supposed aneurism of the innominate or aorta. The patient died eleven days after operation. At the post-mortem examination (which was hurried and imperfect) no aneurism of either artery was found, though both were dilated. The tumour which existed during life "was undoubtedly the distended arteria innominata pushed up into the neck by the dilated and stiffened aorta."

A case is related by Dr. Cheever in the same journal for May 6th, 1869, (vol. iii., p. 239), in which he endeavored to tie these arteries, without success. The patient was rendered insensible and almost killed by ether. He took it for twenty minutes, and finally his blood became entirely dark and venous. The incision on the carotid artery failed to reach it, as the venous oozing obscured everything, and the operator could not see the parts. So with the subclavian: nothing could be seen; great venous hæmorrhage took place; the operator tried to secure the artery by touch; in passing the needle, as he supposed, under the artery, he lacerated the vein; and then tried to secure the vein by free dissection, and at last by cutting through the clavicle, but unsuccessfully. The patient died in two hours. The aneurism was purely innominate, contained a good deal of clot, and the vertebral was plugged. It was therefore a very good case for the operation.

Such is our present experience of the double distal ligature. It is undoubtedly a formidable proceeding, and one not to be lightly undertaken. There are the operative difficulties, which, in a tumour so large as Mr. Durham's, for instance, will prove sometimes formidable; there are the chances of erroneous diagnosis, as in Dr. Hodges' case, where there was no aneurism at all—in Mr. Heath's, Mr. Maunder's, and Dr. Sands', where it was purely aortic, and where any beneficial effect that could follow the operation would probably be secured by the ligature of the carotid only; and, lastly, there is the danger of not being able to complete the operation. In Dr. Cheever's case the failure seems to have been caused by extreme venous congestion produced by the ether. In a second case in which Mr. Heath attempted the double ligature, he was unable to reach the subclavian in consequence of its being overlaid by a lobe of the sac, filling the subclavian triangle. Yet even in this case the tumour was purely aortic—a notable instance of the impossibility, in the present state of our knowledge, of pronouncing an absolutely confident diagnosis between aortic and innominate aneurism.

Let us now turn to the cases comprised in our third class, in which the carotid alone has been tied on the distal side of an aneurism; and here, I think, the evidence of benefit derived from the operation, in some of the cases, is so conclusive as to justify me in the inference that this operation is urgently indicated in any instance which may prove appropriate.

It will be convenient here to separate from each other those cases where the aneurism was proved to be innominate, or where there were good grounds for assuming that it was so, and those in which it was wholly or partly aortic; and I will only dwell on those cases which are known with sufficient accuracy to warrant me in founding definite conclusions on them.

Those which are reported as purely innominate are eleven in number: Aston Key's, Hutton's, Dohlkoff's, Wright's, Hewson's, Broadbent's, Neumeister's, Scott's, Ordile's and the two cases of Nussbaum. At first sight, and to one who followed the miscalled statistical method of reasoning, nothing can appear more disastrous than the experience of these eleven cases, since all the patients died within a few weeks at furthest from the time of operation. But in some of the cases which are more accurately known to us there is quite sufficient evidence of the curative power of the operation to justify its repetition—nay, I think, to make it imperative.

Perhaps the most striking and convincing case is that operated on by Dr. Wright, of Montreal. I can only give the briefest possible sketch of this important case; but I would urge upon those surgeons who wish to make up their minds fairly as to the worth of the distal operation in appropriate cases of innominate aneurism to study the able pamphlet which Dr. Wright has written upon this case, and to compare the preparation, as represented here, with the drawing which I have previously referred to of the spontaneous cure of an aneurism of this nature from St. George's Hospital museum. Finding that this important pamphlet is but little known, I have induced Dr. Wright to present copies to the library of this College and of the Royal Medical and Chirurgical Society.

Dr. Wright's patient was a man seventy years of age. The tumour presented in the episternal pit, in about the median line. It had been mistaken for an abscess, and had been poulticed; but it had the pulsation and bruit of aneurism, and was on the point of bursting, the skin over the apex of the tumour being "greatly attenuated and slightly excoriated." The ligature of the carotid was followed by immediate subsidence of the tumour, and rapid consolidation of its more prominent part. And what is, as far as I know, unique in this case is that the attenuated skin gave way on the second day after the operation, and large quantities of serum continued for more than a week to drain away through it from the subjacent blood-clot, nor did union become perfect in the resulting fistula for more than three weeks. The tumour gradually subsided, and in about a month had ceased to be perceptible. But about three weeks after the operation he was attacked with hemiplegia, and, after lingering in a dubious state for a long while, he died, eighty-seven days after the ligature, with abscesses in the brain.

The immediate effects of the operation speak for themselves: the aneurism on the point of bursting before the operation; the immediate collapse of the tumour, and the diminution of its pulsations; the giving way of the skin on the second day after the operation; the fact that the sac was then so obstructed by coagulum that the fluid which oozed from the rupture was not blood, but serum; the continuous shrinking of the tumour as the clot became firmer and more fibrillated; and the ultimate healing of the sinus as the tumour shrank. The operation proved fatal by its effect on the brain, the patient dying with abscess in the cerebral hemisphere, as has happened in some other cases of ligature of the carotid. But the state of parts shows clearly that, had he survived, the aneurism would in all likelihood have given him as little trouble as Dr. Bence Jones's patient experienced after the obliteration of the carotid by impacted clot. The resemblance is, in fact, most striking. The carotid is seen in Dr. Wright's, as in Dr. Bence Jones's case, to be obstructed from its origin in the tumour to its bifurcation. The whole of the upper and front part of the sac is filled with firm clot, and the only portion which remains unobliterated is the channel which has been left for the transmission of the blood from the aorta to the subclavian.

Dr. Hutton's case is interesting mainly on account of the suppuration of the sac which ensued, and which was the cause of death. In this case "Valsalva's treatment" had been tried previous to the operation; and if by this expression is meant the enormous venesection and the injudicious depletion which that surgeon recommended, it is quite open to us to conjecture that both the failure of union which affected the portion of the artery included in the ligature and the failure to form firm coagula in the sac may have been due to the impression so produced on the system. In this case the subclavian artery was partially obstructed by coagula, which would have been probably a favorable circumstance for the cure of the aneurism had the patient possessed sufficient power of recovery. The failure of union in the artery and of consolidation in the sac in this instance does not render the occurrence of both less probable in a patient of more vigorous constitution

LECTURE II.—Part III.

Continuing our summary of the table of distal ligatures of the carotid artery, we come to the cases reported as mixed innominate and carotid or subclavian, which are seven in number—viz., Evans's, Morrison's, Rompani's or Vilardebo's, Pirogoff's first case, in which the carotid was judged to be also affected; Mott's, Fergusson's, and Porta's, in which the subclavian was implicated.

Of the first four, allowing the accuracy of the diagnosis, it seems that the first two were cured. The third died from the effects of the operation, but the aneurismal sac was found almost filled with fibrine three weeks after the ligature; and Pirogoff's patient derived so much benefit from the operation that he insisted on leaving the hospital ten weeks after the operation.

Evans's case is so well known and has been so fully commented on by Mr. Guthrie that I need not spend much time on it. The attempt Mr. Guthrie makes to show that the cure which undeniably followed on the operation was due to "inflammation" seems to me quite unsuccessful, and I am much surprised to find this explanation adopted by eminent surgical pathologists. It is true that the explanation is that of Mr. Evans himself, deduced from the acute symptoms which are described as preceding the disappearance of the pulse from the right arm. But the phenomena which attend the impaction of clot in a great vessel were then unknown. Judging from the analogy of cases like those already referred to, in which symptoms of precisely the same nature followed on the impaction of clot, with no possible cause or suggestion of inflammation, and of such preparations as that described by Mr. Lawrence (St. Barth. museum, ser. xiii. 69), which shows clearly how easy it would be for a small piece of clot to be washed into the mouth of the subclavian artery as it leaves the tumour, and how far such an obliteration might ultimately extend, I have no hesitation in ascribing the symptoms to the latter cause. If I understand Mr. Guthrie's (or Mr. Evans's) explanation aright, they conceive that, the ligature having been placed on the artery near the tumour, either the foreign body might have propagated inflammation down the coats of the vessel to the sac, or the disturbance of the cellular tissue near the tumour might have caused it to inflame, and hence the subsequent rupture of the sac. The latter is, indeed, a probable cause of inflammation of the tumour, whatever we may think of the former. But how would either explain the rupture of the aneurism after a year's interval of health? It seems to me clear enough that the ligature produced coagulation of blood in the sac, and that afterwards (the patient living an irregular life and being frequently thrown from his horse in the interval while intoxicated) some accidental violence caused the tumour to suppurate—an event by no means unusual after the cure of aneurism in other ways. At any rate, there is no doubt of the reality of cure, for Dr. Evans of Belper, the nephew of the operator, tells me that the patient lived till the year 1865, thirty-seven years after the operation, dying then of bronchitis at the age of sixty-seven; and he enjoyed good health, and lived a life of great activity and, for some time at any rate, of great irregularity. There was no autopsy, consequently the exact position of the aneurism was not ascertained.

Dr. Sands, of New York, in an interesting paper in the *New York Record* before referred to, speaks of this case as probably aneurism of the carotid only. It is impossible now to decide the question, but it seems to me more likely that Evans was right, since, if we look carefully at the notes, we shall see that the cure of the aneurism did not commence till the symptoms announcing obliteration of the subclavian had set in. I have little doubt therefore that the innominate was really implicated; and the facts I have stated, I think, go far to show that in aneurism of the innominate, after ligature of the carotid, should the mouth of the subclavian be anyhow obliterated, the cure of the aneurism would be the natural result if the patient survives.

In Morrison's case, again, there seems no doubt that a perfect and permanent cure was effected. The man died twenty months after the operation, of disease of the heart, if I understand the account aright. One interesting feature in this case is, that it is almost the only one of the kind in which a very great increase of pulsation is noted as following immediately on the operation. The exact condition of the tumour at the conclusion of treatment is not described, but the man recovered sufficiently to "labor hard at the making of charcoal," and dropped down dead quite suddenly after a walk, twenty months after the operation, being then in good general health. The account of the post-mortem examination is as follows:—"The right subclavian artery was traced to its source, at which point it was partially dilated. The arteria innominata was double its ordinary size and studded with spiculæ of ossific matter. The right carotid, from its origin to the point to which the ligature had been applied, was dilated into a sac, which was plugged up with a dense fibrous deposit. The portion of the tumour which was within the chest was much more voluminous than that which rose into the neck. The anterior surface of the thoracic portion of the tumour was firmly adherent to the sternum. There never was a more splendid specimen than this, showing the manner in which nature cures this formidable disease, when assisted by art." The diseased appearances in the heart walls and valves are then described, and to this cardiac mischief the patient's sudden death seems to have been due.

Vilardebo's case, so far as can be judged from the slight notice of it in the *Arch. Gén. de Méd.* (ser. 4, t. xv., 1847, p. 547), seems to go, along with Mr. Lane's case, with the preparation from St. George's Hospital (vi. 188), and with many other of the published cases, to show that coagulation may be produced in the carotid portion of an aneurism involving the innominate and aorta, by ligature or obliteration of the carotid. The notice in the Bulletin of the Acad. de Médecine runs thus:—

"M. Vilardebo communicated a case of aneurism of the right carotid and innominate, operated on after Brasdor's method. In this case, the patient was a negro aged seventy, who had, on the right side of the neck, between the sternal extremity of the clavicle and the lower border of the jaw, an enormous rounded, pulsating tumour. Valsalva's treatment having failed, the common carotid was tied below its bifurcation. The patient died on the twenty-first day.* On post-mortem examination, the carotid was found obliterated at the seat of ligature: the aneurismal sac in the neck had been formed at the expense of this vessel. Stratified layers of fibrine, firm and very adherent, covered all the internal surface of the sac, except at the highest point of the tumour, where the skin was very thin. A small quantity of black viscid blood filled the rest of the cavity of the aneurismal sac. The latter, which was of considerable volume, extended downwards and outwards, resting by its lowest part on the right lung, which it compressed, and in its course it also compressed the subclavian artery. The innominate trunk formed a second tumour, much smaller than the former, springing from the posterior portion of the vessel and extending backwards; it was almost entirely filled with fibrinous concretions, which left in their centre only a very narrow channel to give passage to the blood. The aorta was much dilated."

Of the three cases in which the subclavian artery was implicated in the aneurism, as well as the innominate, by far the most interesting to us is that under Sir W. Fergusson's care, of which the authorities of King's College have been so kind as to lend me the preparation; and this case is made still more valuable by the fact that its history has been related in full, and with a commentary worthy of the importance of the subject, by Sir W. Fergusson. In that excellent paper Sir W. Fergusson calls attention to the diminution in the size of the tumour which followed on the operation, and to the formation of clot, which, he says, was nearly similar to what is found in aneurismal sacs at about the same period after the Hunterian operation. The carotid alone was tied, partly on account of the much more marked effect produced on the size and circulation in the tumour by distal pressure on that vessel than on the subclavian,† and partly in deference to Mr. Wardrop's opinion, who saw the case, and believed it quite possible that the ligature of the carotid might be perfectly successful, and no further operation be required. It seems, however, that the fact, shown by the preparation, that the aneurismal enlargement affected the subclavian also, renders this very doubtful; and the case also illustrates the additional danger which might beset the attempt in such a case to secure the first part of the subclavian artery.

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* The case spoken of as Rompani's in Schmidt's Jahrbuch, vol. lxxvii., p. 236, was also a man aged seventy, who died on the twenty-first day, and also had a double aneurismal sac. I have no doubt that the two are identical.

† It may be remarked that in this case, as in that of my patient, in whom the double distal ligature was performed, the distal pressure produced not merely a diminution in the size of the tumour and the strength of its pulsation, but also a remarkable retardation in its rate of pulsation.

In Porta's case, as in that above referred to at Boston, U.S., the mistake was made of confounding a dilated condition of arteries, resembling that shown in the drawing from a preparation at St. George's Hospital, for a sacculated aneurism. Otherwise the case is of little interest, for the patient died on the second day after operation.

In Mott's case the improvement was so great that it may be said that the patient was temporarily cured. When the first notes of the case terminate, all tumour and all pulsation above the clavicle had disappeared, and the man had gone home as cured. But the tumour continued to grow internally. The man soon began to suffer from dyspnœa, the consequence of pressure on the trachea, a lobe of the sac having passed between the bifurcation of the trachea and the arch of the aorta, and he died of dyspnœa seven months after the operation. The amount of coagulation in the sac is not described, nor is the account of the tumour sufficiently exact to enable us to judge what had been the real effect of the operation. Mott, in his remarks on the case, regrets that he did not tie the subclavian as well as the carotid. The disappearance of the upper part of the tumour after the ligature of the carotid appears to support the idea that the obliteration of the efferent artery was accompanied by obliteration of the carotid portion of the sac, but that this was insufficient for cure in consequence of the preponderance of the subclavian portion.

We have next four cases in which the aneurism was either purely aortic, or aortic and innominate, and for which the right carotid was tied —namely, O'Shaughnessy's, Campbell's, Knowles's, and Hutchinson's, of Brooklyn. In the first case no time was given to test the effect of the ligature, galvano-puncture being resorted to only a few days afterwards with fatal results. In Campbell's case there was a considerable amount of coagulum in the sac, but the aorta was extensively diseased, and the case was probably incurable. Hutchinson's case is both curious and interesting. The operation was undertaken as one of ligature of both arteries. But no pulsating vessel could be found in the situation of the subclavian, and after death it was proved that the substance around which the ligature was placed was not that artery. The subclavian was obliterated; the operation on the carotid did not check the growth of the aneurism (as I believe it would have done had the aneurism been purely innominate), and the patient died in six weeks of dyspnœa.

The table is completed by seven cases in which the left carotid has been tied for aortic aneurism. These cases belong to Rigen, Tillanus, Montgomery, S. Lane, Pirogoff (two), and Heath.

In all these cases I think there is clear evidence that the operation was beneficial, except in Pirogoff's last case, where the patient died from the direct effects of the ligature upon the brain.

The cases of Tillanus and Rigen are those which are best known and most universally quoted in proof of the proposition that aneurisms affecting the arch of the aorta may be sometimes, at any rate, checked in their progress by ligature of the left carotid.

The following is what Velpeau * says about these cases:—

"It would appear *a priori* that ligature in the hyoid region might suffice in all the cases in which the aneurism occupies the carotid only; but that it would be necessary to join to this the ligature of the subclavian when the innominate trunk is itself affected. Now M. Kerst, of Utrecht, has communicated to me two facts, which, with those of Evans and Montgomery, prove incontestably that the ligature of the carotid only may arrest the development not only of aneurisms of the innominate, but also of those of the arch of the aorta.

"A man was admitted into the civil hospital at Amsterdam with an aneurism projecting above the sternum. M. Tillanus, believing it to be an aneurism of the left carotid, tied that artery a little higher up. The patient recovered. Five months afterwards he died suddenly. The aneurism, which occupied the arch of the aorta itself, was completely filled with a white coagulum. The preparation is preserved in the Pathological Museum at Amsterdam.

"In the other case the aneurism appeared in the same situation, and was on the point of bursting. Believing also in this case that the aneurism was of the left carotid artery, M. Rigen, of Amsterdam, tied this artery a few inches higher up, on Feb. 21st, 1829. The grave symptoms disappeared and the tumour diminished considerably. On May 9th of the following year it became necessary to operate on this man for strangulated hernia; but he died on June 13th with symptoms of spasm or asthma. The autopsy showed that the aneurism affected the arch of the aorta between the left carotid and the innominate. As in M. Tillanus' case, it was filled with white coagulum and considerably diminished in size."

After relating the cases of Tillanus and Rigen, Velpeau goes on to say:—"We see then from this that ligature of the carotid on the method of Brasdor deserves trial even in cases where the aneurism appears to be prolonged down to the aorta. I must, however, still doubt whether the chances of success would not be notably increased by the ligature, simultaneous or consecutive, of the subclavian; only there remains the doubt whether the internal mammary, vertebral, thyroid axis, &c., would not keep up the circulation in the root of this vessel, and thus destroy all the effect of the ligature upon the aneurismal sac."

Montgomery's case has been very fully detailed and commented upon by Mr. Guthrie, and he uses it as another argument, in addition to Evans's, to prove that the distal ligature, if it cures, cures by producing inflammation in the sac. It happens singularly that Evans's and Montgomery's cases, which were alone known to Mr. Guthrie, are the only cases, except that of Hutton, out of the whole twenty-nine operations in this class in which this event followed. So dangerous is it to generalise upon the result of one or two cases. Whatever else may be thought of Montgomery's case, there can be no doubt that the operation was followed by a great subsidence of the tumour.

In Pirogoff's case which survived, the operation was followed by striking relief to the dyspnœa from which the patient had suffered, and some diminution in the tension and pulsation of the tumour. The patient quitted the hospital relieved in about ten weeks. We can hardly doubt that in this case, as in Mr. S. Lane's, the carotid portion of the sac was consolidated, and the tumour thus diminished and its growth stopped for the time.

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* Nouveaux Elements de Med. Op., 2nd ed, 1839, vol. ii., pp. 243-4.

Mr. Lane's case is usually quoted as one of aneurism of the left carotid, since the aortic portion of the sac was imperceptible before the operation, and it was, therefore, diagnosed as a carotid aneurism, but the account in the last edition of Cooper's Surgical Dictionary (vol. i., p. 215) removes all ambiguity. The preparation has unfortunately been lost; but Mr. Lane's description of the post-mortem examination states distinctly that the patient died on the sixty-eighth day after the operation from the increased growth of the lower or aortic portion of the sac and its rupture into the left lung, while the upper or carotid portion was entirely consolidated. The aorta in this case was not itself dilated, but the opening of the carotid artery from the aorta was; and it is quite evident that it was the entrance of this great stream of aortic blood into the sac which hindered the consolidation of its lower portion, and prevented the complete success of the operation.

Mr. Heath's case is the latest in which the operation has been practised. It is not at present completed, but as far as it has gone it furnishes an ample justification for the course pursued. The relief obtained by the operation was great and immediate, and there were no unpleasant symptoms whatever from the operation. As the patient happens to be in town, I have asked him, with the kind consent of Dr. Cockle, to attend here in order that any gentleman who wishes to examine him may satisfy himself from the man's own statement of the reality of the relief which the operation produced. He can now walk fourteen miles in one day, and feels perfectly well. The left side of the chest is quite flat. The tumour is felt pulsating in the episternal notch, and the right thoracic parietes bulge somewhat forwards, but the disease is plainly receding. The patient was a man aged forty-eight, a farm laborer from Cambridgeshire, who came under Dr. Cockle's care in January, 1872. He had experienced pain, for four or five years, in the right side of the head, neck, shoulder, and chest, attributed to rheumatism. In April, 1871, he noticed a pulsating swelling in the hollow of his neck, and was in Addenbrooke's Hospital for two months without relief. After Christmas, 1871, he was compelled to give up work, from the increasing pain in the shoulder and neck. He presented marked symptoms of aneurism of the ascending and transverse portions of the arch of the aorta. There was displacement of the right sterno-clavicular articulation, with projection of the right and upper portion of the sternum; and the episternal notch was filled by a pulsating swelling. There was a strongly heaving and expansile impulse over the whole of the swelling, and marked dulness on percussion existed over the whole area of the tumour. No bruit could be heard, but a double concussion-shock was felt. The respiratory murmur was feeble or absent over the upper portion of the right lung. The heart-sounds were dull and muffled, but without appreciable murmur. There was no cough or difficulty of deglutition. The left radial pulse was decidedly feebler than the right; and the left pupil was much dilated, and the left eye congested. On exertion he complained of shortness of breath, palpitation, and pain. Mr. Heath tied the left carotid artery above the omo-hyoid on February 26th, 1872. Carbolised catgut was used for the ligature, and the wound was covered with cotton-wool. No constitutional disturbance followed. Within forty-eight hours the patient could lie and sleep on his right side, which he had been unable to do before. The left pupil became natural, and the congestion of the eye disappeared. On the eighth day, when the dressings were removed for the first time, the wound was found completely healed by the first intention. Since the operation, the condition of the patient has in every way improved, the chest having become flatter, and the heaving impulse greatly diminished. He was free from all pain or inconvenience.

Our table concludes with two cases in which the subclavian artery has been tied in its third part for innominate aneurism. The first is the well-known case of Mrs. Denmark, operated on by Wardrop. The patient had very clear symptoms of innominate aneurism, and was suffering from most distressing feelings of impending suffocation. The pulse in the branches of the right carotid was imperceptible, and it was thought that there was no pulsation in the trunk of that artery, though on this the report is less positive. The carotid being, therefore, taken to be obstructed, Mr. Wardrop thought it well to tie the right subclavian in its third part on July 6th, 1827. "There was an immediate relief in her breathing, which now became free and tranquil, and this change was more striking as during the preceding twenty-four hours she had suffered an unusual degree of suffocating dyspnœa...... The peculiar sensations which she complained of in her head were also removed." The pulsation of the tumour was also thought to be diminished, though its size was not. On the ninth day a pulsation became perceptible in the carotid and temporal arteries, which Wardrop referred with great probability to a diminution of the size of the tumour, and consequent liberation of that vessel from pressure. The ligature came away on the twenty-second day, by which time the tumour was "greatly reduced in size." In the sequel she remained for some time with hardly a trace of the former disease, insomuch that a surgeon who saw her at the end of August declared that unless he had previously been acquainted with the nature of the case he should not have known that any aneurism existed. At the end of the year she suffered severely from bronchitis, and was treated on the strongly antiphlogistic plan. In August, 1828, Mr. Wardrop makes the following note on this head: "It is important to observe that from the commencement of my attendance on this patient she has now been bled above fifty times, to an extent at each operation seldom less than a pint of blood, and frequently to nearly double that quantity. Since the ligature of the artery she has been restricted to about an ounce of solid meat daily and twelve ounces of fluid. Besides these, however, she has frequently taken a very small quantity of bread-and-butter, and occasionally a little fruit. No tumour was now perceptible in the situation of the aneurism, but a hardness was perceived there from its condensation. She had had transient œdema of the feet, but this had subsided, and she takes exercise in the open air daily." This note was made on Sept. 9th, 1828. The patient survived till Sept.

18th, 1829, when she died of dropsy, and previous to her death the aneurism had enlarged above the sternum and up the root of the right carotid artery. Wardrop proposed to tie the right carotid, but was dissuaded, apparently, by some consultants, who thought the diagnosis wrong. However, on post-mortem examination, the tumour turned out to be an innominate aneurism. "The clavicular and tracheal portions of the sac were filled with firm coagulum, the cavity of the aneurism being chiefly limited to the division between the sternal and tracheal portions, and was about the size of a walnut. The layers of the coagulum were remarkably firm, and of a pale color, being of a softer consistence and darker color as they approached the boundaries of the aneurismal cavity." The aorta was healthy.

As far as this case goes there seems no possibility of denying that the patient derived benefit from the operation, though the possible effect of the rigid depletion before and after the operation must not be left out of sight. It is greatly to be regretted that the carotid was not tied, for the post-mortem examination seems to have proved that the subclavian portion of the sac was obliterated, and if she had survived the ligature of the carotid, there would have been a good prospect of the permanent cure of the aneurism as in Mr. Fearn's case. It is also much to be regretted that Mr. Wardrop did not fulfil the intention which he expressed in his report of this case, of presenting this very valuable preparation to the museum of this College.

The only other case in which the subclavian has been tied on the distal side of an innominate aneurism is that by Broca, of which full details are given in the "System of Surgery," and of which it will be sufficient to say that the benefit from the operation was well marked, and that the patient's death was produced by a disease which seems to have been quite unconnected with the aneurism.

There is a case on record (by Laugier) in which he tied the axillary for the cure of innominate aneurism; but this is not an example which anyone, I think, would not follow. The case is one of great interest in this respect, that the carotid artery was obliterated, and the subclavian healthy. The patient died from cough and dyspnœa, and after death extensive disease of the vertebræ was found. As far as the account of the tumour enables us to judge, it is very possible that a ligature placed on the subclavian at an earlier period, before the vertebræ were implicated, might have been successful.

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Clinical Lecture

ON A

CASE OF ACUTE TUBERCULAR DISEASE, WITH OCCLUSION OF THE VAGINA.

By Henry Thompson, M.D., F.R.C.P.,

Physician to the Middlesex Hospital.

Eliza W——, aged fifteen, was admitted under my care on the 26th of April, 1872. Her father and mother are alive and healthy, and there is no history of consumption in the family. The patient herself had always enjoyed good health until about six months ago, when she experienced acute pain in the head, recurring from time to time. For the last three weeks her fellow-servants had noticed that she was stupid and dull; previously she had been an intelligent and bright girl. On the 22nd of April she complained of severe headache, pains in the back and loins, sickness, and constipation. For some indefinite period she had been subject to a slight harsh cough. She had never menstruated. On admission, the tongue was foully coated; complexion purplish; expression dull; no cough; no dyspnœa; anorexia, but no thirst; complains chiefly of headache, nausea, and pains across the loins, in knees, and under right axilla; a few large pustular spots on the trunk; nothing of any moment discovered on examination of the chest. Ordered an effervescing draught, with spirit of chloroform, every six hours, and a warm bath.—Evening: Pulse 96, respiration 18, temperature 99°.

April 27th.—After a sleepless but not delirious night, the patient seems drowsy this morning. When moved and examined by percussion and auscultation she is fretful, and apparently hyperæsthetic; pupils dilated; no headache; tongue densely coated orange and white. No urine passed since admission. Twenty-five ounces withdrawn by catheter; specific gravity 1015; contains no albumen. Morning pulse 96; temperature 98·9°. Ordered, two grains of calomel at once, and in three hours a draught of castor oil. Twenty grains of bromide and two grains of iodide of potassium added to each dose of the effervescing mixture. Hydrate of chloral, fifteen grains, administered at night.

28th.—Slept well after the chloral, but tosses her limbs about this morning, seems stupid and heavy, and scarcely answers questions at all. Temperature 98·4°; pulse 102, unequal in force and rhythm. Ordered dry cupping to the nape of the neck, and a castor-oil enema.

29th.—Lies in a semi-comatose condition. Jactitation continues. Urine, after retention for twenty-four hours, withdrawn by catheter: specific gravity 1025; reaction acid; contains a trace of albumen. Motions after castor oil abundant, and extremely offensive. The head to be shaved, and compound camphor liniment, with the addition of half its volume of strong solution of ammonia, to be applied to the scalp; enemata of strong beef-tea—three fluid ounces with an egg—to be administered every four hours.—3.30 P.M.: Patient grinds her teeth; complexion intensely livid.—Evening: Pulse 188; temperature 99·6°.

30th.—Complete unconsciousness; extreme lividity. Pulse 146; respiration 54; temperature 101·8°. Died at 8 P.M.

Autopsy, twenty-four hours after death.—The following is an abridgment of Dr. Robert King's report:—Body well nourished. Both lungs, as well as their pleural membranes, were thickly studded with grey miliary granulations. There were also numerous subpleural ecchymoses. In both lungs were small patches of extravasation, and a few islets of lobular pneumonia in an early stage. The right lung was gorged with black blood; the left was almost equally congested, but with blood of a brighter red. The under surface of the diaphragm and the capsule of the liver were copiously beset with miliary granulations.

The liver itself contained a number of cheesy bile-stained bodies, a little less than a split-pea; these were found to consist of bile-ducts, nearly if not entirely obliterated by deposits within and around their walls. Granulations were found abundantly in the spleen and the kidneys, and in both organs they had partially undergone the cheesy metamorphosis. There was no ulceration of the intestines; but specks of tubercle were discovered on some portions of the peritoneal surface, especially on that forming the recto-uterine fold. The uterus and ovaries appeared to be healthy; but around the os uteri was a little patch which seemed to be roughened by miliary granulations. The vagina formed a huge sac, measuring eight inches in circumference, and containing some twenty-five or thirty fluid ounces of dark, grumous, offensive material, which escaped in abundance on cutting through the thick, tough, and imperforate hymen. Grey granulations were met with at the base of the brain, in the fissure of Sylvius, and on the upper surface of the cerebellum. The brain-tissue was extremely soft, and the lateral ventricles were distended with fluid and greatly dilated.

Commentary.—Gentlemen, at the onset we had the greatest difficulty in ascertaining the previous history of the case. The facts had to be gathered piecemeal from various sources. When gathered they were meagre in the extreme, and we were left in great measure to our own resources. There were many features of hysteria about the case, but a moment's reflection sufficed to dispel that idea. The tongue alone, densely and foully coated as it was, gave positive evidence strongly in favor of more serious mischief, while the absence of all quivering of the eyelids bore witness to the non-existence of hysteria. Indeed, I should rate the negative evidence as even stronger than the positive; for in my experience, amidst all the fleeting phases of that wonderful malady, this quivering of the eyelids is almost invariable. We were, then, compelled to look for some other way of accounting for phenomena which, apart from hysteria, were in the highest degree alarming. With some misgiving and some mental reservation, I wrote down the diagnosis of tubercular meningitis. When, however, I learnt that there was a history of severe and recurring headache six months ago, I began to waver in my decision. It seemed natural to connect that headache with the existing disease; and if there were grounds for assuming this connexion, it appeared impossible to retain the original diagnosis, for tubercular meningitis would assuredly have done its work and taken the life of the patient long before six months were over. I wavered then for a while, and bethought myself of abscess or tumour in the brain, but I never erased the written words from the card, and the last thirty-six hours of the girl's life proved conclusively to my mind that the old diagnosis was the true one. The girl, during the whole period of her residence in the ward, had been remarkable for the purple flush of her complexion. Now, however, she became intensely livid, and even cyanotic. You are aware that death by apnœa and death by coma are formally distinguished from each other, and they are truly distinguishable at their origin, but at their close they present many phenomena in common, and in the dead-house the appearances in the lungs are much the same. It might, therefore, have been argued with some show of reason that the intense lividity of the girl betokened nothing more than coma assuming naturally the characters of apnœa at its close. The argument would have been untenable. The lividity had existed at the onset long before the coma began, and in the end it was intensified in a degree, and for a period of duration, utterly inexplicable on the hypothesis of coma alone. There must inevitably have been all along intrinsic disease of the lungs quite independent of any pathological process going on within the cranium, and such intrinsic disease by exclusion must have been tubercular. I say by exclusion for this reason: if the lungs on examination tell you nothing at all, or nothing of any special significance, and yet, on general grounds, you are assured of the presence of lung disease, then you may be equally sure that the lung disease is tubercular, and under the same circumstances you may reasonably surmise that the tubercle is widely disseminated, not in the lung-tissue only, but elsewhere throughout the several organs which it usually invades. Amongst these organs the membranes of the brain, in particular the pia mater, occupy a pre-eminent place. Tubercular meningitis, then, was about as safe a diagnosis as any fallible man could make. The old headache was, in all probability, connected with the arrest of the catamenia, of which I shall presently speak, and stood in no direct relation to the existing symptoms.

If you remind me that there was no history of consumption in the family, I reply that such a history, to the best of my belief, has little or nothing to do with the far-spreading development of miliary tubercle. If, again, you tell me there was no acceleration of pulse or breathing, at least before the closing scene, no cough, no dyspnœa, properly so called, no elevation of temperature, I reply that, strange as it may seem, you *may* have none of these things in tubercular meningitis. With regard to cough and dyspnœa, as the mere diffusion of pulmonary tubercle cannot be discovered by physical examination, so it fails to betray itself outwardly by any violent or explosive act; and in truth it is one of the best-marked features of tubercular meningitis that the chest-symptoms go to sleep as it were, while the brain is unnaturally awake. With regard to the pulse I have known at least two cases of acute and universal tubercular disease where the pulse never exceeded 96 from first to last, so long as they were under my observation. Similar remarks apply to the respiration. Understand me rightly, however. I am not going to deny that before the accession of brain symptoms both the pulse and the respiration may have been, and probably were, above the normal average. Finally, with regard to temperature I have been enabled to diagnose tubercular meningitis in a difficult case on the simple ground of a temperature never exceeding 99°. Again understand me rightly. I am not affirming the absence of well-developed fever heat as the rule; far from it. I only say that fever heat may be absent in some cases towards the close, and even during the whole period of medical supervision.

So much for the aspect of the case during life. Let us now pass in review the two most remarkable features in the discoveries of the dead-house: the wide-spread dissemination of tubercle, and the arrest and imprisonment of the catamenia within the sac of the vagina, for sac it may be called with propriety when its orifice was closed by an imperforate hymen, and its walls presented a circumference of eight inches.

Gentlemen, I need not inform you that modern researches have gone far to set aside the old doctrine so long prevalent as to the origination of tubercle, and have established the general, if not universal, law that tubercle, at least in the disseminated form, is not a neoplasm or primary growth, the result of a peculiar dyscrasia, but a secondary deposit, the offspring of an infective process. The aboriginal source of that infection is said to be for the most part some material which has undergone the caseous degeneration. Analogous deposits in the form of secondary abscesses occur, as you know, in pyæmia. Now, assuming the truth of the aforesaid law, how are we to account for the acute development of tubercle in our case? On careful examination we found nothing whatever to explain it, save only the collection of decomposing catamenial blood in the vagina. Blood of course contains white corpuscles, and white blood-corpuscles are held by many to be chiefly, if not exclusively, the progenitors of pus-globules. Blood, then, in its retrograde metamorphosis, may be supposed to go through a process akin to suppuration, and on this view no one would have been surprised if we had discovered in our case the evidences of pyæmia. Again, there is but one step between suppuration and caseation; and, indeed, the essentials of the change in question may have actually existed in this girl, although, of course, the appropriate form and consistence were wanting; for condensation was impossible with a fresh flow of blood recurring at each catamenial period. On this view there is nothing unnatural or astonishing in the discovery of tubercle, which, in the absence of all other conceivable sources, may, with extreme probability, be ascribed to the decomposing blood-mass in the vagina. Perhaps, however, I have been refining too much and drawing unnecessary distinctions. It may be that sometimes the same foci of infection give rise to pyæmia and tubercle indifferently, the particular product developed varying with the constitution and surroundings of the patient. Nay, more, it is not beyond the limits of a fair presumption that both processes may occur at the same time in the same individual, and in our own case it might not be going too far to designate as products of something like pyæmia the pustular scabs on the trunk and the ecchymoses beneath the pleura. There is an obvious reason why the two processes should not *often* be found co-existing in the same person; pyæmia will, for the most part, kill before tubercle has time to grow.

I conclude, therefore, in favor of the disintegrating blood as the fountain-head of the mischief, and this conclusion is positively and strikingly confirmed by the multitude of miliary granulations clustered around the os uteri, and scattered throughout the recto-uterine fold of the peritoneum. If I am right, the case is almost unique, for I cannot recollect having heard or read of tubercle originating in the decomposition of blood.

Clinical Lecture

ON

TRACHEOTOMY IN CASES OF CHRONIC LARYNGEAL DISEASE.

By Eben. Watson, M.A., M.D.,

Professor of Physiology in Anderson's University, and Surgeon to the Royal Infirmary, Glasgow.

Gentlemen,—Chronic disease of the larynx is of many kinds and varieties, and of these you have lately had some illustrations in my clinical wards. Most of these cases get better, if properly treated, without operation; but for this end the patient must be perhaps confined to the house, and certainly retained for a long time under medical care. Hence it is likely that a certain proportion of such cases will become worse from various causes, not quite under the control of either doctor or patient. Again, there is a peculiar danger incurred by this class of patients which can never be wholly obviated—namely, the tendency to acute exacerbations of their disease. This tendency, while in a degree common to all chronic diseases, is peculiarly remarkable in the chronic affections of the larynx, and thus the patient may at any time be brought into imminent peril requiring surgical interference.

These remarks have been dictated by two cases which we have just been watching with much interest—the one to an unexpectedly fatal termination, the other to a more favorable issue. I wish now to call your attention to the chief features of these cases, and the more important clinical points connected with the operation performed in both of them.

The first case was that of a man, aged forty-three, recommended to me by a medical gentleman in the country. I saw him in private some time before his admission to the hospital, and prescribed for him; but, as he became worse, he returned to town, by my advice, to have tracheotomy performed. His case was one of long-continued chronic laryngitis ending in ulceration of the glottis and necrosis of the right border of the thyroid cartilage. There was also some œdema of the aryteno-epiglottidean folds. I showed some of you these lesions by means of the laryngoscope before operating, for they could thus be quite clearly diagnosed. The stethoscope, however, gave very unsatisfactory evidence of the state of parts because of the great noise produced in breathing; but it was evident that the glottis never yielded sufficiently, and that very little air was admitted to the lungs. Yet the percussion was good over the greater part of the chest, and the man's general health was reported to be fair.

I therefore determined to open the trachea as the obstruction to the breathing was at the glottis, and could not be, at this stage of the case, relieved within a considerable period of time. To enable the man to breathe during the period of treatment was thus the primary object of the operation. But it had also another object—viz., to give the parts complete rest. For as in ordinary breathing the glottis is constantly moving, so still more it is agitated in forced or difficult respiration. Hence the continual aggravation of the ulcers on its surface and the frequent excitement of œdema in its neighborhood.

In approaching the operation of tracheotomy you have first to consider the question of giving chloroform. Now, in the case before us the anæsthetic seemed to act very badly. The man fainted and ceased to breathe, and the operation was performed on him while in a state of apparent death, from which he was only rescued by artificial respiration performed through the tube. The same thing has happened three times in my experience, and it is very alarming; though I am happy to be able to add that all three cases rallied and recovered. It is very strange that two of them occurred to me in this hospital and during the present session, within a few weeks of each other. Other surgeons have met with the same difficulty more or less frequently, and it becomes us very seriously to consider the cause or causes of the occurrence referred to, and whether or not it should prevent us from employing chloroform during tracheotomy.

I remark here, then, that in some, perhaps in many, of these cases the cause of the syncope is neither entirely due to the chloroform nor to the laryngeal disease. And this is proved by the case under discussion; for when the patient died, a fortnight after the operation, it was discovered that the surfaces of the pericardium were completely adherent. Now you will remember that the man fainted before he became insensible with the chloroform, and that while in the hospital he had another similar faint without chloroform; and, indeed, I believe he died in a third attack, which might not have been fatal had it not happened during the night—for he was found dead in bed. He had never been so well as on the day before: he had been out of bed for the most part of it, and was cheerful and happy. The faints, and finally his death, were caused by the heart disease; and I believe the faint at the time of the operation was but doubtfully connected with the chloroform. Any agitation or excitement might have produced it, or it might have occurred, as the fatal seizure did, without any cause appreciable by us. And so likewise may it be in other cases. I do not mean that adherent pericardium can reasonably be supposed to exist in any considerable number of them; but I do think it not unlikely that causes quite apart from the action of chloroform, or the conditions demanding operative interference, often occasion the difficulty to which I now refer, and that, as most cases recover, these causes are never definitely discovered. I have, indeed, been sometimes tempted to explain this occurrence as caused by the retention of more than the usual quantity of chloroform in the lungs, owing to the partial failure of the expiratory efforts under the anæsthetic influence, combined with the obstruction in the larynx. This is especially likely in cases of so-called œdema glottidis, in which, when the muscles of tongue are paralysed, the œdematous folds at its root and at the sides of the epiglottis are allowed to fall down upon and to fill up the glottis. In this way a more abundant and rapid absorption of the chloroform would be caused, and it might thus effect the heart so as to produce syncope. Of course it may be said that in every administration of chloroform there is some risk of a similar misadvanture; but it is undeniably more frequent in cases requiring tracheotomy than in others. Fortunately, however, it is not, after all, very common; for, out of a considerable number of cases, I have only met with it three times; and I think it need never end fatally if you attend to the precautions I am about to mention to you.

In the first place, be on the watch for its occurrence, and be prepared to combat it. Never, if possible, attempt such an operation without at least one reliable surgical assistant being present to help you. In the next place, be not easily dismayed, and do not for a moment think of desisting from opening the windpipe; on the contrary, proceed, as rapidly as may be, with the operation: introduce the tube and make artificial respiration, taking care that in doing so the tube is not jerked out of the trachea. I beg you not to neglect the practice which you saw me pursue—namely, to pull forward and to jerk the tongue frequently during the period of returning animation. Of course this is not done to admit air to the windpipe, for we suppose that the tube is fairly in the trachea; but I believe it acts as a powerful stimulus to the pharyngeal branches of the pneumogastric nerve, which you know is the chief excitor of respiration. Lastly, persevere long in your efforts at restoration, and, as soon as possible, administer a little brandy-and-water. In one of my three cases it was fully a quarter of an hour before the above-mentioned proceedings could be safely desisted from.

The second of the two cases of tracheotomy which you have seen with me this session was likewise sent to me from the country, and was an example of an acute exacerbation of long-continued laryngeal disease. There were no ulcers visible in this case even on careful examination with the laryngoscope, but only very abundant œdema of the aryteno-epiglottidean folds. Some of you will remember that a year ago this patient was treated by me in Ward 26 for the very same state. The means then used were, free incision of the œdematous masses by means of the laryngeal lancet, and the administration of the iodide of potassium, with the application of iodine liniment over the larynx externally. She at that time recovered; but the present attack was more severe, and, taking into consideration the condition of the glottis beneath the œdema, I recommended tracheotomy as a measure indispensably necessary in order to a more thorough treatment of the larynx.

Now, in your surgical text-books there is a good deal written about the choice of the part of the trachea to be opened in such cases—namely, whether it should be above or below the isthmus of the thyroid body. I do not, however, think that this is much attended to, or much deserving of attention, in practice. I believe that the isthmus is often cut through with perfect impunity, and I am sure that it is seldom seen in the performance of the operation. The great rule which I would impress upon you is, after cutting through the skin and superficial fascia (which you ought to do exactly in the mesian line), to use the edge of the knife as little as possible. Take the back of its point, or even its handle, and thus separate the muscles in front of the trachea, so as to avoid wounding the vessels; and if, after doing this, you perceive the isthmus, push it up with the blunt end of the scalpel and clear the tracheal rings below it. You may then plunge in a small sharp hook to steady the trachea, and open it to the necessary extent to admit the tube. In a few cases you may find the veins very turgid; and they sometimes are torn or cut, and bleed abundantly. It is now pretty well established as a rule in surgery that you ought not to wait for the complete subsidence of this venous hæmorrhage before opening the trachea. Yet it is better to do so if you can, and if there is no very urgent call for haste. You will, in such cases, find acupressure needles very useful, for they may be made to include and restrain bleeding from the entire lip of the wound.

The only other point to which I think it necessary at present to refer is the length of time which a patient may require to wear the tube in the trachea. Now this varies greatly—in my own experience, from a few days to several years. Only a few days ago I withdrew a trachea-tube from a boy's throat four years after I had introduced it for him. At the time of the operation his case was very similar to the one on which I have just been commenting, being an acute exacerbation of a chronic laryngitis. I saw him with Dr. James Stewart, whose patient he was, and by whom he had been carefully treated for a long time previously. Soon after the operation he became strong and active. He attended school, and latterly, with his father's family, resided out of town. When he was brought to me the other day, I found the tube quite filled up with granulations, and the boy had a very fair voice, which was improved on withdrawing the tube. I had some difficulty in persuading the parents to allow the wound to heal, but I am sure the tube was no longer necessary.*

Other examples of retention of the tube in the trachea for periods nearly as long as in the case just mentioned have come within the range of my own observation, and I never knew any bad result from this cause. Notwithstanding, therefore, a contrary experience in a few exceptional cases, I believe you need not fear to retain a properly made silver tube in the windpipe, even for years, if it is required so long.

The patient on whom I last operated will in all probability require to wear the tube for a long time, because of the nature and previous continuance of her disease; in fact, she must wear it until other measures have had time to effect a cure and enable her to breathe freely through the glottis. The progress of the case will be from time to time ascertained by means of the laryngoscope, which is quite invaluable for all purposes of diagnosis in the larynx, and the use of which is quite indispensable to the right treatment of its diseases. But I cannot in this lecture enter upon the various remedies appropriate to such a case as that before us; I may, however, mention that these must be both local and constitutional, having for their object the complete removal of the œdema and the restoration of the indurated valve of the glottis to its natural mobility. The laryngeal lancet and various kinds of inhalations from the spray apparatus should be first employed, and afterwards the topical application of solutions of nitrate of silver, of different strengths, according to the state of the parts. The chief internal remedy on which I rely in such a case is the iodide of potass; and sometimes I prescribe a short course of the iodide of mercury, taken at the same time with the other drug. By such means I hope that our patient will in due time be restored to perfect health.

Original Papers.

OPHTHALMIC NOTELETS.

By J. F. Streatfeild, F.R.C.S.

I. ON THE PRESERVATION OR DESTRUCTION OF THE CONJUNCTIVA IN CASES OF EXTIRPATION OF THE GLOBE.

There is one other, a fourth, class of cases in which the total destruction of the conjunctiva should probably be accomplished for the benefit of the patients who are minus an eye—those, namely, whose conjunctivæ are so peculiarly irritable naturally, or the shape of whose conjunctival sac after the excision operation is so peculiar, that though the artificial eye looks well and can be well retained in place, it, for some unknown reason, is a cause of so much irritation that it cannot be worn very well or continuously, and if an artificial eye can only be worn at intervals it no longer answers the purpose of the counterfeit organ. In the cases I now recall to mind the conjunctival sac is always discharging more or less; if the offensive discharge does not exude from between the lower lid and the glass eye, the deep cup of its concave back is at all events filled with matter of the discharge when the eye is removed at bedtime. (This shape of the back of the artificial eye is an absurd tradition of the times when artifical eyes were put upon old shrunken stumps of accidentally wounded and lost or surgically abscissed and dangerous eyes, and I have tried ineffectually to get the makers to make the concavity much shallower, and so get probably more movement from the deep hard mass of scar-tissue on which the artifical eyes now have to rest, and a broader, blunter edge.) The glass eye being removed the formerly ocular conjunctiva is found to be raised into four œdematous swellings by the cruciform depression of which I have spoken, and perhaps in the centre of it is one of those common little button-like fleshy excrescences (which should be snipped off). The conjunctiva is secreting more or less offensive discharge. Alum lotion and the artificial eye no longer being worn soon cure all this, and yet it will return again and again when the artifical eye is in use. (Of course I am not referring to the cases in which the glass eye is rough at the edges, and a new one is required.) The conjunctiva in such cases as these had better probably be altogether destroyed.

I have heard tell of cases in which, after removal of a lost eye and the wearing of an artificial eye in its place, the other, the sound eye, for the sake of which the excision operation was done, has become, so it is said, *sympathetically irritated, though never so before the operation*, and ever recurrently irritable whenever again it was attempted to begin the use of the artificial eye. I have not myself seen any such cases. I have seen such intolerance of the use of an artificial eye and a similar consequent irritation of the remaining eye, but not, I think, in any case in which the latter had not been irritable before the excision; in such cases as those I have seen I should not now be contented with the omission of the glass eye, ostensibly causing the continuous irritation, but I would also advise the total destruction of the whole conjunctiva by way of also improving the patient's appearance and relieving him of the offensive discharges, &c. And if I ever see a case in which an eye sound and not irritable before the excision of the other becomes, as a consequence of the wearing of an artificial eye, the subject of sympathetic irritation or inflammation, I shall feel it my duty not only to recommend the operation but strenuously to urge its adoption, so that at no time can the conjunctiva of the eye excised be irritated in any degree, or the patient tempted at any time again to wear the dangerous substitute.

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* The wound soon closed up, and the boy remains well.

II.—WHEN AND WHERE TO IRIDECTOMISE IN CASES OF ANTERIOR SYNECHIÆ REQUIRING AN ARTIFICIAL PUPIL OPERATION.

Of all the very numerous iridectomies performed nowadays, a large number are of those done on account of a former iritis as a primary (ophthalmic) affection, leaving extensive synechiæ as results, and, without surgical remedy, the time for belladonna and medical treatment being passed, all their ill-consequences to follow them, and a still larger number of iridectomies, to include those making the first stage of cataract extraction operations, are made up of the cases in which there has been no iritic starting-point for the operation, and the iris, unless secondarily involved in inflammation, has been apparently quite unconcerned. Of the former class, the last annual report of operations at the Moorfields Hospital, for instance, gives 155 (as having been cases of recurrent iritis, or choroido- or kerato-iritis, or simply as synechiæ and closed pupil, or as artificial pupil unexplained), and of the latter 348. Some of either class of cases, no doubt, should be transposed if they had been all explained and classified with a view to my so dividing them, and making consequent deductions, but they are fairly enough treated for my general purpose. In the second class I have added together the iridectomies for Graefe's cataract extraction (165) for *congenital* cataracts, glaucoma, staphyloma, ulcer of the cornea and hypopyon, and other suchlike less frequent cases. A small number of this larger class, no doubt, must include the iridectomies done either for anterior synechiæ or in cases in which there are anterior synechiæ, and, *practically*, in my point of view, it comes to the same thing. They appear, I suppose, in the list under the head of *staphyloma*, for ulcer of the cornea and hypopyon, properly so called, as a present state, only, when not operated on as such, can become, for they are not such cases as those to which I refer, or lead to the operation of which I wish to speak—namely, those in which (either for the immediate purposes of vision, as artificial pupil, or to reduce tension of the globe, or to re-establish the blocked communication between the two aqueous chambers and relieve the distension and compression, *a tergo*, of the iris; all the cases, in fact, in which *anterior* synechiæ are associated with the necessity for an iridectomy. I have myself had several of these cases just lately, so that I am surprised that I can trace so few (only twelve under staphyloma) in the statistical report of 1871 above mentioned, and think some of the cases to which I refer must be sought among some of the other iridectomies not done for any essential (iritis) results.

When there are anterior synechiæ there are corneal opacities, these being very probably the result of loss of substance of the cornea replaced by cicatricial tissue, weaker than any part of the natural circumference of the globe of the eye, or the corneal tissue is weakened by a precedent keratitis, so that a staphyloma (if any bulging, however slight, is so to be called) is commonly a later consequence, and a prominent symptom of the disease for which, in the subdivision of iridectomies of which I speak, the operation is done; but I doubt if they can be all classed under staphyloma in the report, for among the earlier results they would be either ostensibly characterised as leucomata or anterior synechiæ, and so called.

When there are anterior synechiæ and leucomata (an artificial pupil to be made), whether the latter be so soon staphylomatous or not, an iridectomy should be done at a very early period.—In order to the adoption of this rule of early interference by operation, I would remark that I believe the cases are quite apart and unlike the common cases of *posterior* synechiæ in theory and in practice, but they are not commonly so distinguished, and it is a practical point that they should be. As there is no case more urgently needing an iridectomy, and for more reasons, unless it be acute glaucoma, than that of *total* posterior synechia, so, also, a *total* anterior synechia case urgently needs it. In the latter case the lens and other deeper-seated parts are not, as in total posterior synechia, necessarily involved, but there is much the same ever-persistent unremitting distension and compression, *a tergo*, of the iris, leading soon to atrophy of the iris. This is quite enough to require an iridectomy without unnecessary delay in either case of *total* synechia, but in neither can it be done as soon as belladonna has been found to have failed; there must be some waiting for subsidence of the remains of inflammation because of the greater probability of its recurrence if the eye be not "quiet" when the operation is done. Some say three months, some will defer it even nine months, but the time may be advantageously very much reduced in all *anterior* synechiæ cases. After an essential iritis, for posterior synechiæ, one must wait a month or two at least, I think, after subsidence of all redness and other external evidences of inflammation before doing an iridectomy; total posterior synechia, because it is total, is an urgent and sufficient reason for reducing the time of delay to the shortest period the individual surgeon may deem advisable, even willing to run some risk of an abortive attempt to re-establish the communication between the aqueous chambers for the sake of the great end sought to be attained by the operation—the relief, if there be no tension of the globe, of the distension of the iris. But in total anterior synechia the delay may be reduced to a minimum; for the risks of an abortive attempt, or of inducing a recurrence of iritis, are so much less as to be almost nil. I think now I would make an iridectomy in such a case in perhaps a week after all redness in the ciliary region had disappeared. And in *all* anterior synechiæ cases requiring iridectomy, this may be safely, and should be therefore, done much earlier than in posterior synechiæ cases requiring it. And not only more safely at an early time, after subsidence of the ciliary redness, &c., but *more successfully inasmuch as i tis thus done earlier.* I wish to point out what I have noted in reference to this very practical matter, and then to remark on what seem to me to be sufficient theoretical reasons for what I have found to be true in practice; and, finally, to urge an *early* iridectomy in all cases of *anterior* synechiæ requiring operation,. and then if this operation be done for the purpose of vision, as an artificial pupil, I wish to point out that it may and should be done downwards and inwards, or in any direction considered otherwise most favorable to vision in any case of total anterior synechia, and without regard to the favorable or unfavorable situation of the (anterior) synechiæ in any case in which the synechiæ are not total. I call to mind many cases I have had at the Moorfields Opthalmic and at University College Hospitals, more particularly the first case, in which my attention was directed to this point. The patient was a lad with a considerable, recent, not at all staphylomatous, but rather depressed, leucoma, and extensive but not total anterior synechiæ. Without a particular description of these results of the ulcerative inflammation, I may say of the former that, though the active disease had only so lately disappeared in this case, it evidently must leave a dense opacity without hope of its clearing, because of the loss of corneal substance there had been; and of the latter, that belladonna could not detach them, and that they were fewest and the iris seemed altogether least bound to the back surface of the leucoma at the upper part or outwards, I should have liked to make the artificial pupil down and inwards or downwards; and in this case the cornea in the down and inward direction was fairly sound and transparent; but the iris, at its lesser circumference, was here altogether bound by the anterior synechiæ. In an ordinary case, after the usual delay before the iridectomy operation, to have made the iridectomy down and inwards might succeed or not. The better position (as to iridectomy without regard to vision) in this case would have been in any (other) direction in which the iris was freer. Certainly the iris would then only separate in the down and inward direction by tearing it from the cornea; it would then bleed perhaps considerably, and, by a fresh inflammation lit up, or by remains of clot left in the pupil, the result would be more or less unsatisfactory. So that I seemed to have the choice of doing at a future time a successful iridectomy in an unfavorable position for vision, or one not improbably unsuccessful in the right place otherwise. The inflammatory symptoms had only recently but entirely subsided; there was no redness remaining in the ciliary region, nor was the eye any longer very prone to blush on examination. The adhesions were too firm for belladonna—not only too old, but too broad, for belladonna can seldom detach broad adhesions, however recent they may be, and, conversely, can detach sometimes old but narrow (non-elastic) adhesions. To return to my case: I performed the operation *at once, in the position best adapted for an artificial pupil,* and found the pupillary margin come away freely and in perfect shape from the leucoma, without any apparent resistance to the traction of the iris forceps, and without any tearing of the iris or bleeding from the separated parts. I had hoped for, but had hardly expected, so successful a result. In the end there was an excellent artificial pupil; and as, happily, the leucoma has shown—as yet, at least—no tendency to become staphylomatous, and as the patient is now in good health, I have the best reasons to hope the leucoma will not bulge, but will remain in the same position, and in that case the eye, with an excellent artificial pupil in the best situation, will remain as useful as it now is or as it can be.

I need not particularly describe all details of this or of other similar cases, as such particulars are very uninteresting, even if they are considered worth reading by those not immediately concerned, and will give instead of them my conclusions on the subject, and the line of practice into which, in these cases, I intend to enter.

1. When there are anterior synechiæ, and an artificial pupil will be required to be done, if the case is in our hands from the time of the formation of these adhesions, and belladonna has been used abundantly, and has failed to separate them, *do an iridectomy at an early period.*

2. The first condition being accepted, if an artificial pupil for purposes of vision is required, choose any part of the circumference of the cornea that seems to be best (most transparent, or best for any other reason), and there iridectomise without any regard to the position of the anterior synechiæ, *which in this case will detach themselves.*

An objection will be made that, as regards the cornea, the best position cannot be told at so early a period, that much of the clearing of the cornea has yet to take place, that it does not always clear most where it most promises to clear, &c. This will be, no doubt (without the formal old rule of so many months' necessary delay in all cases), to many operators, and in many cases, a reason for too much procrastination. But I think that we can generally form an opinion as to the clearing of the cornea where it will be most, and that about half (the upper and outer quarters) of the circumference of the cornea is, even if clear, very objectionable for visual purposes; and that if the cornea after all does not clear in the right place, there can be no objection then, but great advantage, in doing another iridectomy.

Another probable objection to be met is the fear of a fresh iritis being lit up by so soon operating for artificial pupil iridectomy; but the cases of iridectomy for artificial pupil in which I propose this early operating are not the common cases, but a small subdivision of the same, and must not be mixed up with the greater number. In the iridectomies for artificial pupil—

With posterior synechiæ.	*With anterior synechiæ.*
(*a*) There has been probably an essential precedent iritis of constitutional origin, and, therefore, there is a special tendency to a recurrence of a similar iritis.	(*a*) There has been no iritis, or only a secondarily induced, adventitious, non-specific, non-recurrent, minor iritis.
(*b*) The cut iris-tissue is in contact with, or close upon, the anterior surface of the lens, to which it has been adherent, and with which, by proximity, it is the more likely to become glued by a slight inflammatory effusion.	(*b*) The cut iris-tissue is far away from the posterior surface of the cornea, to which it has been adherent, with which by its remoteness it cannot become reunited by any inflammatory effusion so long as the aqueous humour is retained.

With posterior synechiæ.	*With anterior synechiæ.*
(c) The more contracted the pupil after the operation, the more likely is the iris to become adherent from its greater proximity to the part to which it has been adherent.	(c) The more contracted the pupil after the operation, the less likely is the iris to become adherent, from its greater distance from the part to which it has been adherent.

The aqueous humour, we know, is resecreted in a very short time after it has been let out in an operation, and the oblique opening in the cornea at once retains it if the wound is not so soon united. This brief period of time that elapses before the aqueous humour is resecreted and retained is of comparatively small importance in cases of operations for posterior synechiæ, the iris and *lens*, under all circumstances, are so close together; but a deep and full anterior aqueous chamber is the great advantage we have to reckon upon and to make the most of in the anterior synechiæ operation cases with which I am now concerned. That there has been no precedent iritis, and, as a consequence, none to recur, is the other great advantage. Synechiæ of any kind, in some proportion to their extent, are ominous of evil in any case whatever. It is a very important duty of an ophthalmic surgeon to prevent, mitigate, and detach these adhesions whenever he can. The operation of iriddesis is therefore and inevitably becoming obsolete. The anterior synechiæ, as compared with those behind the iris, are comparatively unimportant, they are also much less common; but when, for tension of the globe, distension of the iris (total synechia), or for artificial pupil, an iridectomy is done in these cases, it may be safely, and therefore it should be, done early, not only by way of lessening the time lost in delay or to lessen the chances of ill results occurring as a consequence of them, but because the operation can then be done with better success. The whole width of the iris, from its greater to its lesser circumference, can then be removed; and at a later time its pupillary margin and a greater or less portion of its width therefrom will be closely attached—too firmly consolidated to the corneal cicatricial tissue to separate itself when it is attempted to be withdrawn, but instead of this it will tear, and the inner portion will be left *in situ* on the back surface of the cornea or leucoma, an irremediable opacity without regard to the major and inevitable opacity of the cornea itself or its substitute. Posterior synechiæ in any one case, when plural, are often of various dates; but anterior synechiæ are all of one period—all occurred at some one time when the aqueous was evacuated, and the iris fell forwards towards the aperture for a sufficient, brief, time to become adherent to it or to the back surface of the cornea with which it is brought into contact; so that in all probability any part of any anterior synechia can be, for this matter, as easily separated as any other part, and there can be therefore nothing to choose—no part easily or with difficulty to be detached; and, in point of fact, they are easily detached in iridectomy operations when the operation is done sufficiently early. And the particular character, position, and extent of these corneal adhesions of the iris need not, and indeed ought not, then to influence us in determining upon the place of the iridectomy. We have only to consider the leucoma as it now is, and as it will be in the future probably, and the ordinary rules of surgery in choosing the position of an artifical pupil. For my part, I should prefer to operate down and inwards, and to remove the portion of iris as near to the centre of the cornea as may be. The anterior synechiæ are non-existent for practical purposes in these early operations, and offer no resistance.

The common posterior synechiæ may also probably no doubt freely detach themselves in doing an early iridectomy; but besides the greater proclivity to iritis, a recurrent iritis, in the nature of these cases *ab initio*, synechiæ are, it seems to me, generally more closely and broadly and firmly united to the lens with which the iris is so much more nearly in relation, or in actual apposition at all times, than with the more distant cornea with which the iris has been casually for a short time only apposed, that the iridectomies for posterior synechiæ must be necessarily longer deferred, or a second iritis and readhesions and a failure of the operation will be the only result.

A *slight* attack of iritis after an essential or specific iritis and an iridectomy, and the iris imperfectly dilated as it then, after an operation, will be only by the use of belladonna, is very much more likely to cause readhesions and a failure of the operation than is any degree of iritis in anterior synechiæ cases, for the several reasons I have given above. A slight iritis after an iridectomy will have no effect to produce readhesions in anterior synechiæ cases, though they be, as I now beg to propose, operated on at an early period, in whatever position, irrespective of the adhesions, may seem to be most conducive to good vision, or may be otherwise most desirable.

In the practical suggestions I have put forth on this subject I have omitted to make a note of exception which had long ago been placed in the memoranda of my note-book, concerning the detachment of anterior synechiæ in iridectomy. If the iridectomy be done at an early period, when the inflammation has subsided and the adhesions in front are quite recent, the iridectomy may be done as well or better at any part of the circumference of the cornea (downwards and inwards?) that may seem to be best, without any regard to the corneal iris adhesions, which will freely separate themselves and cannot re-form subsequently. In an ordinary long-deferred iridectomy, where there are anterior synechiæ, and the iris is to be removed at a part where these exist, it is torn through its substance, and the part external to the tear is drawn out and excised. It probably bleeds very much, and the more central portion of the iris at the part chosen for the excision is left. Then there are greater probabilities of continuous bleeding from the iris or of a fresh inflammation of it. When, in these cases of adhesions of the iris in front, the operation is done early, and, if necessary, at the part where the adhesions are, the concave pupillary margin reappears in proper form, when and where the traction of the iris forceps is applied, and the iridectomy can extend up to this inner circumference, and an entire segment be removed and some at least of the hateful iris-adhesions are abolished.

But in the proper class of cases for these early iridectomies at any part of the circle—a class which is neither the commonest nor very uncommon,—I would not be misunderstood to mean that all cases in which anterior synechiæ form may be taken in this class and be thus successfully treated by operation. There are some few in which, though operated on early, the iris will only tear from the cornea; and the position of these adhesions in these cases cannot be disregarded, but must be considered beforehand, as they have been by operators in the old way of deferred iridectomies for anterior synechiæ; indeed, when the cases can be diagnosed before the operation is begun, they may be deferred as all have been hitherto.

Where leucoma and anterior synechia follow penetrating wounds or perforating ulcers of the cornea not quite in the centre, and, the aqueous humour being let out, the iris of course falls forward, and not only by apposition then with the back of the cornea becomes glued there, but, being opposite the external opening, it prolapses and becomes impacted or strangulated, as in old iriddesis operations, the iris will then only tear, and cannnot detach itself in an iridectomy, which may therefore be deferred. Sometimes these cases are certainly to be diagnosed by the appearance in the white leucoma of a small dark-brown patch of uvea there fixed. The iris cannot then be separated as a whole. If we were to suppose a window-curtain to be in part accidentally glued to the shutters, separation might be effected without tearing by drawing the curtain aside; but if also shut in between the closed shutters, the tearing of the curtain must inevitably attend the act of separation. The simile is not perfect, or we must suppose the curtain affixed to the shutter would separate as a perfect whole only before the glue was dry.

III. ON THE ADVANTAGES OF USING TWO SHARP HOOKS WHEN IT IS NECESSARY TO EMPLOY TRACTION IN CATARACT OPERATIONS.

There seems to me to be an increasing number of examples of the surgical instances in which it is found that two similar instruments are better in use than one. If an operator had many hands, instead of only two, it would be, no doubt, of great advantage to him. Some monkeys seem to have, practically, four (or five); but if we want to employ more than two hands we must engage an assistant for a third (or third and fourth) hand, and to him is entrusted the least important parts of the work to be done. Much more may be accomplished by determination and practice with our two hands than is usually done; and an assistant is, at best, a necessary evil in an operation, if he is indispensable. It does not at all interfere with the small movements required of our fingers in an eye-operation if the various little instruments are placed one in each of the interspaces of the first phalanges of the fingers of the left (or right) hand; three at least can be so held, besides the forceps to steady the eye, which is committed to the thumb and forefinger of the same hand; the knife in the right (or left) hand. When each instrument has been used it is laid down, and we have not to wait an instant for the next that is wanted, or to arrange to hold it differently from the way in which it was given into our hand by the assistant. For my part, I frequently, in order to be independent, hold in my right (or left) hand, as well as in the other, more than one instrument, of those that are not sharp or cutting, between the palm of the hand and the ring and little fingers of the same. These remarks and suggestions would, I presume, hardly ever apply in general surgery, with larger instruments and more considerable movements of the hands and fingers in the operations. In eye-operations it is almost always best to be behind the head of the patient, lying on a high couch, with his feet in a well-lit window; and in operating, as in an ophthalmoscopic examination, it is best to stand, for then our movements are more freely made, not only from side to side, but by raising or lowering the head, so that the eye of the patient may be viewed in all directions and in various lights, as may be necessary, and this without approaching to or receding from the subject of the operation. Most eye surgeons prefer to sit down when operating or in an ophthalmoscopic examination; and for this purpose, in operating theatres and ophthalmoscopic rooms, at the hospitals at least, music-stools are provided: in operations, by way of getting, in some degree, the use of a third hand, I generally screw up this stool to a convenient height, and put my left (or right) foot on it, and, the patient's head being covered with a towel, I rest it against my knee to steady it; the wrist of the hand with which I am operating being also engaged in the operation on the other side, to aid in preventing any little unexpected movements of the head of the patient. I do not know that I could make any greater use of my own abilities in order to acquire a complete independence. Two fingers of the left (or right) hand are all-sufficient to hold open, and keep apart, the lids, and at the same time to steady the eye in any operation in which no considerable opening is to be made in it, as in cataract-solution operations, when one needle only, in the right (or left) hand is to be used. But, if an incision is to be made in the coats of the eye, the pressure required to be made upon them by the two fingers, which are placed actually on the eye, on the ocular conjunctiva and the margin of each of the two separated lids, would be too much for safety, and then a spring speculum and fixing forceps must be submitted to. If the middle and ring fingers are thus used, their soft ends are put between the lids and upon their margins and then separated, and with them the globe of the eye is pressed sufficiently backwards to secure its not moving, the forefinger and thumb of that hand being still available. When an eye is fixed thus with two fingers, it is much more securely fixed than by means of any ordinary fixing forceps, which holds the eye at one point only of the mucous membrane, and, as this coat is loosely attached and easily torn, the instrument will only ineffectually prevent some of the movements effected by the rectus muscles, much less will it restrain the rotatory movements of the obliqui. Impressed with this defect of our fixing forceps, wishing to have the eye to be operated on more completely under my command, and considering that only *two* fixing forceps could effect my purpose, I, some years ago (Ophthalmic Hospital Reports, iv., 100), had forceps made which control *all* the movements of the eye completely; each blade is bifurcated with a U-shaped extremity, the width apart of the two ends of either blade being equal to one and a half or two diameters of an ordinary cornea, so that with it the eye is secured at *two* opposite and far apart points, by holding with it the conjunctiva and subconjunctival tissues at the customary most convenient distance from the margin of the cornea. These double ends (for the two blades of any forceps only supplement one another) are equivalent to *two* similar instruments.

But the forceps must be carefully regulated in use, that the two ends be neither dragged upon nor depressed unequally; but, as we have no more than two hands, it is useful that the forceps may only employ one of them. The double hook used in general surgery in excising tumours is preferred to two hooks in separate handles for the same reason and for other reasons, one of them being that there is less chance of the double hook tearing out, as the force of the traction is halved, or unequally divided (as it is in the case of my double-ended forceps). When two separate and duplicate instruments can be used together, one in each hand, it is often very advantageous; it is no longer a question of economy of hand-power, but traction is halved, or any other specific action of the two instruments is divided between them, and the action of either of them can be, and should be, always made supplementary to, and regulatory of, the action of the other, its opponent or coadjutor. With one hand one instrument "plays into the hand" of the other. When the best of all instruments only are used, "two are better than one." In the final stage of enuncleation *two* fingers are better than forceps, with which to move, or fix, and guide, and retain the eye-ball —better even than more than two fingers, which are unnecessary, and probably in the way. *Two* hands are similarly useful in removing very large tumours.

Two opposite iridectomies are often advantageous when one is sufficient; so that, in these latter times, two are sometimes made at once, opposite each other, in the same eye, at the same time of operating; sometimes even the corneal openings are made simultaneously with *two* broad needles, one in each hand, to avoid the escape of the aqueous humour by the first-made opening, when they are made separately, with an interval, however brief it may be, of delay. I am not now concerned in the consideration of the advantages of double iridectomies or other double or single (e.g., squint) operations, but only in the advantages of using two similar instruments (in one or two handles) in some cases in which the objects of the operation, and how it is to be done, have been long ago determined.

There is a little-used, old-fashioned instrument in cataract instrument cases which has held its place in all the vicissitudes to which extraction operations have been subject of late years; for it is supposed to be sometimes indespensable, although as it is generally employed (too late to be of use) it might as well be absent. I mean the "sharp hook." *It is really a useful instrument; but we ought always to have not one but two of them for use together, and begin to employ them soon enough in the (unpromising) operation cases in which the sharp hook or some other traction instrument has been hitherto made use of as a last resource.* When I say *soon enough,* I am embarrassed by the conviction that I cannot quite define when the need for the instrument (instruments) has arisen. In mere operating practice is very much, and we learn to appreciate symptoms without bringing our minds even to the effort to describe them. Still, I think I can make this clear: when the need for a sharp hook (or of two sharp hooks) has arisen, the two sharp hooks are infinitely better, not only than one, but also than any spoon or scoop, or any other of the traction instruments, which are still used in these cases of emergency by all the best operators, although the practice of "out-scooping" cataracts has been long abjured by them.

The common sharp hook is as fine as may be to have sufficient strength and stiffness, though not much of either is required: it tapers, very gradually, from its proximal end in the handle to the very sharp point of the end hook. This hook is about one-twentieth of an inch, and somewhat C-shaped; but the hook does not form more then one-third of a circle altogether. It was never much used, but its occasional use has never been abandoned. It was called for when the lens, in extraction of cataract, receded into the vitreous chamber instead of coming forwards in the regular way. In flap-extraction this recession, when it occurred, was as rapid—often instantaneous—as was the advance of the lens in a successful course. If it descended it was lost to view; and yet the operator, armed with the sharp hook, would seek for it, and then he abandoned the attempt, and there the lens was left and the eye closed. When a suicide has thrown himself into the water we do not "drag" the river with but one hook. The operator may sometimes have succeeded in hooking the lens, more probably the retina, but of course he would not so be able to hold it. As I do not now operate by the flap-extraction, I need not determined exactly what I should do in it; but if I could not see the lens I think I would not seek for it, and if I could see it I would not attempt to hold and extract it with one hook only.

To proceed to von Graefe's extraction and my own experience. In the various stages of this operation, and under anæsthetic influence, there is no such need for hurry nor any such cause for sudden alarm: all is much more leisurely and safe. The lens advances slowly and gradually, gets a little more than half outside the corneal opening (and then it cannot recede); or it does *not* advance but a very little way, or not so far at least as to be half outside the corneal opening, and then, soon, pressure being still kept up, it will recede, and the vitreous advance. If the lens do not advance (and the corneal opening does not require enlargement) or begins to recede, or advances a little and then begins to recede, I take two of the sharp hooks, one in each hand, and, inserting one and then the other in the corneal opening, with the convexity of the hook downwards, pass them, held in the same way, both some distance directly back into the vitreous chamber and behind the lens till both are considerably past the middle of its back surface; then a slight rotation in the right direction (the necessary amount—a quarter—to be observed and calculated before the hooks are introduced, for their ends cannot now be seen) of the two instruments, one in either hand, brings them towards the lens. Their hooked extremities, being brought somewhat forward and then in a direction as if to withdraw them from the eye, are securely caught in the lens at its lower third below the centre. From the moment when the lens is first caught in this way by one or other of the hooks, as at the same time a movement to withdraw them, and the lens of course with them, is made, the lens will rotate a little and incline to the right or left, and then we begin to appreciate the advantage of having two hooks, each one to aid the other: the lens, meeting with some slight resistance to its being withdrawn and removed from the eye, would inevitably fall off on one side or the other of the single hook, and the result would be worse than *nil*, for not only would the lens be lost, but it would be more or less comminuted, and so become more difficult of removal. Of course the two hooks may get a good anchorage in the body of the lens at the same instant, and this regulatory movement with the second hook may not be required. We have, in the next place, only slowly and very observantly, to withdraw the two hooks equally together, and the lens on them; withdrawing a little more, comparatively, the one or the other of the hooks accordingly as one observes the lens to make a slight beginning of a rotation towards that hook, so as to stop that rotation and make the lens advance straightforwardly to its complete extraction. The patient, meanwhile, is kept profoundly under the influence of the anæsthetic, and if the eye is inclined to roll upwards, it is held down by fixing-forceps committed to the hands of an assistant.

Thus much concerning what I have called the "regulatory" action of the second (either) sharp hook. As to the "supplementary" action of the same I would explain it by a reference to a case in which I failed for want of it—i.e., a second hook. It was some years ago, but I remember it very well because of the extraordinary end of the case. It was at University College Hospital, and I had not a second sharp hook, because one is now generally, and was then universally, supposed to be all-sufficient. At this hospital we have all necessary instruments, all that can be required—a complete set; but at Moorfields, where I first used a *second* sharp hook, I could have it without ordering it, because, with so large an ophthalmic staff, and so many instruments constantly under repair, there are duplicates of all kinds. In the case at University College Hospital, all had gone well at the beginning of the operation; the cataract was mature, there were no unfavorable symptoms, the globe was not soft, it was an ordinary senile cataract, commencing to form also in the other eye, and there were no posterior synechiæ, etc. But the lens, I do not know why, would not come forward but a very little, and soon began to go back, until the lower half of it was hidden behind the lower part of the iris. Some vitreous humour escaped, and the lens seemed inclined to descend lower and out of sight. I took the sharp hook, put it behind the lens in the vitreous chamber, and secured it and brought it forwards and upwards at last; then I essayed to bring it out, but it rotated and swayed from side to side, comminuting the back surface of the lens and detaching fragments which must be left behind. In order to make sure of not losing the lens altogether I had to keep the single sharp hook very firmly pressed into the lens, and, just as I thought I could extract it as a whole, it broke into two, and I brought out one half which was in front of the hook. (This would not have been so, I believe, if the pressure and traction had not been necessarily so great, and all at *one* place.) The other sank low down in the pupil, and another gush of the vitreous humour occurred. I was now afraid to attempt the removal of the second half of the lens, for it had become so much involved in the vitreous humour, and I did not think I could hold it with the (one only) instrument of whose powers, in my hands, I had just been making trial. So I was glad to close and bandage the eyelids. The extraordinary end of the case is, that the man gained at last, and has at the present time, a perfect eye as regards vision. The half of the cataract, and the fragments also remaining in the eye, involved in the vitreous humour, rose towards the corneal opening and left a considerable space below them, and above the iris, of free pupil. In this situation they were found when I examined the eye next after the operation (the corneal opening was then already united and healed) and so they have remained ever since, only somewhat shrunken in the course of time. I think I could have removed the lens whole if I had had two sharp hooks to divide the pressure and dragging force between them, to prevent the sideways rotation and swaying to and fro of the lens, and the comminution and final split into two. All this a second sharp hook would have done for me, had I known it then and provided myself with a second hook. So would a spoon or scoop, some will say; but I will not omit to speak of the *comparative* advantages of the two hooks.

The two fine hooks occupy no appreciable amount of space in the corneal opening; they do not comminute the lens as one hook does, they transfix and hold the lens (it does not merely lie on them as a lens lies in the spoon or scoop which is used by most operators on these occasions), so that it can be brought out straightforwardly without need (or fear) of compressing it against the back of the cornea, *and some part of the iris, perhaps between them.* It is as different in practice as it is to land a fish with a gaff rather than to secure him only in a landing-net; the sense of safety and power is so very unlike in the two cases; it is a triumph even *before* the complete extraction of the lens which had threatened to escape, and a feeling very unlike that afforded by the partial security of a single hook or the danger of a scoop retaining the lens. Some soft cortical lens matter is almost certain to be left to be removed after the main body, nucleus, &c., of the lens is extracted; but this is commonly the case in all Graefe's extraction-cases, and the main body being out of the way, this soft lens matter may be extruded with a curette, as in other cases of extraction (or, at least, it should be attempted) in which there has been no mishap, or any fear of one. The rotatory tendency in a lateral direction, of which I have said so much in order to its prevention by using the two sharp hooks instead of one only, and by drawing one or the other of them a little more or less during the extraction—this very rotation which I have tried to avoid by all means, may be utilised in the final stage of the extraction when the lens is almost loose from the eye, by watching the opportunity of any inclination that it may have to rotation, and then turning it out so, sideways.

The advantages I have found in the use of the two sharp hooks I have described, in the cases in which, with a sufficiently large corneal opening, the lens will not advance, or having advanced a little way will not proceed through the opening, but seems rather inclined or has begun to recede, and the vitreous humour instead of it inclined to advance, are—that an extraordinary amount of power is obtained, so that the lens cannot but be extracted; that it may be so well guided by a little more traction on one or other of the hooks; that by them it can be drawn straight out through the external opening without any harmful dependence and pressure upon parts opposite the traction instruments—the cornea and iris, against which the lens is often thrust violently in extracting by means of a spoon or scoop.

In a case recently at Moorfields, in which I operated for cataract in my usual way, von Graefe's method with my modifications—for every one now adopting this method of extraction has some particular fancies in which he allows himself to depart from the exact rules of the master,—all had gone well, and the corneal section was of fair size, but the lens after a little advance stopped, and beside it at one angle of the wound a prominent bead-like protuberance of the vitreous humour showed itself. I of course remitted all pressure with the curette; the vitreous protuberance remained. With the two hooks I drew the lens straight out through the opening in the way I have described; the vitreous humour retired altogether, and none of it was lost. This is almost the only case I know in which by any method when some vitreous humour has protruded so decidedly in an extraction-operation, the lens has been then afterwards extracted without loss of any portion of the vitreous humour. But if, as in most of my cases in which in alarm I have used the two hooks, some small portion of the vitreous humour is lost, and the lens has been extracted before or afterwards, it is very much better and more certainly done, I believe, with the two very fine little sharp hooks than with a single larger instrument, and probably much more disturbance and compression of the neighboring parts, besides the other disadvantages of a larger instrument and of one only.

IV. ON THE USE OF A FORCIBLE JET OF COLD WATER SPRAY IN CASES OF PHLYCTENULAR OR OTHER OPHTHALMIA WITH MUCH INTOLERANCE OF LIGHT.

Some few years ago a new method of treating the cases of ophthalmia in children with much intolerance of light was introduced into this country from Germany. The treatment was, I believe, von Graefe's. It has been very successful whenever I have tried it; but there are many objections to its employment. A large basin is nearly filled with cold water, and an attendant takes the refractory child round the waist, with its arms beside it, under his right arm. The child's head is then brought down, and held under water by the left hand of the attendant for a quarter of a minute or more. If then the child will not, or cannot open its eyes, it is again inverted, and the head immersed. And, as I have said, it hardly ever fails soon to effect a cure. But the child, and the attendant also very probably, get very wet; and the mother of the child, and the child also I suppose, think it will be drowned. And that the cold water should be applied even to the whole face and head of the patient does not seem to be necessary.

When the grand new system of baths in the skin department of University College Hospital was started by Dr. Tilbury Fox, I resolved to ascertain for myself, with such facilities as I then had close at hand, if the cold-water treatment of these eye cases might not practically be improved. There were to be, amongst others, douche-baths of all kinds—descending, ascending, and horizontal jets. I began to make use of the latter as soon as the baths were completed; and I have not been able since that time to devise any practical improvement on the system of treatment thus inaugurated. It was only necessary to provide an ample little waterproof cloak, which opens behind, and is tied closely around the neck of the little patient. The bath attendant holds the child up in his hands in the current of cold water spray, and facing it, as as soon as the water is turned on. The cloak in which the child is enveloped and thus held is sufficient to keep its hands down, and the spray current is so large that it cannot, by turning its face aside, get out of the way of the jet. The power of the application may be very readily increased or diminished by bringing the child nearer to, or removing it further from, the rose from which the spray current is given off. This rose is attached to a pipe in the wall of the apartment. It is perforated with many very small holes close together, and in a direction perpendicular to the surface, so that the separate jets of which the long column of spray is composed are nearly parallel, and but little divergent. The pipes are supplied by the mains of the water-works, and, the baths being on the basement floor of the hospital, the force of the water is greater from this cause also. The pipe to which the rose is attached has a tap close at hand, by which the water is turned on and off, as it is required. The whole apartment is cemented, and the floor is drained.

With a little more time and thought, the remedy may be employed in any house supplied with water derived from taps and fed by a cistern. It will only be essentially necessary to have an india-rubber tube with one end tied over the mouth of the tap and the rose at the other end of it. The force of the water will of course be most in the lower rooms.

This method which I am now advocating I have found to be more rapidly and certainly successful than the old way of inverting the child and immersing its face and head. And if I am right in this, I presume it must be attributed to the mechanical force of the spray jet; for in either case we have the cooling effects of the cold water, which are very valuable, as the good results of the immersion system have, I think, proved to us. Probably, in such hot weather as we have had recently, the remedial effects of the douche would be greater if the temperature of the water were to be artificially lowered. The force of the water may be regulated as required in any case, and suddenly or gradually increased or lessened in the way I have pointed out or in any other way. I believe that, with quite cold water and sufficient force, no child, however intolerant of light and obstinate, can resist the spray-current. It is not only that it can or will then open its eyes, but it both can and will do so. The effects are, no doubt, both physical and moral.

The face of the child is directed towards the jet of water and held in the spray-current for a minute or two, once and again, as may be necessary, till the child opens its eyes. Then they can be examined, and any other treatment proper for the case be prescribed accordingly. The *immediate* effect of this cold-water treatment, when it is first employed, is that the child struggles and tries to turn its head away and escape, then its screams are checked by the flow of the water-spray into its mouth, or by astonishment and curiosity, but even more, I think, by subsidence of fear and the gratefulness of the local application. Perhaps the child cannot at once open its eyes, or will not make the effort, but the power is soon attained, and it is of a different mind. After two or three applications of the remedy it has never failed in my hands. In the next place it seems to act as a sedative and to allay the terrible irritability of the patients who require this treatment. The *remote* effects of the remedy are seen in the second and subsequent visits. The child tries at least to open its eyes, and generally it can do so; but if not, the cold water spray is employed again. The mother is generally much pleased and grateful. The child is less fractious, and in all probability can open the eyes at least sufficiently to allow an examination without manual force. It is not only for fear of the surgeon and the douche that the so-called "naughty" children will open their eyes at the second visit, but they have before this, become more tractable and "good," as the mother say, in the absence of any such fear. They have acquired a new power. The eyes are much less injected, the flow of tears is checked, and the cure progresses steadily from this time forward. There is no more of the violent spasmodic action of the orbicularis palpebrarum, and the eye itself is less and less irritable and inflamed. Any other local remedies employed for the ophthalmia, and to be applied to the conjunctiva, are very ineffectual so long as the photophobia exists especially because of the extreme difficulty of their introduction between the lids and the flow of tears which immediately follows any attempt of the kind, or even, in some cases, the suggestion of an attempt. The intolerance of light must first be conquered. It is absurd to prescribe for the local inflammation in the first place, because it is in some degree at least due to the photophobia and the concomitant lachrymation and spasmodic contraction of the lids. 1st. Treat the photophobia with cold water and air, and such light as can be borne; (*a*) give cod-liver oil and syrup of the iodide of iron or steel wine or other tonics; (*b*) give a belladonna or atropine lotion. 2nd. At a later stage, if the ophthalmia be chronic, add an astringent to the lotion given (sulphate of atropine and sulphate of zinc, a grain or two of each to the ounce of water is a good formula). The congestion of the lids, the palpebral and ocular conjunctivæ, and, with it, the watery or other discharge, become manifestly less when the cold water alone has been employed, after a first application, and so also each time afterwards, with or without other remedies; the congestion is lessened, and as it is less so are also the photophobia and lachrymation equally diminished. But to effect a cure the intolerance of light must be overcome, and at any rate must not be subordinated to the other symptoms. The ophthalmia is often very slight when the photophobia is excessive, but the former cannot be quite cured whilst the latter is in force. The photophobia no doubt is but a symptom in itself, but it is productive of many evils, and so it must be combated. I think it probable, however, that not only indirectly does the cold water benefit the ophthalmia, but that it acts also as a local astringent, and in other less important ways. It promotes cleanliness, and the direct cooling effects of the douche as I have employed it are no doubt important in more ways than one. I do not know if, in the cases in which this treatment does so much good, the temperature as shown by the thermometer within the lids is above the average, but it seems probable that it is so. I mean to make the experiment; and I mean to try the effects of a fine single forcible jet of very cold water applied directly to the surface of inflamed conjunctiva between the lids, held open for the purpose, in cases of ophthalmia in which there is no intolerance of light. I think it may be of much use, in the same way that eye lotions are of use, in some of the superficial eye inflammations. I would not try it in any of the cases of redness (congestions or inflammations) of the eyeball. I mean also to try the effect of my cold water spray-current in some of the cases of hysterical girls who have slight ophthalmia of any kind, and much photophobia with it, if I can persuade them to make a trial of the remedy. Like children, they want the resolution to try to open the eyes, but it will be a less effort to them to resolve to try the effect of the cold water applied to the closed eyelids. The children and the hysterical girls, too, have been probably secluded as much as possible from light and air. If at an early period of their diseases they had been encouraged as much as possible to expose their eyes to cold air, instead of having had them carefully secluded from it by bandages, and shaded from the light in the same way, or by eye-shades or darkened rooms, &c., the cold air, without the more powerful cold water douche, would have been very beneficial, and would alone perhaps have effected their cure. The aggravated cases of photophobia we so constantly see in convalescence from the exanthemata, have generally been fostered by erroneous treatment of the medical practitioners, and misplaced kindness of the patient's relations and attendants. In these naturally or artificially aggravated cases the strong current of cold water spray will be found to be, I think, a most valuable remedy in addition to general constitutional treatment in a class of cases hitherto very obstinate, and in which any local remedies that have been also employed have been, from the very nature of the cases, most inefficiently applicable. The cold douche makes a powerful and sudden local impression, the shock of which is found to be sufficient to enable the children, who are the chief subjects of this persistent and intractable photophobia, to open their eyes. Without it they have no power of resolution to begin and try to bear even the least degree of light. The moral power is wanting, any degree of light is painful, and no degree of pain will be willingly endured by them for an end they cannot be supposed to appreciate or to anticipate for themselves.

Finally, in comparison with von Graefe's method of treating these cases, mine is, I think, better, in that (1) it is more local in its application; (2) it may be graduated in its use; (3) no unnecessary fear is excited in the patient or in the mother, as it is when the child is held head downwards in actual water; (4) the attendant is not splashed with water, and the child's body is kept dry; (5) it is more easily employed when the means are at hand; (6) and more effectual. For the *force* of the jet of cold water spray is of the essence of the new mode of applying the old remedy.

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CASES OF

SMALL-POX TREATED BY THE HYPODERMIC INJECTION OF VACCINE LYMPH.

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In The Lancet of August, Mr. Furley, of Edinburgh brings under the notice of the profession the alleged curative action of vaccine lymph in small-pox. My attention had been previously directed to letters sent by him to a daily newspaper in Edinburgh, in which ordinary vaccination was lauded as a specific and certain remedy for variola. Hoping fervently that Mr. Furley was in the right, and that it would be proved that my ideas about the want of any influence of cow-pox over small-pox when concurrent, unless induced before the sixth day of the period of incubation of the latter, although derived from the observation of a considerable number of cases, would turn out to be erroneous, this plan of treatment was tried on six of our patients. Three of the six died, and in the others the disease followed its usual course, uninfluenced to any noticeable extent by the vaccination. These were not very successful results, but, as with one exception the subjects upon whom the treatment was tried were adults, they agree with Mr. Furley's, for he now says that "it (vaccination) is almost inoperative in adults." Believing, apparently, that the vaccine poison is a true antidote to the variolous, and the quantity required to be introduced into the system to neutralise the disease is larger than that used in the ordinary operation of vaccination, he now recommends the hypodermic injection of the lymph. Upon this theory nothing need be said, although it seems to me that it is by their great similarity, if not virtual identity, that the vaccine acts upon the variolous disease, and that if Mr. Furley is right in his deductions, in this instance at least the dogma of homœopaths is correct, *similia similibus curantur*. But putting theory on one side in order to give the practice a full and fair test, seven patients were operated upon after Mr. Furley's method. In this communication I propose giving the details of the cases so treated as entered in the hospital case books by Mr. Bland, assistant medical officer, appending only such remarks as may be necessary to convey the impressions received while watching the daily progress of the patients.

One point which Mr. Furley's paper brings out forcibly, and it is of very general application and interest, is this: that the deductions either from statistics of mortality or from the results of any particular line of treatment in small-pox are sure to be fallacious unless the age and particular state as to primary vaccination of every patient under notice be considered. In Mr. Furley's instance, he had had by his own statement five deaths in sixty cases. It entirely depends on the conditions mentioned above whether this is to be considered a low rate of mortality or not. If all his patients were unvaccinated it is assuredly a very low one; but if they were all between the fifth and twentieth years of age, and had four or more good marks of primary vaccination, then it is a high rate, especially for private practice. These suppositions are both extreme, but in the absence of the particulars required for making a correct estimate, any opinion formed as to the success or non-success of Mr. Furley's treatment is pure conjecture. Another difficulty in forming a correct judgment as to effects of remedies in small-pox in the *vaccinated* subjects is the impossibility of telling at the earlier periods of the attack, say up to the sixth day, what will be the exact course of the disease even if left to itself. I have often seen patients who on the fourth and fifth days of the disease had a temperature of over 104°, whose faces were swollen, and of a general bright-crimson hue, and who were delirious at night, and on the eighth or ninth day they felt well, their temperature was normal, and a few hard scales was all that remained of the eruption that promised to be so severe; and this without treatment of any kind. In the *unvaccinated* class it is different: the disease may not always take the confluent form, nay, sometimes it is quite discrete, but it is very rarely indeed that the rash aborts; however sparse it may be it reaches the pustular stage, and the pustules are full-sized and umbilicated. In my experience, in all vaccinated cases the disease is modified, even when it proves fatal, and the proportion of instances is very large indeed in which it runs a highly modified and more or less abortive course. In the Hampstead Hospital the mortality in this epidemic in the vaccinated of all classes has been 11·40 per cent.; it is found that in about 12 per cent. the disease is severe, either confluent or semi-confluent, leaving over 75 per cent. for very mild and aborting cases. One can therefore imagine it possible for a practitioner to have in succession fifty or sixty mild cases, especially in a community said to be so well protected by vaccination as that of Edinburgh is.

The method followed in injecting the lymph was strictly as described by Mr. Furley, through a grooved needle widened at one extremity (made for me by Messrs. Arnold and Son). The contents of two tubes of vaccine lymph were blown into the subcutaneous tissue on the outer side of the arm, near the usual place of vaccination. The lymph used was taken from a primary vaccination in a healthy child on the eighth day of the pock, and was only five days old.

Case 1.—J. S——, male, aged twenty-five, clock-maker, admitted June 2nd. Two vaccination marks. Five days ill. Rash vesicular, small, and very copious; semi-confluent on face; the areola well defined.

June 3rd.—Temperature 102·8°; pulse 120. Eyes suffused; coughs a little; bowels open; takes food fairly; had a quiet night; tongue furred, red at tip. Large crepitation distinct over both lungs posteriorly. Expectoration frothy and very profuse. Respiration 24. Vaccine lymph injected (in presence of representative of The Lancet).—9 P.M.: Temperature 101·4°; pulse 104; rash the same as this morning, but fuller.

4th.—Eruption on face vesicular, small in size, very copious, and semi-confluent; on trunk and limbs also vesicular, small in size, but less copious and discrete. Lips swollen; eyes watery; tongue slightly furred and moist; cough less, and expectoration slight. Muttering delirium all night; no sleep. Bowels confined. Swelling and tenderness at seat of injection. Pulse 104; temperature 101·6°; respiration 30.—9 P.M.: Temperature 100·4°.

5th.—Eruption matured, and in some places exudation taking place from pustules on face; on trunk and limbs still vesicular. Swelling of upper lip increased; some general swelling of face; lachrymation; great tenderness of skin; tongue dry and glazed. Wandering delirium during the night; very little sleep. Eyes suffused, and conjunctivæ injected. Redness and tenderness at seat of inoculation. Respiration loud and cooing, 30. Expectoration less. Pulse 112; temperature 101·8°. —9 P.M.: Temperature 100·6°.

6th.—Eruption beginning to dry on face, pustular on limbs and trunk; swelling of face slightly less. Slept well; no delirium. Tongue dry; eyes suffused, and conjunctivæ much injected; bowels open. Takes food well. Still slight swelling, tenderness, and redness at seat of inoculation. Temperature 103·2°; pulse 130; respiration 24.—9 P.M.: Temperature 103·4°.

7th.—Eruption on face crusting round the mouth and nose, fully pustular on other parts; face still swollen. Has had a very restless night; no delirium. Lies in a semi-stupor, with his head thrown back. Tongue very red and dry. Respiration 30, and rather labored; pulse 130, feeble; some tremulousness of hands; temperature 104·6°. The whole arm at seat of injection is swollen, hard, red, and painful.—9 P.M.: Temperature 104°.

8th.—Eruption on face drying; on the hands the rash is becoming horny, with a few pustules here and there. The whole of the right arm is swollen, tender, inflamed, and becoming brawny to the touch. Tongue dry and brown. Had a rather severe rigor last night, lasting a quarter of an hour; quiet night; slept well. Bowels confined. Pulse 120; temperature 103°; respiration 32. No cough or expectoration.—9 P.M: Temperature 103·2°.

9th.—Face covered with irregular crusts; on limbs and trunk nearly dry. Slept well; no delirium. Tongue dry and glazed; bowels open; very tremulous; no swelling of face; fluctuation becoming distinct over the whole of the anterior surface of right arm, more particularly at the seat of operation; forearm œdematous. Pulse 112; temperature 101°; respiration 32, labored. The arm was incised, when about two ounces of thin pus escaped. A probe passed into the wound went in a direction downwards and outwards for about four inches. To have sulphate of quinine mixture, half an ounce, every three hours.—9 P.M.: temperature 103°.

10th.—Eruption quite dry on face, and crusted on trunk and limbs; drying fast. The wound has discharged very freely since yesterday, quite half a pint of thin pus escaping; swelling and inflammation somewhat less. Was very delirious all last night; purged three times. Tongue dry and tremulous, and he is unable to protrude it; countenance pinched and drawn. Takes milk and beef-tea well. There is general muscular tremor. Pulse 120; temperature 101°; respiration 32, stertorous.—9 P.M.: Temperature 102°.

11th.—Arm discharging very copiously pus thin and unhealthy, otherwise the arm the same as yesterday. The patient lies on his back without moving, with mouth open, and breathing loud and deep. He is losing flesh rapidly; diarrhœa set in last night, but is now stopped; had a draught of catechu and opium. Tongue dry, brown, and tremulous. Was rambling and talking all night; no sleep. Bowels acted once to-day. Takes food well; prostration very great; sweats a great deal. Pulse 112, very compressible; respiration 34; temperature 102·6°. Both lungs posteriorly have a want of resonance; respiratory murmur very feeble; anteriorly resonance good, but murmur feeble, although less so than posteriorly.—9 P.M.: Temperature 104·6°.

12th.—Pulse 140, tremulous; respiration 40; temperature 104°. Incision has sloughed to the size of a half-crown; the cellular tissue quite dissected out from between the muscles. Diarrhœa began again last night, and still continues (about six times in twelve hours); motions very loose, and passed under him. Delirious rambling and talking all night. Tongue very dry, brown, and tremulous; has a general tremor; weakness very much increased; sweats very much; skin quite moist and clammy. Takes food very freely. Died at 5.40 P.M., after convulsions.

Permission for a post-mortem examination was refused. This was a severe case of small-pox, complicated in the first place with bronchitis, and afterwards by consequences which were clearly traceable to the injection of the lymph. It was found, on inquiry into his previous history from his friends, that he had not enjoyed good health for some time, and had suffered from some chest affection.

CASE 2.—E. L——, female, aged twenty-two, domestic servant, admitted on the 3rd of June, on the fourth day of her illness, with a papular rash, small in size, moderately copious in quantity, and discrete on the face; on trunk and limbs the eruption is sparse; temperature 99·8°. In the afternoon the lymph was injected in the usual manner (also in the presence of the representative of THE LANCET). 9 P.M.: Has had a comfortable day; tongue furred; eyes suffused; takes food freely; pulse 88; temperature 101·4°.

June 4th.—Eruption small and becoming vesicular; no swelling; had a quiet night, but without sleeping; slight soreness of throat; temperature 99·4°; pulse 88.—9 P.M.: Temperature 103°.

5th.—Eruption maturating irregularly on face, where there are a few pustules, with a large number of vesicles and papules hard to the touch; on the trunk and limbs eruption small and aborting. Slept well; no swelling or uneasiness, except at the seat of the injection, where there is slight enlargement and tenderness; pulse 104; temperature 100°.—9 P.M.: Temperature 101·2°.

6th.—Pustules drying in places; pulse 96; temperature 99·8°—9 P.M.: Temperature 100°.

7th.—Eruption generally drying and hardening; sleeps well; tongue clean and moist; the pain and swelling at the place of injection has quite gone; pulse 90; temperature 99·4°.—9 P.M.: Temperature 99°.

8th.—Pulse 84; temperature 98·6°.—9 P.M.: Temperature 98·5°.

9th.—Eruption quite dry; small crusts left, which are beginning to fall off; pulse 96; temperature 98·5°.—9 P.M.: Temperature 98·5°.

10th.—Face almost clear; feels quite well; pulse 72; temperature 98·6°.—9 P.M.: Temperature 98·6°.

11th (twelfth day of illness).—Pulse 96; temperature 100·4°.—9 P.M.: Temperature 100°.

12th.—Temperature 98·5°.—9 P.M.: Temperature 98·5°.

This case was a mild one from first to last, as shown by her condition when admitted. The impression of those watching her was, that the patient did not progress so rapidly as might have been expected from the general symptoms. The disease did not completely abort, or we should not have had the rise in temperature on the twelfth day, for which no accidental cause could be discovered.*

CASE 3.—M. A. P——, female, aged twenty-three, domestic servant, was admitted on the 5th of June. Had three good vaccination marks. She states that after premonitory symptoms, which she describes as severe, a rash appeared yesterday. On admission the face was thickly covered with a rash of a dusky-red color, and slightly elevated above the surface. An eruption of the same character, but more sparse, exists on the trunk and limbs. Eyes suffused; lips swollen, and the cuticle separating.—9 P.M.: Temperature 104·2°.

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* Since the above was written, this patient has suffered from inflammatory symptoms over the dorsum of the left foot and the patella of the same side, threatening suppuration in the latter situation.

6th.—The eruption much the same as yesterday; lips and eyelids much swollen. Did not sleep in the night; was not delirious. Early this morning hæmorrhage from the vagina set in; has slightly the characteristic hæmorrhagic fetor; bowels open; tongue much coated. Pulse 140; respiration 28; temperature 104·2°. Vaccine lymph injected.—9 P.M.: Pulse 140; temperature 104°; vaginal hæmorrhage much more severe. To have ten minims of solution of morphia with fifteen minims of dilute sulphuric acid in water, every three hours; ice and iced drinks *ad libitum*.

7th.—Eruption on face becoming irregularly vesicular, and seems confluent; on trunk and limbs also partially vesicular, but discrete. Swelling of face more marked; eyes suffused; lips black; fetor stronger. Vaginal hæmorrhage increased. Did not sleep and was delirious in the night; is now rational. Tongue covered with a thick brown fur. Pulse 120, small; temperature 102·8°. A patch of ecchymosis, about the size of a florin, exists at the place where the lymph was injected. She coughs slightly, and expectorates mucus streaked with blood.—9 P.M.: Temperature 103·4°.

8th.—Eruption on the face of a dingy-white color, interspersed with livid patches; on the trunk and limbs many of the vesicles are filled with a black fluid. Lips and lower part of face much swollen. Did not sleep last night, but was not delirious. Vaginal hæmorrhage still severe. Tongue covered with a thick brown fur, and dry in the middle. Cough becoming troublesome, and expectoration of the same character as yesterday. She now complains of pain and a feeling of oppression across the upper part of the chest. There is dulness on percussion posteriorly on both sides, more marked on the right; loud râles are audible in the same parts. Pulse 140, very feeble; temperature 103°; respiration 28, labored.—9 P.M.: Temperature 103·6°. Died at 11 P.M.; was conscious to the last.

Could not obtain leave to make a post-mortem examination. This was a case of malignant small-pox, in which the hæmorrhagic symptoms, although rather late of being developed, ran a short and severe course, uncontrolled by the injection of the lymph. Death occurred on the eighth day of the disease, evidently hastened by the most common complication of variola nigra—if it can be called a complication—congestion of the lungs. It was ascertained that this patient had led a rather irregular life.

CASE 4.—E. G——, female, aged forty-eight, tailoress, admitted June 5th. Three vaccination marks, small and close together. Eruption appeared to-day, and at present consists of redness of the face, with a very slight elevation of the cuticle. On the trunk and limbs some scattered red spots; premonitory symptoms severe. The contents of two vaccine-tubes were injected into the left arm. —9 P.M.: Temperature 104·8°.

June 6th.—Eruption becoming papular; no sleep; no swelling; tongue furred; thirst; corneæ bright; slight lachrymation; throat very sore. Bowels open; pulse 100; temperature 102·2°.—9 P.M.: Temperature 102·4°.

7th.—Eruption on the face vesicular; on the trunk and limbs becoming so; slight swelling of the face; no sleep in the night; lachrymation; less soreness of the throat; is free from pain; very slight tenderness at the seat of operation; tongue coated and moist. Pulse 82, feeble; temperature 99·8°.—9 P.M.: Temperature 100·4°.

8th.—Eruption on the face pustular; and on the trunk and limbs vesicular; face more swollen; tongue furred; bowels open; quiet night; no sleep; no pain or tenderness at the seat of operation. Pulse 112; temperature 102·2°.—9 P.M.: Temperature 101·2°.

9th.—Eruption fully matured on the face; pustules exuding. On the trunk and limbs the eruption is becoming white; face continues swollen; tongue furred; bowels open; very good night. Pulse 88; temperature 100·8°.—9 P.M.: Temperature 102°.

10th.—Some pustules on the face have risen more since yesterday, the rest are exuding freely; face and hands more swollen; tongue furred; bowels confined; had a very fair night; takes food well. Pulse 96; temperature 101·4°.—9 P.M.: Temperature 100°.

11th.—Rash on the face drying; on other parts the same as yesterday; swelling of the face less; slept well; bowels open; tongue furred; no pain; takes food well. Temperature 100·6°; pulse 96.—9 P.M.: Temperature 101·2°.

12th.—Greater part of the rash on the face dry; on the trunk and limbs pustular; pustules well filled and discrete. Pulse 72; temperature 100·2°. —9 P.M.: Temperature 100·2°.

13th.—Pulse 72; temperature 100°.—9 P.M.: Temperature 99°.

14th.—Tongue clean and moist; bowels open; sleeps well. Pulse 80; temperature 100°.—9 P.M.: Temperature 98·8°.

15th.—Rash not yet dry on the extremities. Pulse 80; temperature 99°.—9 P.M.: Temperature 104°.

16th.—Pulse 100; temperature 100°. Improving.—9 P.M.: Temperature 100°.

17th.—Pulse 80; temperature 98·6°. Desiccation complete.

18th.—Convalescent.

This was a case of moderately severe small-pox running an ordinary course.

CASE 5.—A. C——, female, aged sixteen, servant, admitted June 1st; two faint vaccination marks, three days ill; has a copious papular eruption.

June 2nd.—Rash becoming vesicular and very copious; slightly delirious last night; tongue furred and swollen; bowels confined; takes liquid food well; was very sick both before and after admission; pulse 88; temperature 102·4°.—9 P.M.: Temperature 102·4°.

3rd.—Hæmorrhage from vagina commenced about one o'clock this morning, but only slightly, and continues so; bowels confined; tongue furred; wandered a great deal during night; face a little swollen; rash small and vesicular; very thirsty; throat very sore; takes very little food. Vaccine lymph injected in the usual way; pulse 84; temperature 101°.—9 P.M.: Patient has been very restless all day; rash vesicular and small, but well elevated and discrete; vaginal hæmorrhage increasing all day; bowels not open yet; takes very little food. Towards six this evening she began to wander; at present lies quiet, but staring vacantly up at the ceiling; tongue coated; pulse 100, full and regular; temperature 101·4°. To have fifteen minims of dilute sulphuric acid, five minims of solution of morphia, and half an ounce of water every three hours.

4th.—Eruption on face vesicular, slightly aggregate; on limbs and trunk also vesicular but discrete; considerable swelling of face; hæmorrhage somewhat lessened; some suffusion on inner side of right eye; wandering delirium in night, with occasional sound sleep; tongue dry and brown; great thirst; slight swelling and tenderness at seat of injection; bowels confined. To have half an ounce of castor oil immediately. Pulse 96; temperature 99·8°.—9 P.M.: Pulse 100; temperature 102·4°.

5th.—Eruption on face becoming pustular, vesicular on rest of body; swelling of face continues; tongue dry and brown; hæmorrhage still decreasing; throat sore from eruption on tonsils and soft palate; quiet night; slept fairly; no delirium; no tenderness, and very slight swelling at seat of operation. Pulse 112; temperature 102·2°.—9 P.M.: Temperature 102°; pulse 100.

6th.—Eruption on face matured, many of the pustules exuding; on hands and trunk rash becoming pustular, the whole of it is well developed; swelling of face less; hæmorrhage quite well; no delirium last night, and no sleep; tongue dry at tip; bowels confined; throat better; pulse 104; temperature 102·4°.—9 P.M.: Pulse 100; temperature 101·8°.

7th.—Eruption on face crusting; on other parts is fully developed and pustular; swelling of face less; throat easier; slept badly; no delirium; tongue dry in centre, moistening at edges; hands and feet swollen and tender; no local effect at seat of injection; pulse 112, soft; temperature 101°.—9 P.M.: Temperature 103°.

8th.—Greater part of the eruption on the face drying, especially about the forehead, nose, and lips; the swelling of the hands and feet rather more, of face less. Pulse 140; temperature 105°. —9 P.M.: Temperature 103·6°.

9th.—Eruption crusted on the face; on the limbs and trunk the pustules are full-sized, white in color, and entire; swelling of the face decreasing, and of the extremities increasing; very restless night; no delirium; great thirst; tongue coated and dry in centre; bowels open; throat quite well; takes food well; skin very tender and inflamed. Pulse 120; temperature 102·1°.—9 P.M.: Temperature 101°.

10th.—Very restless and somewhat delirious last night—after that slept fairly; tongue dry, glazed, and tremulous. Pulse 112; temperature 100·8°.—9 P.M.: Temperature 102°.

11th.—Eruption on the face quite dry, on the limbs beginning to break; swelling of the face nearly gone, but continues at the hands and feet; tongue dry in the centre; slept a little better last night; bowels open; takes food well; general weakness great. Pulse 104; respiration 32; temperature 102°.—9 P.M.: Temperature the same.

12th.—Tongue moist; bowels open. Pulse 96; temperature 101·4°.—9 P.M.: Temperature 103°.

13th.—Crusts coming off and leaving the skin very much inflamed; patient drowsy and heavy; slept all night; bowels confined; tongue dry and glazed; takes food well. Pulse 112; temperature 105·3°.—9 P.M.: Temperature 105·4°.

14th.—Over the sacrum and towards the left side is a little redness, with great tenderness on pressure; skin of the face still inflamed, with swelling chiefly of the forehead and eyes; tongue dry and glazed in the centre; bowels open; no rigors; takes food well, especially liquids, being very thirsty. Pulse 112; temperature 103·6°.—9 P.M.: Temperature 103°.

15th.—Pulse 104; temperature 104·8°.—9 P.M.: Temperature the same.

16th.—Pulse 96; temperature 104·8°.—9 P.M.: Temperature 103°.

17th.—Has a hesitation in her speech, and a childishness of manner verging on imbecility. Pulse 112; temperature 102°.—9 P.M.: Temperature 104°.

18th.—Fluctuation distinctly felt over the upper part of the sacrum—also on the forehead, but only slightly so. Pulse 100; temperature 101°.—9 P.M.: Temperature 103·6°.

19th.—Abscess over sacrum is as large as half a cocoanut, and full of pus; the one on the forehead is discharging. Patient slept fairly. Tongue moist and clean; bowels open. Takes food well. To have sulphate-of-quinine mixture, half an ounce three times a day. Pulse 88; temperature 102°.—9 P.M.: Temperature 101·6°.

20th.—The abscess was incised yesterday, when about eight ounces of healthy pus escaped; since then it has been discharging freely a thin reddish-brown matter. Her mind appears much weaker; is very drowsy. Bowels open. Temperature 101°; pulse 120.—9 P.M.: Temperature 104·8°.

The subsequent history of this case may be condensed into a few words. The patient had a succession of abscesses in various situations, but in the beginning of July those that were in existence healed up, and the formation of new collections of matter ceased. Her bodily strength improved, and the mental weakness at the same time seemed gradually to be overcome. We have here the small-pox entirely unaffected by the injection of the lymph.

CASE 6.—M. G——, female, aged twenty, servant, admitted June 3rd. Four vaccination marks; five days ill; eruption vesicular, moderately copious, but discrete and well elevated. Dr. Grieve injected subcutaneously, shortly after her admission, two tubes of vaccine lymph (also in the presence of the representative of THE LANCET).—9 P.M.: Has been quiet all day; taken food well. Tongue furred; bowels confined. Rash coming out well, especially on the hands and wrists. Temperature 101·2°; pulse 104.

June 4th.—Rash pustular on face, vesicular on limbs and trunk; very considerable swelling of face; slight swelling and tenderness at seat of injection. Case progressing naturally. Tongue dry, glazed, and brown; pulse 112; temperature 100·6°.—9 P.M.: Temperature 101·2°.

5th.—The matter beginning to exude from the pustules on the face; on the trunk and limbs the eruption changing to a whitish-yellow color, but in all parts keeping discrete. Very fair night; slept well; no delirium; swelling of the face decreased; neither tenderness nor swelling at the seat of operation. Throat somewhat easier; tongue coated, but moist; bowels freely open. Pulse 112; temperature 101°.—9 P.M.: Temperature 101·8°.

6th.—Some crusts formed on the face; on the trunk and limbs rash large and fully developed; swelling of the face subsiding; slight tenderness and swelling at the seat of operation. Round the ankles and on dorsal surface of the foot the pustules have run together in places, and formed blebs, filled with dark-blood-colored fluid. Tongue dry and brown. Pulse 100; temperature 100·2°.—9 P.M.: Temperature 101·2°.

7th.—Swelling still decreasing; slept fairly; no delirium. Tongue cleaning at the edges, dry in the centre. Local symptoms at the seat of injection gone. Pulse 100, of good strength; temperature 99·2°.—9 P.M.: Temperature 99·6°.

8th.—Tongue glazed and dry; bowels open. Pulse 96; temperature 99·2°.—9 P.M.: Temperature 100·3°.

9th.—Eruption drying well on the face; swelling rapidly decreasing; on the trunk and limbs partly drying. Tongue slightly furred and moist; no pain; feels comfortable; takes nourishment well. Pulse 100; temperature 100·6°.—9 P.M. Temperature 102·5°.

10th.—Eruption crusted, and falling off. Says she feels quite well, except for weakness. Pustules on the arms are not yet dry. Pulse 96; temperature 99·8°.—9 P.M.: Temperature 101°.

11th.—On the extremities some few pustules still moist, as well as the bullæ on the feet. Pulse 96; temperature 98·6°.—9 P.M.: Temperature 98·6°.

12th.—Eruption dry all over, but bullæ on the feet and ankles still continue. Pulse 88; temperature 99°.—9 P.M.: Temperature 99·8°.

13th.—Pulse 88; temperature 99·8°. Improving. —9 P.M.: Temperature 99°.

14th.—Slight circumscribed swelling, with tenderness at the seat of injection; tongue red. Pulse 88; temperature 99°.—9 P.M.: Temperature 99·4°.

15th.—Pulse 82; temperature 99°.—9 P.M.: Temperature 98·5°.

16th.—Swelling about arm less. To use red iodide of mercury ointment. Pulse 104; temperature 100°.—9 P.M.: Temperature 100°.

17th.—Temperature 98·5°; pulse 88. To get up. The swelling at the seat of the injection, which was the size of a walnut, and elastic to the touch, gradually disappeared under the use of the ointment. Desiccation proceeded slowly, and she was discharged well on July 13th.

A mild case of small-pox, with a natural progress and termination; rather troublesome local irritation caused by the injection.

CASE 7.—O. U——, male, aged forty-eight, coachman, admitted June 3rd. Three vaccination marks; four days ill; discrete pustular rash beginning to dry in one or two places on the face; vesicular on arms and chest; throat a little sore; very restless last night; bowels open. Temperature 99°; pulse 72.—9 P.M.: The eruption is filling out well and becoming pustular on the arms. Dr. Grieve injected subcutaneously two tubes of vaccine lymph into the right arm this afternoom. Pulse 96; temperature 102°.

June 4.—Eruption pustular and discrete; very sparse and vesicular on the limbs and trunk; slight swelling of the face; slept well; no swelling, but slight tenderness at the seat of injection; tongue coated. Temperature 99·6°; pulse 80.—9 P.M.: Temperature 100°.

5th.—No swelling nor any symptom of general disturbance; tongue clean and moist; bowels confined; no symptoms at the seat of inoculation. Temperature 99°; pulse 74.—9 P.M.: Temperature 99°.

6th.—Eruption quite dry on the face and maturating irregularly; aborting on the trunk and limbs; slept badly, which he ascribes partly to pain in the right arm at the seat of inoculation, where there is now redness the size of the palm of the hand, with swelling and tenderness; tongue dry. Pulse 112; temperature 101·4°. Arm to be poulticed.—9 P.M.: Temperature 101·6°.

7th.—Eruption on face dry; great thirst; tongue dry. Slept badly. Inflammation in the arm seems somwhat less, but is still considerable. Patient very low. Pulse 104; temperature 102·6°.—9 P.M.: Temperature 102·2°.

8th.—Eruption dried up everywhere, and coming off; redness of arm increased since yesterday, also the pain and tenderness. Tongue dry and brown; bowels confined. Restless night, very little sleep; no appetite or inclination for food. Had a slight rigor yesterday, lasting a few minutes. Temperature 101·6°; pulse 100, feeble. — 9 P.M.: Temperature 101·8°.

9th.—Patient becoming emaciated; complexion sallow; countenance anxious; tongue dry and brown. No further rigors. There is still a considerable extent of redness and induration on the outer surface of the arm. No fluctuation. Pulse 100; temperature 100°; respiration 28. To have sulphate-of-quinine mixture, half an ounce, every four hours.

10th.—Condition much as yesterday, excepting that the inflammation has extended on the outer side of the arm down to the elbow; tongue moist at the edges, drying in the centre; quiet night; slept well; no delirium; bowels open; takes food better to-day than he has for a few days past; no more rigors. Pulse 120; temperature 100°.—9 P.M.: Temperature 102°.

11th.—Inflammation extending upwards; over the scapula hard and brawny to the touch; no fluctuation; patient wasting rapidly; face pinched; complexion sallow; quiet night; slept well; tongue dry and glazed. Pulse 96, small; temperature 99·6°.—9 P.M.: Temperature 100°.

12th.—The inflamed arm is of a dark purple hue and feels boggy, but there is no distinct fluctuation; general condition much as yesterday; slept fairly well last night; tongue very dry and brown; bowels open; takes plenty of food. Pulse 88; temperature 100°.—9 P.M.: Temperature 101·4°.

13th.—Fluctuation distinct; the arm was incised, and about two ounces of healthy-looking pus escaped: quiet night; slept fairly; tongue dry and brown; was sweating in the night very much; takes food in the daytime well. Pulse 80, very feeble; temperature 101·8°.—9 P.M.: Temperature 103·4°.

14th.—Tongue-edges moist for first time; quiet night; slept fairly; takes food well; pulse 80; temperature 98·5°.—9 P.M.: Temperature 98·6°.

15th.—Fluctuation very distinct over acromion. An incision was made and about 5 oz. of pus escaped; slept well; bowels open; tongue dry and brown, with edges moist; takes food well; pulse 72; temperature 99·4°.—9 P.M.: Temperature 99·4°.

16th.—Wounds look very healthy and discharge freely. Both have been washed out with carbolic lotion (1 to 20); pulse healthy; inflammation and swelling decreasing; sleeps well; tongue keeps dry in centre but is gradually getting moister.—9 P.M.: Temperature 101·8°.

From this date the patient improved, although his progress towards recovery has been slow. It was found necessary to make another opening on the outer side of the arm just above the elbow.

This man had a very mild attack of small-pox, and his severe illness and protracted convalescence I believe to be entirely due to the injection of the lymph.

The results in the seven cases when summarised are as follows:—In two instances death occurred (one from the hæmorrhagic form of the disease, the other with severe local and constitutional symptoms seemingly produced by the injection of the lymph); in three more there was extensive suppuration or a succession of abscesses causing a protracted convalescence; in one there was some irritation at the seat of the operation, leaving a solitary instance in which the recovery was satisfactory.

Although abscesses are common sequelæ of small-pox I have not before observed them occur in so high a proportion as they did in these cases, and therefore feel compelled to ascribe their production partially, at least, to the injection of the lymph.

From the history of cases as given in this paper the profession will be enabled to form its own opinion of the value of Mr. Furley's cure for small-pox, but having carefully watched the cases while under treatment, and compared the result with that in others admitted during the same period, I feel myself justified in saying that in not a single instance did the treatment exercise any beneficial influence either over the progress of the disease or over the result, but rather the contrary. Consequently the opinion forced upon me is that the hypodermic injection of lymph in small-pox is worse than useless.

July 13th, 1872.

THE ANTERIOR SUSPENDING SPLINT IN THE TREATMENT OF FRACTURES OF THE LOWER EXTREMITY.

By Charles Shrimpton, M.D. Paris.

The anterior suspending splint invented by Professor Smith, of Maryland University, U.S.A., was brought from America and introduced in France by Dr. Moffitt, through the medium of Dr. Gantillon, who presented it to the Société de Chirurgie de Paris July 15th, 1864. On the 31st of the same month I had occasion to apply this splint in a case of compound comminuted fracture of the leg, and was enabled by this means to bring the patient safely, per rail, without any displacement of the fracture, and without any pain or suffering, from the fall of the Rhine to Paris—a distance of 200 leagues. This case, with an accompanying print, was published in the *Gazette Médicale de Paris* Aug. 5th, 1865.

Professor Smith published, I believe, the first description of his splint in his monograph of 1867, in which he did me the honor to give a prominent place to the case just mentioned. I had the pleasure of making Professor Smith's personal acquaintance in Paris shortly after the publication of his monograph, when he kindly approved of some modifications I had made in the splint, and of some accessories to his system, which he has since, I believe, adopted.

The object of the present paper is to give a description of the apparatus and of its mode of application, to show the principle of action of the system, and to suggest, what I presume to be, the probable reasons why the anterior suspending splint has not yet been universally adopted.

The apparatus is of itself extremely simple: a splint made of two parallel wires of sufficient length to extend from above the anterior superior spinous process of the ilium to beyond the extremity of the toes, a suspending cord, some wadding, and some bandages.

The splint I now employ is made with the parallel wires kept an inch and a half from each other by four transverse fixed bars. (Fig. 1.) B B are sliding bars, made with curves in the centre to receive the hooks of the suspending cord. The suspending cord is formed of two parts, one to hang perpendicularly from its support, the other with a hook at each end, to be attached to the sliding bars of the splint. This part of the cord passes through a pulley, which is hooked to the extremity of the perpendicular cord. The perpendicular cord, as seen in Fig. 2, is double, with a block in the middle, so that the limb may be raised or lowered by lengthening or shortening the cord at will. The wadding to be placed on the surface of the limb before the splint is applied. A double stout band to pass round the pelvis, and fix the upper part of the splint. Five short pieces of band, one to pass round the foot and the splint, two in the same way round the leg, and two round the thigh to keep the splint in place whilst it is being adjusted. Several strong rollers to be applied to the whole length of the limb, to replace the short bands, which are to be withdrawn as the roller is applied. The accessories to the apparatus are the pincers (Fig. 3), which are very convenient for bending the splint *in situ*, to adapt it to the limb. The wood-frame supports the suspending cord. A can, trough, and syphon for irrigation, complete the apparatus.

The print represents the splint *in situ* before the roller-bands are applied. The limb is supported by the temporary short bands. The wadding has been withdrawn to show that the wires cease to touch the limb as soon as it is suspended.

Very little remains to be said in describing the mode of application of the splint. The fractured limb should be left, as it may then be, until the splint has been adapted to the *sound* limb, in order to take its exact length, bends, &c. The suspending cord being now fixed, and the splint prepared with a bend corresponding to the groin of the fractured limb, we may proceed to suspend the limb. The large double thick band is to be attached to the upper part of the extremity of the splint, and passed firmly round the pelvis. The wadding is to be placed on the front part of the fractured limb to receive the splint, and the short bands are now to be tied *lightly* round the foot, leg, and thigh. This done the limb can at once be suspended by the cord without any attempt at reduction, and the reduction will take place almost of itself by the mere weight of the limb. As soon as the splint is perfectly adjusted *parallel to the axis of the limb* the rollers may be *lightly* applied, leaving any part requiring dressing free from every kind of bandage. The wounds may thus be attended to without interfering in any way with the rest of the apparatus. Extension and counter-extension are necessary only to bring the limb to the length of the splint whilst the rollers are being applied.

The following case illustrates the advantages to be derived from the anterior suspending splint. This was a case of gunshot wound fracture of the left leg just below the insertion of the ligamentum patellæ. The subject of it was a French soldier wounded during the siege of Paris. Cure without shortening or deformity of the limb, though the tibia was comminuted to the extent of more than an inch. The patient was enabled to move in his bed at pleasure during the whole period of the treatment.

Adolphe C——, aged twenty-one, of good constitution and healthy temperament, a soldier of the 117th Regiment of French Infantry, received a gunshot wound of the left leg at noon on November 31st, 1870, at the battle of Champigny. On the field, very soon after he fell, Sir John Rose Cormack dressed the wounds and applied a temporary apparatus to the fractured limb. The wounded man was then wrapped in a blanket, placed on a stretcher, and carried the distance of at least two miles over very rough ground, and in a bitterly cold wind. He remained in the "Ambulance volante" for six hours before the journey to Paris could be undertaken. After much jolting and many delays he reached the ambulance, 16, Rue Demours, at 4 A.M. on December 1st. He was then very cold and exhausted. After the administration of warm broth, a full dose of opium, and the bathing of the feet with hot water, he fell into a quiet sleep for some hours, and was in a pretty comfortable state when Sir John left the ambulance about 11 A.M. on the 1st, to return to Champigny, where he remained until the morning of the 3rd, the battle having been renewed with great fury on the 2nd. On the 3rd, in consequence of the pain and swelling of the limb, Sir John relieved it from all pressure, placed the fractured limb in a trough, and ordered irrigation. In the afternoon of that day, about seventy-six hours after the wound was received, Sir John showed me the case, and asked me to apply the anterior suspending splint.

I found the limb much swollen. The man was in a state of great nervous excitement, and complained of great pain in the region of the wound. The ball had passed through the left leg, entering on the inside a little below the insertion of the ligamentum patellæ, passing obliquely downwards and outwards, making its exit about the middle of the opposite side of the leg, thus producing an extensive comminuted fracture of the tibia. The leg probably was raised in the act of running when he received the shot.

Fig. 1.

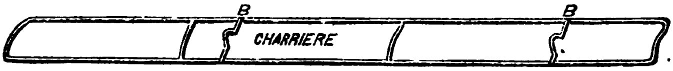


Fig. 2.

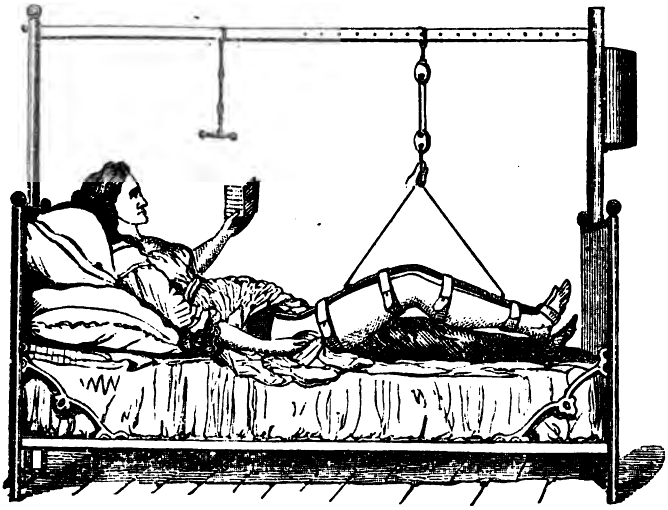
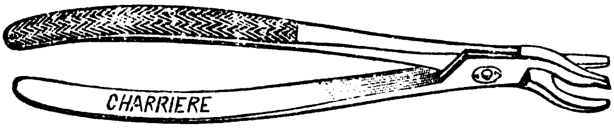


Fig. 3.



The anterior suspending splint was readily applied, and the patient felt great relief as soon as the limb was abandoned to its own weight. He could move himself freely in his bed, raise himself by the aid of the hand-cord to allow the bed-pan to be placed under him, to have his sheets changed, &c., without the least fear of disturbing the fractured limb. The two wounds, the entrance and exit of the ball, were left perfectly free for the daily dressings, the application of poultices, &c. This system of suspension was found very convenient in giving every facility to the surgeon to examine every part of the limb without disturbing the fracture, and without giving any pain to the patient. The bandages were removed and renewed frequently with the greatest ease, and giving great comfort to the patient.

Several abscesses formed and were opened in different parts of the limb. With this exception the patient progressed favorably.

Feb. 18th, 1871.—The ambulance in which he had hitherto been treated was about to be closed. The patient had to be moved to the ambulance, 16, Rue d'Agnesseau, maintained by Sir Richard Wallace. The suspending splint was of great service on this occasion in protecting the limb from the effects of any shock or commotion during the transit. A few days after the transit a large piece of necrosed bone, which had formed part of the internal surface of the tibia, was extracted. This piece of bone was at least an inch square, so that the loss of substance, the other fragments of the comminution considered, involved at least an inch of the entire thickness of the tibia. The consolidation of the fracture was, however, found to be sufficient at this time to allow the suspending apparatus to be applied only at night, leaving the limb free during the day, so that the muscles might gradually recover their power, and the joints their flexibility. Two other pieces of necrosed bone were extracted about this time.

The apparatus was entirely removed on the 15th March, and the patient, after walking on crutches for a fortnight or three weeks, was enabled gradually to make use of his limb. One other piece of necrosed bone was removed after he was able to walk. He has now recovered the use of his limb for more than twelve months. There is no shortening of the limb, only a slight indentation where the tibia had been fractured, and a very small fistulous opening still remains, indicating the process of elimination of some other portion of necrosed bone. The limb is not otherwise deformed, and the man walks as well as he ever did. His left leg is almost as strong as the other; the muscles are developed, and the poor man has been in great distress fearing he may be called on to continue his military service.

It will be observed that the patient felt relief directly the limb was left to its own weight. He very soon abandoned the limb altogether to the apparatus, feeling complete confidence and security in the suspension, and was thus enabled to move himself freely in his bed. This security of the patient is accounted for by the action of the splint, which, being placed *parallel* to the axis of the limb, carries the action of the weight of the limb on the bandages from the splint to the axis of the limb, thereby holding the fractured bone steadily in all the movements of the body. The splint becomes a substitute for the fractured bone, the fragments of which are kept constantly in position, so that the process of ossification is not disturbed by any movements of the patient.

The fracture was so comminuted in this case that we may safely say there was loss of substance of at least an inch in length of the bone, yet the limb moved as freely on the suspension as if there had been no solution of continuity at all. Some portions of this comminuted bone having retained their periosteum served with the callus to consolidate the fractured tibia; the remainder became necrosed and was either extracted or thrown off afterwards.

It is important to remark that the usual pain in the heel and the violent spasmodic contraction of the muscles are effectually prevented by the action of the suspending splint. The continued action of extension and counter-extension and all violence in reduction are superseded by the action of the weight of the suspended limb. The fractured bone must inevitably recover its natural length and position to adapt itself to the splint, which has been modeled on the *sound* limb. The muscles of the fractured limb lose their power of contraction by the uniform compression of the bandages which suspend the limb.

The reasons why the anterior suspending splint has not been universally adopted appear to me to be because the principle on which the splint acts has never yet, I believe, been explained, and because it is so difficult to abandon our old friends—the old splints, the double inclined plane, &c.

The splint employed by Professor Smith until he came to France was nearly four inches in width between the wires. That which I employed myself at that time was about three inches wide. Under these circumstances, not being yet aware of the principle of action of the splint, I was naturally inclined to increase the width between the branches, thinking I might thereby increase the action of the splint. After a time, however, I discovered that the nearer I brought the wires together the greater was the facility I acquired in the application of the splint. *This led me to discover the action of the anterior suspending splint on the axis of the limb—the principle on which, I believe, the whole system is based.* The splint I now use is only one inch and a half wide between the wires, a distance which appears to me to be the most useful and safe in its application. I am convinced that the splint has been brought into disrepute, and been abandoned by many surgeons, because they, like myself, have employed the splint with the branches too wide apart.

Probably the second reason why the anterior suspending splint has not yet been universally adopted is that which I have already hinted at—viz., that we cannot so easily depart from the habits of routine in the treatment of fractures of the lower extremity. It has been supposed that by placing the suspending cord more or less obliquely from the perpendicular line, a power of traction or extension was obtained. I thought so, and have frequently tried it, but always without any advantage. I can account for this now, I think. I asked myself if it is not evident that, as the splint must be attached to the part of the limb above the fractured bone—viz., to the pelvis above the femur, to the femur above the leg, all the traction produced by the oblique suspending cord must act on the pelvis in cases of fracture of the femur, and on the femur in cases of fracture of the tibia. In fact, the action of the oblique cord cannot possibly have any power at all on the fractured bone. On the contrary, by taking the cord from the perpendicular the action of suspension on the axis of the limb is considerably weakened. *The perpendicular cord, in supporting the splint uniformly through its whole length, brings the weight of the limb to act uniformly on its own axis. The cord therefore should never be moved from the perpendicular.* The theory of the action of the obliquity of the cord has most probably been very injurious to the reputation of the anterior splint.

It would only be a repetition of details if I were to apply them to all the surgical cases of the lower extremity. I must however, add that the anterior suspending splint seems to find its special application in cases of fracture of the neck of the femur, and in the transverse fracture of the patella. In fractures of the neck of the femur, either within or without the capsular ligament, the only recommendation is to fix the thigh immovably on the pelvis, and in fractures of the patella to keep the splint quite straight from the groin to the instep. The straight splint, as above applied in the case of fractured patella, is applicable precisely with the same advantages in suspending the limb after resection of the knee-joint.

I have selected the above case from others equally illustrative of the numerous advantages of the anterior suspending splint over every other apparatus in the treatment of fractures of the lower extremity, but having already taken up so much valuable space in regard to this one, I reserve the remainder for future elucidation should the above description be considered too imperfect or obscure.

Whitehall, London, May, 1872.

CASE OF

ACUTE LOCAL MENINGO-CEREBRITIS, WITH APHASIA.

By T. CLIFFORD ALLBUTT, M.D.,

Physician to the Leeds General Infirmary.

THE following case seems to me to be so remarkable from the clinical point of view that I think it right to publish it at once, before it is forgotten in the lapse of time and under the pressure of other work. I am the more disposed to do this as the case falls in well with the series now in course of publication by my friend Dr. Lockhart Clarke.

Mr. X., aged thirty-eight, a patient of Mr. Crocker, of Bingley, was first seen by me about one year ago, during an attack of rheumatic gout. He was a large well-built man, but somewhat heavy, and rather sallow in complexion. He had not been an intemperate man in the ordinary sense of the word, but had lived very well, and had been accustomed both to drink wine and to smoke rather freely. The rheumatic gout fell upon both feet, and subsequently upon the ankles and wrists. He had often suffered before, but chiefly from rheumatism proper, and on a previous occasion he had been under the care of Dr. Garrod. In the present attack he had no heart complication, nor any tendency to endocarditis or pericarditis. The case proved somewhat obstinate. The patient was himself no doubt rather difficult to manage, and may in some measure have retarded his own recovery; but Mr. Crocker and myself were strongly of opinion that the renal excrétion was defective, and hence that his sufferings were protracted. There was no albumen, however, to be found in the urine.

Mr. X. gradually recovered, and entered as usual into business. I heard no more of him until Dec. 10th, 1871, when I was suddenly summoned, as he had been seized in a "fit." Mr. Crocker on that day was in attendance upon some other member of the family, and Mr. X. was, on Mr. Crocker's arrival, sitting alone. Mr. Crocker, on speaking to him, found him unable to connect words into sentences. He was conscious of his own inability, and caught at the true reply when suggested to him. There was then no stupor or dizziness; and, at Mr. Crocker's request, Mr. X. walked unaided upstairs, undressed himself, and went to bed. Two or three hours later stupor set in, and deepened into the state in which I found him at midnight. When I spoke loudly to him he showed some signs of hearing, but no speech or recognition could be drawn from him. There was no palsy, but the pupils were, I think, dilated. We agreed to place two drops of croton oil upon his tongue, and the brisk purge had the effect of clearing all up again, and my services were not in any further request.

I heard of him as attending to business as usual, asserting himself to be quite well. His wife, however, and Mr. Crocker were not quite satisfied about him, and I was written to by both of them to such effect more than once. The nature of this attack was a matter of much questioning between Mr. Crocker and myself. Its transient character made hæmorrhage rather unlikely, and the diagnosis of uræmia seemed also improbable, as there had been no marked drowsiness, no sickness, and no headache beforehand, nor was there any albumen in the urine. Moreover, something like complete recovery at once set in, and continued, so that we attributed the attack to a sudden congestion of the brain. Mr. Crocker, indeed, urged complete rest, and warned the friends more than once against impending brain disease. As subsequent events showed, that diagnosis was probably the true one.

On the 10th of last May I was again requested to meet Mr. Crocker at the house of Mr. X. Neither Mr. Crocker nor his family thought him seriously ill, but during the last few days he had been out of sorts, and his secretions had gone wrong, nor did he seem to get quickly right again. His chief complaint was of intense frontal headache of unusual severity. Some alarm had sprung up lest a new attack of gout were brewing, and hence the consultation. When I first saw Mr. X. the headache had ceased, and I was disposed to look upon his ailment as a mere temporary disorder, and was on the point of committing myself to this opinion somewhat boldly. Fortunately I was saved by an invariable rule of taking the temperature in every case to which I am called. I had now inserted the thermometer as the merest form, but the cheering words died on my lips as I withdrew it at a temperature of 103°. Clearly we were in for something, but for what was not so clear. The pulse I had previously counted at a moderate rate, I believe under 80. There was no eruption, no history of shivering, or indeed of any febrile movement, no diarrhœa, no tenderness of body, nor were there morbid physical signs of any kind about the lungs or heart. Our next two visits did but little to remove our anxiety; the daily variation of temperature between 102° and 103·6°, or even 104° on one or two occasions, was really our only substantial symptom. Now, however, the tendency to deep flushing of the cheek became visible, though no important sweats or shivers occurred. One brother had died of phthisis, and acute tuberculosis presented itself to us as probable, but as yet there was no overt sign of such a thing. During this early stage we noticed the curious independence of the pulse, which never reached 100, and was once or twice as low as 60 in the minute. This was the first observation which gave us a hint of cerebral disease, and this hint once gained, the previous head attack could no longer be forgotten. And we were beginning to realise the fact that Mr. X. had not quite a natural manner, not even the manner of an ordinary invalid. He was curiously obstinate in denying that he was ill, and declined to take medicine, so that the remedies ordered, if taken at all, were only taken under much persuasion. He was, moreover, remarkably taciturn, speaking if at all in very brief sentences. But there was no intolerance of light, no vomiting, no symptoms of palsy or disordered sensation, no delirium, and no decided mental defect. On inquiring more closely from his friends it seemed that Mr. X. had of late been more indolent and taciturn. He would either not speak at all or would speak in a very brief way, though by nature a kindly man. He had also been very much annoyed by one or two matters which, if not actually small matters, were at any rate not of a size to effect a healthy man so profoundly. Chiefly, however, we learnt, for the first time, that twice within the last six months Mr. X. had been unable to reply when addressed; for a few moments it seemed as if the words would not come. But these isolated occurrences were put down as mere "fits of absence" and almost forgotten. On the other hand, shortly before his illness, he had gone through a long and complicated process of "stock-taking" in his business. As the illness went on it seemed more and more clear that the brain was the offending part, and that the temperature disorder was one of those curious clinical phenomena which help to prove the great influence exerted by the nervous system upon heat production. A very characteristic sign now also appeared in the twitching of the lips and of the muscles about the mouth, the "tache cérébrale" was well marked, the tongue became very tremulous, and it was evident that he really could not say more than two or three words, and that even these exhausted him. At a later time these short sentences became rather inappropriate in their matter, little oddities of expression and unseasonable jokes making their appearance. He would also watch people as if suspiciously; the memory seemed to fail in recent things, and the obstinate refusal of medicine and even food drove his nurses almost to despair. The pulse now generally rose to about 100; the temperature to about 102° to 103·5°. Now, also, for the first time, Mr. Crocker found a little albumen in the urine. At one of my last visits some words were wrongly used. On being asked if he knew his brother, he replied, "I know what it was"; and one or two other such errors occurred. In about one month our poor patient sank exhausted, becoming physically weaker and weaker, as though worn down by fever. There was no palsy, but a good deal of twitching and starting of all the limbs continued during the last week or ten days of life. There was no coma or convulsion proper, nor even any change in the pupils. No ophthalmoscopic examination could be made on account of the large well-lighted room and the position of the bed.

We very urgently pressed for post-mortem examination, as we had taken the friends into full confidence throughout, both during our early times of doubt and afterwards. This was at first opposed, but afterwards, to the great credit of all concerned, full permission was given to us. The examination was made, in my unavoidable absence, by Mr. Crocker, assisted by Mr. F. Wright, my house-surgeon at the Leeds Infirmary. Mr. Wright had no knowledge of the history of the case, and I abbreviate simply from his notes.

Autopsy June 7th, thirty-eight hours after death.—Body fat and well nourished. Rigor mortis slight. Decomposition...... somewhat advanced....... There was meningitis over the surface of the left hemisphere;...... brain-substance normal everywhere except in the anterior lobe of the left hemisphere on its upper surface, where the tissue was soft, of a uniformly greyish color, and readily broken down under the touch....... Lateral ventricles contained a small quantity of reddish fluid. No arteritis. Heart healthy...... Other viscera healthy, save the kidneys, which were both rather larger and paler than natural, and presented an early stage of fatty degeneration. Parts were removed for microscopic examination; but the autopsy was made in the evening, and on the next day the parts were too much decomposed for the purpose.

This case is full of interest. The mischief in the anterior lobe of the left hemisphere was attended, not exactly with common aphasia, but with a decay of conversational power. The patient looked intelligent enough, but was unable to speak more than a few words—the few spoken being, however, for the most part, quite appropriate, and varying with the questions. In a word, he was rather mute than aphasic; and this muteness and disinclination to converse (apart from any sulkiness) had been creeping on for some time. He had had two several attacks of common aphasia during the preceding six months, which, however, were transient; his sudden attack in December commenced with aphasia; and I should have added that at the outset of his final illness, Mr. Crocker, at his first visit, found Mr. X. sitting in his chair, not seeming very ill, but quite unable to reply to him for a few seconds. This was so transient as scarcely to attract much notice at the time. On the other hand, he had been able to go through with a heavy stock-taking without any errors.

Another very interesting point is the indication of the thermometer. By the thermometer alone did we learn at first that we had any serious ailment to deal with, the skin having a natural feel, and the pulse being of a natural rate. I may add, indeed, that this very contrast between pulse and temperature did more—it put us on the traces of the kind of disease that was insidiously at work. Of meningitis we had scarcely a single acknowledged symptom from first to last, except the high temperature and the initiatory headache. I should perhaps add that all evidence opposed any suspicion of syphilitic taint.

P.S.—Since the above was placed in the hands of the printer I have had a case almost exactly like it, both in symptoms and in pathological appearances, under my care at the Leeds Infirmary. I must not venture to occupy another column of THE LANCET, but I may shortly say that the symptoms were almost the same, so much so that I referred to Mr. X.'s case, and prophesied the same lesion. In my Infirmary case, as in the other, we had not so much to deal with common aphasia as with muteness and a tendency to speak in short sentences. The more we see of common aphasia, indeed, the more we see instances of its melting off into defect of memory and of mind at one end, and into faulty articulation at the other. It is an interesting fact to note that the districts of softening in both cases were rather on the upper and inner tracts of the left anterior lobe than on the outer and lower portion.

Leeds, July, 1872.

A SUCCESSFUL CASE OF IMMEDIATE TRANSFUSION.

By J. H. Aveling, M.D.,

Physician to the Chelsea Hospital for Women.

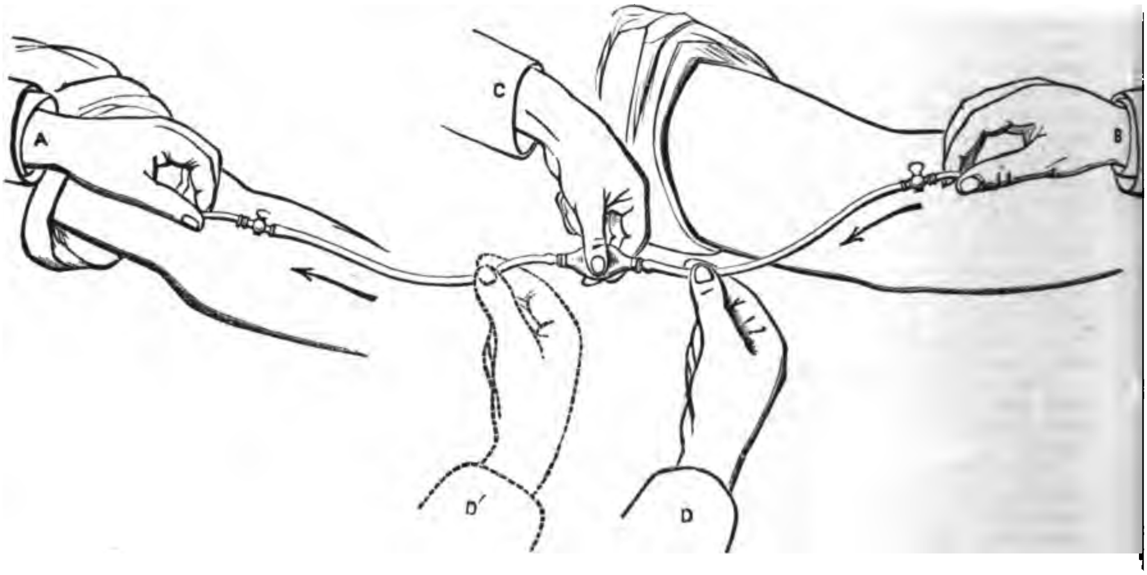
The following satisfactory case of transfusion proves that the old "immediate" method of performing the operation has many advantages—so many, indeed, as to lead to the belief that it must ere long be adopted by the profession. For one hundred and fifty years no other plan was known; and it is only during the last half-century that "mediate" transfusion has been in vogue. The delay and difficulty in operating and the deteriorated condition of the blood transmitted are the great objections to this latter method. If it can be shown that the immediate plan is as successful as it is simple, transfusion will assume its proper place among the remedies of the healing art, and will be used both by physician and surgeon in all cases where deterioration or loss of blood threatens extinction of life.

Mr. F. E. Webb, of Maida-vale, has kindly furnished the following notes of this case previous to the time of my being called in:—

"Mrs. W——, a small, fair lady, aged twenty-one, of rather delicate constitution, was seized with abdominal pains on the evening of March 24th. Early on the morning of the 25th I was sent for. The pains were frequent, but there were no expulsive efforts. The os was slightly dilated and the vaginal secretion abundant. Labour proceeded slowly, owing to the brim of the pelvis being narrow; and the child's head did not descend into the cavity of the pelvis until one o'clock, when sharp expulsive efforts commenced. At two o'clock I began to give her chloroform at intervals, but not to insensibility at any time, until the child was born at 3.30 P.M. As soon as the child was detached smart hæmorrhage set in. I sent for ice, gave ergot and brandy freely, and, grasping the uterus, excited it to vigorous action. The hæmorrhage continuing, I found it necessary to detach the remaining half of the placenta, which was unusually adherent to what felt like the partially inverted fundus of the uterus. This I endeavored to replace, but without success, as I was obliged to desist my efforts at the time lest the shock should entirely extinguish life. Blood continued to flow freely, and the patient became rapidly exhausted and faint, and no pulse could be felt at the wrist. Ice in the vagina at length checked the bleeding; but, as there seemed every probability of the patient dying, the husband was requested to go for further advice. Dr. Cheadle came first, and shortly after Dr. Meadows. At this time (4.30 P.M.) there was no great amount of hæmorrhage, but we all agreed that the only prospect of saving the patient's life was to transfuse some blood into her veins. Dr. Cheadle kindly went for Dr. Aveling, knowing that he would probably have the necessary apparatus, and would be ready to perform the operation."

I found the patient in a most dangerous state of exhaustion, insensible, and no pulse to be found in either the temporal or radial arteries. The pupils were dilated, and did not contract when a light was placed near them. The hands and feet were cold, and the lips and face blanched. The heart's action was weak, and growing perceptibly more feeble. It was evident there was no time to be lost. A fold of skin at the bend of the patient's arm was raised, transfixed, and divided; when a large flattened blue vein became visible. This was opened, and the afferent tube with some difficulty, on account of the insufficient light of two candles, was adjusted. The arm of a coachman in the employ of the family was next prepared as in ordinary bleeding, and an incision made directly into the vein sufficiently large to admit the efferent tube. The man was then seated in a chair beside the bed, and the india-rubber portion of the apparatus filled with water having been attached to the tubes, the process of transfusing commenced. After a few drachms had been transmitted, Dr. Meadows, who kindly took charge of the afferent tube, thought he felt the skin rising near the incision, and suggested that the tube was not in the vein, but in the cellular tissue beside it. This proved to be true, and the tube had to be taken out and inserted into the vein. Its collapsed condition and the want of light made this no easy task; but it was at length effected, and the transfusion then went on steadily and easily until more than sixty drachms of blood had been injected. As the operation proceeded, the pulse at the wrists became perceptible, the lips less blanched, and warmth returned to the hands. The patient also became conscious for a a short time, and said she was "dying." The mental improvement was not as marked and rapid as I anticipated; but this was, perhaps, due to the quantity of brandy she had taken. In a few hours, however, she became quite conscious, spoke, took nourishment, and began her fresh lease of life. The wound in her arm healed by first intention; but it opened again a few days after to allow some pus to escape, the result of the accident already alluded to. When the patient was sufficiently recovered she was placed under the influence of chloroform, and the uterus, which had become completely inverted, was returned to its normal position. After this operation Mrs. W—— improved rapidly, and is now quite well. It would be ungrateful not to admit that a large part of the success of the operation is due to the able assistance I received from Dr. Meadows and Mr. Webb, and, I must add, from the coachman, who was not only collected and cheerful, but able to make several useful suggestions during the process of transfusion.

Mode of operating.—The patient having been brought to the side of the bed and the arm bared, a fold of skin over a vein at the bend of the arm is raised, transfixed, and divided. The flattened vein now brought into view should be seized with a pair of fine forceps, and raised while an incision is made, and the bevel-pointed silver tube inserted into it. The tube should be filled with water, and kept full by placing the thumb over its larger opening. While the operator is doing this an assistant should prepare the arm of the blood donor as in ordinary bleeding, making an incision direct into the vein, and passing the round-pointed tube into it, with its point towards the fingers. This person should then be brought to the bedside of the patient, and seated in a chair. No ligatures are required. B represents the hand of an assistant holding the efferent tube, and the lips of the small wound together, and A shows the afferent tube secured in the same manner. The india-rubber portion of the apparatus, filled with water and kept so by turning the cock at each end, is now fitted into the nozzles. The cocks are then turned straight, and the operation commenced by compressing the india-rubber on the efferent side, D, and squeezing the bulb C. This forces two drachms of water into the afferent vein. Next shift the hand D to D′, and compress the tube on the afferent side; then allow the bulb to expand slowly, when blood will be drawn into it from the efferent vein. By repeating this process any quantity of blood can, at any rate, be transmitted, the amount being measured by counting the number of times the bulb is emptied.



This instrument was invented and shown to the Obstetrical Society in 1864. Dr. Oré, who published an excellent work on transfusion in 1868,* describes an apparatus similar to mine, except that it has valves. These are objectionable, inasmuch as they get out of order, and whip the blood when passing through them. He agrees with me, however, concerning the relative values of the "immediate" and "mediate" methods. He says, in performing transfusion by the immediate plan, his experience has shown that embolism is easily avoided, and that the defibrination of the blood is useless in man, as it does not coagulate for four or five minutes after it has left the vessels.

Upper Wimpole-street, June, 1872.

INTESTINAL OBSTRUCTION OF FIVE DAYS' DURATION, CURED BY KNEADING, AFTER INJECTION PER RECTUM.

By C. Hilton Fagge, M.D.

Assistant-Physician to Guy's Hospital.

F. G. H——, aged forty, was attacked on the evening of Friday, Sept. 22nd, 1871, with great pain in the abdomen, quickly followed by severe vomiting. He had had a motion, natural in every respect, about midday; between that time and the commencement of his illness had lifted no weight, nor done anything to account for the symptoms under which he suffered. He had been a free liver, but had always enjoyed excellent health. He sent, on the morning of the 23rd, for Mr. Brookhouse, of Deptford, to whom I am indebted for the following account of his case:—

"On entering the room, I was struck by the anxious appearance of his face; he was crying out with pain, his hands placed over his abdomen, his thighs flexed on the pelvis. There was great pain in the epigastric region a little to the right of the median line, increased by pressure. The pulse was 80, normal in character; the tongue clean and moist; no hernia; he had had no shivering, and there was no evidence of peritonitis. The urine was very scanty and high-colored. The matters vomited appeared to be gastric secretion with some bile. He was ordered to take a draught of chloral hydrate, twenty-five grains, in cinnamon-water, immediately, and a mixture containing carbonate of soda, tincture of opium, hydrocyanic acid, and spirit of chloroform, every three hours. Hot fomentations were applied over the abdomen, and an injection of castor-oil and turpentine in half-a-pint of gruel was administered. This brought away two or three hard nodules of fæces, and on the evening of the 23rd he was somewhat easier.

"Sept. 24th.—Morning: He passed a very bad night. The pain comes on in paroxysms, recurring every three or four minutes. A little to the right of the median line a small tumour is perceptible, which now and again disappears (? contraction of rectus muscle); the vomiting is constant of the same fluid as before; pulse 90. A second enema ordered.—Evening: The enema returned mixed with a small quantity of blood and much mucus. Ordered ice and one grain of opium powder every hour.

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* Etudes Historiques et Physiologiques sur la Transfusion du Sang. Paris: 1868.

"25th.—Much worse; pain intense; sickness continual; abdomen somewhat tympanitic. In the afternoon he was seen by Dr. Hilton Fagge in consultation. The abdomen was then full, but not over distended; the right loin, perhaps, offered more resistence than the left. To the right of the umbilicus and above it there was distinct hardness, which gave obscurely the impression that a transverse coil of bowel could be felt above, bending upon a vertical one below. This, however, may have been only the rectus muscle; for afterwards, when the legs were drawn up, it became less distinct. The tongue was rather dry; the skin warm and comfortable. The vomited matters were brown, with flocculi of a darker color; but not stercoraceous. He was ordered a pill containing a quarter of a grain of extract of belladonna and a grain of opium every two hours; iced beef-tea and milk.

"26th.—Morning: Has passed a good night, and has had no pain or sickness since 8 o'clock last night.—Evening: Still better; but has passed nothing through the bowels. Urine still very scanty and high-colored.

"27th.—Had no pain or sickness until 9 A.M., when both returned suddenly. A castor-oil and gruel injection administered, but it returned as injected.

"28th.—The pain and sickness still continue at intervals. The man is much lower.—9 P.M.: A long rectum-tube was passed as high as possible, and a pint of gruel, with an ounce of castor oil and half an ounce of turpentine, was injected. After the tube was withdrawn, my assistant, Mr. De Brent, kept the anus closed by pressure with his finger, and I proceeded to knead the abdomen, using great care, but still employing as much force as my patient could well bear. The manipulation was continued for a quarter of an hour.

"The injection was retained until 2 A.M. on the 29th, when between two and three pints of broken-down fæces and mucus mixed with the gruel were passed. Next morning he was free from pain and sickness, and felt quite comfortable. At 9 P.M. the injection with the long tube was repeated, and the abdomen was again kneaded. Half an hour afterwards he passed about two pints of feculent matter, and at 8 A.M. had a natural motion.

"From this date he progressed favorably, but on Oct. 1st he had an attack of delirium tremens, and took fifteen grains of opium before sleep was obtained."

Remarks—In this, as in so many other cases of intestinal obstruction, it was not possible to determine accurately the cause of the disease. The history of the attack pointed to a sudden closure of the calibre of the bowel, and probably to the small intestine as its seat; while the fact that mucus and blood were passed on the third day was in favor of the existence of an intussusception, and of its having become strangulated as soon as formed.

The remarkable feature in the case, however, is the success of the energetic treatment which Mr. Brookhouse adopted. It can hardly be doubted that the life of the patient was saved by the kneading of the belly carried out by his medical attendant; and so satisfactory an issue in this case may well encourage other surgeons to adopt a similar procedure. Yet it cannot be denied that such forcible manipulation of the abdomen would, in many instances, involve great risk of tearing through parts softened by inflammation, or even sloughing, and might thus counteract the curative processes of nature. It can scarcely be said that there was anything in the symptoms in Mr. Brookhouse's case that indicated the special applicability of kneading; nor does the success of the treatment enable a more accurate diagnosis as to the nature of the intestinal lesion to be given than had been given before it was tried. The case still remains a very obscure one in this respect.

St. Thomas's-street, June, 1872.

A NEW THEORY OF GOUT.

By Austin Meldon, M.D.,

Surgeon to Jervis-Street Hospital, Dublin, and Late Demonstrator of Anatomy in the Catholic University of Ireland.

A GLANCE at the history of gout will convince the most incredulous that little progress has been made in the pathology of the disease. There is, moreover, some reason to believe that in its treatment we are not much in advance of the ancients. The humoral theory of Hippocrates and Galen does not differ essentially from the uric-acid and urate-of-soda doctrine of the present day; and there is much uncertainty as to whether our forefathers did not know of the existence of colchicum or some kindred plant. Be this as it may, one thing is certain—gout is now a more common and a more fatal disease than heretofore. Within the last twenty years, at least six thousand persons have died in the United Kingdom directly from this disease, and ten times that number have perished indirectly from it. I therefore deem no apology necessary for laying my views of the nature and treatment of this important affection before my professional brethren, believing it to be the duty of every medical man to forward as far as he can the pathology of disease.

Cullen, who believed gout to be an affection of the nervous system, had few followers; it was difficult to explain by this theory many of its phenomena, and it soon, therefore, passed into oblivion. It was replaced by the reappearance of the humoral theory, which lasted until Dr. Garrod's experiments proved the existence of uric acid in the blood of the gouty. The late Dr. Gairdner believed that the disease was due to venous congestion, but with the exception of a few of his followers, here and in Germany, most medical men have adopted the uric-acid theory.

True, many do not believe, with Dr. Garrod, that the kidney is the organ whose functional impairment causes this undue accumulation of uric acid, but refer it to derangement of the stomach, liver, or kidneys; the principle of the doctrine is, however, universally received. I have, nevertheless, little doubt that every practical physician will agree with me, that the presence of uric acid or urate of soda cannot be the sole cause of this distressing malady. In Ireland, gout is one of the rarest affections met with in hospital practice, yet I have repeatedly found the blood of otherwise healthy men lying in our accident-ward loaded with urates; and my own blood has been in this condition for years, although I have never had the least symptom of gout. Dr. Gairdner found urates in the blood of a boy four years of age, in whose family gout had never been known. It is therefore necessary to find another link to complete the chain; this, I think, is furnished by Cullen's theory.

The predisposing cause of gout is undoubtedly the presence in the blood of uric acid, and of soda in some form; nerve-force, I believe, when in a healthy condition, preserves these two in a fluid state, separately, in a condition in which they may be eliminated by the skin, kidneys, or bowels. As soon, however, as this nerve influence is lessened, these two substances unite in the tissues most removed from the brain and centre of circulation. Irritation and inflammation excite the nervous system to increased energy, and the disease for the time is arrested; often, however, a like cause produces a second exacerbation, a third, or even a fourth, and then nature gradually alters or removes that which has been deposited, and all traces of the fit have passed away. The time that an attack generally commences is at night, when both nerve-force and the circulation are weakest; the position, moreover, most usually affected—the great-toe—favors my theory; and I need scarcely mention the numerous instances recorded where a fit of gout has been brought on by great nervous depression. Columbus never suffered from the disease until disappointment and ingratitude depressed his whole nervous system. Hundreds of similar cases are familiar to all; politicians and speculators are particularly liable to gout.

The action of colchicum furnishes me with one more proof. By an experiment which may be repeated by anyone at will, I have satisfied myself that it is a nervous stimulant. Repeatedly have I, whilst fasting and in perfect health, taken doses of from ten to fifteen minims of tincture of the seeds of colchicum. Its effect was to produce, first nausea, and then increased action of all the organs of the body; the skin became moist, the action of the kidneys and liver increased, and the mental faculties were invigorated. On some occasions the heart's action was much increased, and I have even experienced severe palpitation consequent on its use. All these results can only be produced by a nervous stimulant. This is the only way, too, in which its apparently magical influence on a seizure of gout can be explained, and it accounts also for the injury which its too frequent use may produce.

In conclusion, I have to recommend for trial, in the treatment of chronic gout, some medicines from which I have, in my practice, derived the greatest assistance.

Sulphate of nickel and the triple phosphate of iron, quinine, and strychnia will be found of the greatest benefit.

There is but one other point connected with this subject which I desire to mention. It has often been doubted that the skin can eliminate uric acid; but if a large quantity of perspiration be collected, and, after evaporation, tested, abundant crystals will be obtained. For this experiment it is necessary, in the first instance, to have a large quantity; I have repeatedly obtained as much as a pint, by aid of the Turkish bath. From a small amount of perspiration I rarely succeed in obtaining positive proofs of its existence.

June, 1872.

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ECTROTIC TREATMENT OF SMALL-POX AND SCARLET FEVER.

By Alex. T. Watson, M.D., M.R.C.S. Edin.,

Late Lieut.-Colonel Surgeon-in-Chief of Division, U.S.V. Army.

June 2nd, 1872.—Miss Ada T——, aged eleven, complained of headache and general febrile disturbance, which continued during the 3rd, 4th, and 5th.

6th.—On this afternoon I was called to see the patient, and found the face swollen and covered with an elevated papular eruption, extending over the neck, also on hands and wrists, irritability of stomach, intolerance of light, delirium, intense itching of skin of face and hands, hard dry cough. An enema was ordered, and the patient to be sponged with tepid carbolic-acid soapsuds, and afterwards to have five grains of Dover's powder.

7th.—The entire surface of the body and extremities thickly covered with papular eruption, face and hands greatly swollen, high fever. There is no mistaking the case; it presents every appearance and well-marked symptoms denoting a severe attack of confluent small-pox. The papulæ on the face and backs of hands are forming into vesicles. To be sponged with warm carbolic-acid soapsuds, and immediately afterwards the whole body to be painted over with "glycerinum acidi carbolici" (B.P.), applied with a large camel's-hair brush. Some smarting was occasioned on the parts that had been scratched, but otherwise only a general glow of warmth. Five grains of Dover's powder given to allay irritability; soon afterwards the patient fell into an uninterrupted sleep for several hours. To be sponged in the evening with tepid carbolic soapsuds.

8th.—No vesicles visible, no trace to the touch of papular eruption, no inordinate heat of skin; dark-brown spots occupy the position where the papulæ had been; the swelling of face and hands much subsided; tongue cleaning. The patient's diet to be milk and soda-water, and to be sponged every three hours with tepid carbolic-acid soapsuds.

9th.—The dark-brown spots visible; but otherwise no trace of disease; patient complains of hunger. In addition to soda-water and milk, to have chicken soup and bread; craves to have some of the breast of the chicken. Continue sponging.

10th.—Convalescent, and requests to be allowed to get out of bed.

13th.—Miss Mary T——, aged thirteen, having gone through with almost the identical symptoms as in her sister's case, the eruption duly appeared, and of the same confluent character.

16th.—Master Norman T——, aged five; same symptoms.

19th.—Miss Gertrude T——, aged fifteen; same symptoms.

21st.—Miss Alice T——, aged two; same symptoms.

It would be needless to give in detail the symptoms and treatment of the four above cases, as in every respect it would be a recapitulation of Miss Ada's case. I may add, however, that the glycerinum acidi carbolici was applied in all the cases immediately on the appearance of vesicles being formed, and that the application had only to be made once. They all bore marks of having been thoroughly vaccinated. The atmosphere of the bedrooms was kept constantly saturated with the vapor of carbolic acid, by means of sheets hung up in place of doors and windows, and constantly wetted with a solution of the acid.

25th.—The dark-brown spots fade daily. The patients are all convalescent and have gone to the suburbs of the city.

Early in February, 1872, I was in attendance on several cases of scarlatina in the family of Major A——, in which I made use of baths of carbolic-acid soapsuds, and was forcibly impressed with the mild course of the disease; in contradistinction to that of cases occurring in the same street, and in the opposite house, where, out of a family of four children, three died. In the next cases I attended, carbolic acid was added to the soapsuds, and with most satisfactory results.

Dresden, June 26th, 1872.

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TOXICOLOGICAL GLEANINGS.

By C. Meymott Tidy, M.B.,

Joint Lecturer on Chemistry and Professor of Medical Jurisprudence at the London Hospital.

A few poisoning cases of interest have occurred in my practice that I should wish to put on record.

1. A case was referred to me last July by Mr. Richards, deputy-coroner for Middlesex, where J. H. G——, a girl unmarried, had died somewhat suddenly, and under somewhat suspicious circumstances. Dr. Lawrence Gill, of Bow, at the post-mortem examination, found that she was eight months advanced in pregnancy. The symptoms from which she was suffering when he was first called to see her were as follows. She was unable to swallow, the teeth being tightly clenched. There were convulsions, the legs stiff, and the body slightly arched forwards. The stomach was sent to me for examination, as well as a bottle containing a mixture the girl had taken a short time before death. From the symptoms both Dr. Gill and myself quite expected to find strychnia. There was, however, no evidence whatever of its presence, but we discovered, both in the mixture the girl had taken and in the contents of the stomach, large quantities of savin, from the effects of which there was not a doubt death had resulted. It is somewhat curious the marked similarity in this case between the symptoms produced by savin with those ordinarily found in cases of strychnia poisoning.

2. The second case I wish to note was referred to me last month (May, 1872) by Mr. C. C. Lewis, coroner for Essex. The deceased was a laboring man in the employ of Messrs. Howards, of the Stratford Chemical Works. Not feeling very well, he went into the warehouse, for the purpose of taking a dose of aperient mixture, when, by mistake, he helped himslf to a dose of concentrated ague mixture, containing, as I found, about 85 grains per ounce of quinoidine. So far as we can learn, the man must have swallowed about two ounces of the mixture, or 170 grains of the quinoidine. Immediately after he had drunk it he was taken violently ill with excessive vomiting, and died in about half an hour after the dose had been swallowed.

Dr. Kennedy, of Stratford, made the post-mortem examination, which disclosed a highly congested state of the brain. The right pleura was adherent. The lungs showed the man at the time of death to be suffering from the first stage of pneumonia. The heart, too, showed signs of fatty degeneration.

The stomach forwarded to me by Dr. Kennedy was generally congested with patches of much deeper congestion, and contained about an ounce and a half of a brown liquid, which I found, upon examination, to contain a similar drug to that marked "ague mixture."

This, so far as I know, is the first case on record of death resulting from any of the cinchona alkaloids. I do not regard it as a pure and simple case of poisoning, because of the bad state of the man's general health, disclosed both by the post-mortem examination and by his previous history.

3. The last case I wish to mention was referred to me by Mr. Humphreys, coroner for Middlesex, on the 25th of May, 1872. Two sailors, just returned from a coasting voyage, asked their landlady to procure them two black draughts at a chemist's shop near at hand. Each man took a draught, and both fell ill very shortly after having taken them. One, according to the evidence, had intense vomiting of a thick dark-brown liquid. There were also certain nervous symptoms present. The man, however, ultimately recovered. The other sailor died. The contents of the stomach were sent to me for analysis. They gave an intensely acid reaction, very marked even when diluted very largely with water. Analysis proved that there was present a large quantity of oxalic acid. The man was seen by Dr. Nightingale, of Limehouse, about two hours after he had swallowed the draught. He found him in a complete state of coma. The symptoms (according to Dr. Nightingale, to whom I am indebted for complete notes of the case) set in in about a quarter of an hour after having taken the draught. The man died in about five hours, the loss of consciousness apparently being complete to the last. The post-mortem revealed nothing specially to be remarked except intense congestion of the vessels of the brain.

The case is instructive as illustrative of the remarkably anomalous effects occurring in poisoning by oxalic acid, the symptoms in this case being marked coma.

Cambridge-heath, Hackney, June, 1872.

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CASE OF RUPTURE OF THE HEART.

By D. Hughes, M.D., F.R.C.S. Ed.

Assistant-Surgeon 14th B.N.I., Camp Kolapore.

Bheestie Theile Abelools, F Company H.M. 14th Regiment N.I., died suddenly in his house between one and two o'clock A.M. on the 4th of May, and on the same day a Court of Inquest was held, at which the following facts were elicited:—

The deceased, who had been in his usual health, and worked hard all day, after partaking of a meal of dhall and rice, retired to rest about 9 P.M. on the 3rd of May. At midnight he awoke his wife, and complained of feeling very sick; in about an hour's time he became very ill indeed, and his wife, being alarmed, called in a neighbor, who, seeing that the illness was serious, sent to the hospital for assistance. Shortly after the despatch of the messenger, and before the hospital-assistant could reach the house, the patient died. Before his death he complained of a pain as if his liver were being torn to pieces, and asked to be supported in a sitting posture, as his breathing was oppressed. On being raised he fell forwards, and when lifted up again almost immediately fell backwards and expired. The deceased was about thirty-six years of age at the time of his death, and had never complained to any one of pain in his chest.

Autopsy, six hours after death.—Rigor mortis present. The body was muscular and well nourished, and free from marks of violence. There was a fair quantity of subcutaneous fat. On opening the chest, the lungs were found pale and collapsed. The pericardium was greatly distended, and when opened was found to be full of fluid venous blood. When a sufficient quantity of this blood had been soaked up by a sponge, to allow a clear view to be had of the heart, a rent, half an inch in length, and about midway between the apex and base, was observed in the wall of the right ventricle, close to, and parallel with, the longitudinal sulcus. The heart was then carefully removed from the thorax for further examination. The right ventricle was much dilated, its walls soft and thin, especially in the vicinity of the rupture, and very pale. So soft and thin were they that when the organ was pressed during the manipulations necessary for its removal from the body, and some of the contained blood squeezed through the rupture, the rent was extended for about a quarter of an inch, and the wall as it were collapsed. The valves, both tricuspid and pulmonary semilunar, were incompetent. The cavity contained a few small clots and fluid blood. The right auricle and the chambers on the left side were fairly contracted. The valves of the left ventricle were free from disease, but the walls were softer and paler than usual, this condition being due to fatty degeneration. The aorta was healthy. A cursory examination was made of the abdominal organs *in situ*, and they were found to be natural in appearance.

Remarks.—Rupture of the heart is more common in males than in females, and seldom occurs in persons under forty years of age. Even then it is rare, and rupture of the right side of the heart from disease is especially so. The cardiac walls, as a rule, give way under strong emotion or great physical exertion, and death is generally almost instantaneous. In this case the rupture occurred while the sufferer was asleep, or at least at rest, and death did not take place till considerably more than an hour after its occurrence. The mischief probably began at midnight of May the 3rd, when the patient first complained of sickness, due, no doubt, to the shock occasioned by the giving way of the ventricular wall. As the rent in the heart increased, and the blood flowed more freely, the pericardium became distended, causing the tearing pain described by the patient as in the liver* and the heart's action became embarrassed, causing dyspnœa. When the effusion of blood had become considerable the heart could no longer perform its functions, and the patient died.

I was unable to secure the heart for microscopic examination, but I am tolerably certain that the whole organ was affected with fatty degeneration. The right ventricle, however, was so much softer and more friable than the other chambers that I am inclined to believe that it was affected with true ramollissement.

Camp Kolapore, May, 1872.

STRANGULATED INGUINAL HERNIA OF THE RIGHT SIDE.

ASPIRATING PUNCTURE; ISSUE OF FLUID AND GAS; IMMEDIATE REDUCTION; CURE.

By Dr. Leon Labbe.

Surgeon to the Hopital la Pitie, Vice-Professor at the Paris School of Medicine, etc.

The extreme importance which the process of reducing hernia after an aspirating puncture must rapidly assume in surgical practice, the limited number of facts which have been known until now, and the interest there is in encouraging medical men to follow this direction, induce me to communicate the following case which I have just had an opportunity of observing.

In the night of June 20th Mr. D——, aged seventy, an exceptionally strong and robust individual, after a violent fit of coughing felt an intense pain in the right inguinal region. This was followed in a few minutes by nausea and vomiting; whilst a somewhat large tumour showed itself in the right inguinal region.

On the 21st, at 6 P.M., I first saw the patient, together with his ordinary medical attendant, who had employed taxis with great care, but to no effect. I then tried taxis myself, but was equally unsuccessful. The nausea and vomiting still continued; pulse 75. Taking into account the circumstance that strangulation dated only about eighteen hours, and that probably the anatomical lesions would be very slightly advanced, I without hesitation proposed puncture with the aspirator, and without further delay I introduced the No. 2 needle. About ten grammes (two drachms and a half) of a yellowish liquid immediately escaped, together with a quantity of gas which I cannot exactly estimate. The tumour, which was as large as the fist, flattened immediately; and a very gentle pressure exerted for one minute near the neck of the sac caused complete reduction of the hernia. The patient felt immediate relief, and expressed his satisfaction.

During the few hours which followed—from 6 to 11, and more especially between 8 and 11 P.M.—there was a little vomiting, and more particularly nausea. The patient was somewhat feverish, and had slight rigor. I had administered, as I always do after reduction of hernia, whether by taxis or operation, pills of the gummy extract of opium (each containing one centigramme—one-sixth of a grain,—to be taken one every two hours, so that ten or twelve centigrammes may be absorbed in twenty-four hours), with the object of bringing on paralysis of the intestines. At 11 P.M. the symptoms above alluded to all disappeared, and from that time the patient enjoyed the most complete comfort.

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* Natives ascribe every acute internal pain to the liver (which in the East is the seat of the affections, corresponding in that sense to the Western "heart"), and have very hazy ideas as to the exact locality of that organ: consequently we may assume that the pain alluded to was actually in the cardiac region.

June 22nd.—Pulse 60; countenance normal; scarcely any tenderness in the abdomen over the inguinal region.

23rd.—Three natural stools; no pain or fever; appetite excellent.

Eight days afterwards the cure, which had been evident even on the third day, had become quite permanent. The patient's health is now excellent.

Paris, July, 1872.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Proœmium.

ST. BARTHOLOMEW'S HOSPITAL.

HYSTERIA.—VESICO-VAGINAL FISTULA.—RECTO-VESICAL FISTULA.—TRACHEOTOMY.

(Under the care of Messrs. CALLENDER and MORRANT BAKER.)

THE notes of the following interesting cases have been communicated to us by Mr. G. B. Fergusson, house-surgeon.

CASE 1. *Hysteria; a number of needles and pins removed from the arms and legs.*—M. D——, a fresh-colored, artless-looking girl, spare but not badly nourished, was admitted into the hospital with the following history:—About four years ago she was subject to fainting fits, but has never appeared very excitable. According to her own account she had a fall last July whilst in service, by which her left knee was injured, and was for some time exceedingly painful. In this condition she was sent home, and the knee was then poulticed. After a day or two a needle was seen protruding over the patella; this her mother removed. As the knee was now better, she returned to her situation; but in about a fortnight, the same knee again commencing to distress her, she was again sent home, and before long five needles were removed from the same situation. After a short time the arms and legs became similarly affected. Things proceeding after this fashion, 101 needles and pins, many of large size, curiously twisted and deeply imbedded, were removed by her usual medical attendant, Mr. Wicksteed, of Walthamstow. Although some years ago she had a habit of swallowing strange things—pieces of paper and the like,—she is sure she never swallowed any pins or needles, and cannot, she says, give any explanation of their appearance. During her stay in the hospital in February last, ten needles and pins were removed by Mr. Baker and Mr. Fergusson.

The facts that the right hand and arm presented two marks only where needles had been withdrawn, whereas the left hand and arm showed thirty-seven marks, that the girl was right-handed, that the pins were all destitute of heads, together with other circumstances, led to the conclusion that the case was one of hysterical deception. Since the time of her leaving the hospital there has been no fresh appearance of needles and pins.

CASE 2. *Vesico-vaginal fistula; operation; cure.*—The patient was an old woman, aged seventy-six. Eleven years ago she suffered from prolapsus uteri; for this a pessary was worn with comfort, and prevented the prolapse. The instrument was retained without change for nine years; during this time she had no pain or any other discomfort; after this time she both suffered pain and had a discharge. Eight months ago the pessary was removed, and after this she found that she passed nearly all her urine per vaginam. The labia majora, having become excoriated, subsequently united throughout nearly their entire extent. Mr. Baker, having separated these adhesions, found that the fistula produced by the pressure of the pessary was transverse, sufficiently large to admit the tip of the finger, and situated close to the anterior cervical lip. The edges were pared in the usual manner, and five stitches of silver wire employed; all, with the exception of one, passed through the anterior lip of the cervix uteri, and one, having transfixed the lip, passed out through the os uteri. The operation was performed on May 21st, and on June 6th, a fortnight and two days after, the stitches were removed; perfect closure of the fistula had resulted. The notable feature in this case is the great age of the patient.

CASE 3. *Recto-vesical fistula, following lithotomy; successful plastic operation.*—J. W——, aged fourteen, was successfully operated upon for stone, in Aberdeen, seven years ago; since that time he has always passed a little water per rectum, and small portions of his food, notably seeds of different kinds, with his water. The passage of these bodies has habitually caused much irritation, and on coming to the hospital he complained of several of the symptoms of stone. The sound was used, but failed to indicate the presence of a stone. The fistula, which would just admit an ordinary sound, and which was situated about three inches above the anus, was operated upon after the usual manner on June 22nd, by Mr. Morrant Baker. The operation proved less difficult than had been anticipated, a large-sized duck-bill speculum being made use of, together with the needle-holder recommended by Mr. Thomas Smith for operations for cleft-palate. Eight sutures were passed. After the operation, the patient was confined to bed, and placed on a fluid diet, and given ten minims of tincture of opium twice a day, which only partially restrained the action of the bowels. At the expiration of three weeks, on July 18th, the stitches which had not of themselves escaped were removed, and union was found to be perfect.

CASE 4. *Scald of the glottis in a child seven-*

teen months old; tracheotomy; recovery.—The scald was inflicted in the usual manner on the 18th of February last during an attempt to drink from the spout of a teakettle. Three hours afterwards the child was brought to the hospital in a state closely approaching death from asphyxia. The operation of tracheotomy was performed in the ordinary way, above the isthmus, by Mr. Ferguson, and immediate relief was afforded. As the child was progressing favorably, an attempt was made on the third day to remove the tube, but it was then found that no means whatever of breathing through the glottis existed. On the fifth day after the operation a small quantity of air could pass through the glottis. On the ninth day the child succeeded in breathing without the tube for four hours, but after that time was in great difficulties. On the thirteenth day the tube was dispensed with for nine hours; but great dyspnœa supervening, it had to be reintroduced. Things proceeding in this way, it took ten weeks in all (until April 22nd) before the tube could be finally dispensed with. During this period several narrow escapes occurred, the child at one time breathing easily by the natural channel for several hours, and then suddenly requiring the introduction of the tube. On more than one occasion prolonged artificial respiration was required to save its life. A Fuller's bivalve tube was used at first; subsequently an ordinary rigid English one; afterwards the French pattern, in which the tube is moveably attached to the collar. The various tubes were of different lengths, to avoid continued pressure on the same parts of the trachea. A rigid, blunt-pointed metallic director was used throughout to aid the introduction of the tube. This proceeding was often, after the removal of the tube for some hours, a most difficult one, the operation wound after some weeks being nearly wholly filled up by granulations.

One of the greatest troubles throughout was the abundant production of mucus, necessitating frequent sponging out of the tubes. This was notably affected in amount by the weather, being much increased on cold days. The ordinary steaming apparatus was used for rendering the air moist and warm in the vicinity of the patient.

The details have been given at length from the belief that the case is of some importance from the tender age of the patient. The child is at the present time in perfect health, and the scar of the operation scarcely perceptible.

CIRRHOSIS OF LIVER; ERYSIPELAS.

(Under the care of Dr. Black.)

The following account is interesting in many ways. We do not often meet with cirrhosis of the liver at so early an age, nor in a man not addicted to drink, and who has not had syphilis. The cerebral symptoms were also peculiar. His death was apparently due to exhaustion during a severe attack of erysipelas ambulans. The notes of this case have been forwarded us by Mr. H. Thompson, house-physician.

A. O——, aged twenty-six, a saddler, was admitted into hospital on Feb. 16th, 1872, complaining of pain in the region of the spleen; also, he is unable to work on account of his hands shaking whenever he attempts to do anything. He is a stupid, drowsy-looking man, with sallow complexion and conjunctivæ slightly tinged with bile. Bowels regular; fæces very pale. Urine: sp. gr. 1020, no albumen, but traces of bile. Heart and lungs healthy; liver smaller than natural; spleen much enlarged, and can be felt 8 in. below the costal cartilages. No anasarca or ascites. He has been married five years, and has two children. Has always been a temperate man; never had syphilis; has been ill three years. Eighteen months ago he had general anasarca, which has just left him. In the spring of 1871 he had a febrile attack, lasting six weeks. He lost the partial use of his left side two years ago. Has had occasional attacks of apparently *petit mal* whilst working; during which he lets the tools fall from his hands, but does not entirely lose consciousness. He is sometimes seized in this way whilst walking, and would fall did he not lay hold of something to support him. The attacks are generally preceded by a feeling of tingling, cold, or numbness, especially in the hands and face.

Feb. 25th.—Whilst standing by the ward fire this afternoon he suddenly twisted quite round three or four times. The nurse asked him what he meant. He said he could not help it; it was his head. He did not fall, but quietly sat down in his chair. He says this attack was preceded by tingling commencing in the fingers of the right hand, and gradually spreading up his arms, and when it reached his head the attack came on. For some minutes after he seemed very drowsy.

March 1st.—Yesterday, whilst sitting by the fire, he fainted. To-day he cannot walk across the ward without assistance. Complains of tingling all over his limbs, and giddiness; eats well, but his meat has to be cut up for him. Conjunctiva more tinged with bile. Not so much pain in the region of the spleen.

4th.—About 2 A.M. the house-physician was called to him, as he was very restless. About ten minutes after the patient suddenly became very furious, flung himself out of bed, kicked, and tried to bite. By the united efforts of five persons he was lifted, or rather forced, into bed, and there held whilst his wrists and ankles were strapped to the bed. He was unconscious; pupils widely dilated, but contracted slightly under the influence of light. The eyes were not fixed, nor did he twitch his face or bite his tongue; both sides of the body were convulsed evenly. The attack lasted about an hour. He was very wild and excitable during the next twelve hours, struggling and shrieking. About 11 A.M. he was quiet, and said he did not know he was making a noise, and could not help it. He complained of a severe pain in the hypogastric region, and on placing the hand there the bladder was found much distended. A catheter was passed, and three pints of dark-colored urine were drawn off. This was repeated at 10 P.M.

On the 5th the patient seemed better, and passed his urine naturally. Body and conjunctiva rather more tinged. Bowels confied; ordered a purge.

He had another slight attack on the night of the 7th, but was not so violent. Urine contained more bile, and a small quantity of albumen.

On the 12th, as he had been very noisy and violent the night before, he was removed into the strong room in the casualty ward.

He seemed to improve for a few days, and his hands became less shaky; but on the 21st his face and nose looked puffy, and on the next morning his features were almost entirely obliterated by a reddish-white brawny swelling. Both eyes were closed; face dusky; bowels confined; tongue dry, brown, cracked; skin yellow. Temperature 101·4° F.; pulse 108.

22nd.—Pulse 132; temperature 104·2°. Tongue swollen; skin perspiring profusely; quite sensible. Collodion painted over the face. To take half a drachm of tincture of perchloride of iron every two hours.—10.30 P.M.: Pulse 132; temperature 103·6°; breathing semi-stertorous; bowels freely opened at 5 P.M.; still perspiring; lips brown and dry. To have one ounce of brandy every hour.

23rd. — Pulse 100; temperature 98·6°; respiration 28. Face not so much swollen. Venous pulsation was noticed yesterday, even in the superficial veins on the back of the hands. — 10 P.M.: Pulse 108; temperature 102·2°; respiration 36. He has not been able to open his eyes since the commencement of the erysipelas. One ounce of brandy every two hours.

25th.—Numerous bullæ, which had formed on the left side of the face, were punctured, and gave exit to a large quantity of serum. Pulse 108; temperature 101°. One ounce of brandy every four hours.

26th.—Face and neck much less swollen; the erysipelas is spreading down the chest. Temperature 99·7°; pulse 96.

28th.—Face and neck nearly of the natural size. The rash has extended down the back as far as the loins. There is a small bed-sore on the sacrum. Nose bled freely this morning. At 10 P.M. the patient began to hiccough. He was ordered an injection of one-third of a grain of acetate of morphia. Pulse 84; temperature 100°.

29th.—The hiccough continued all night. The rash has spread down to the umbilicus. Numerous red ecchymotic spots exist on the right side of the body as far as the iliac spine. Temperature 101·6°; pulse 84.—10 P.M.: He is quite sensible; hiccough every fourth inspiration.

30th. — Temperature 100°; pulse 96. Bowels very much relaxed; motions horribly offensive, dark-colored from altered blood, and containing a small quantity of bright-red blood.—10 P.M.: Purging still continued; he has begun to vomit large quantities of blackish material, apparently altered blood.

The patient died at 8.30 A.M. on the 31st. He was quite sensible to within half an hour of his death.

Autopsy thirty-eight hours after death. — Body universally jaundiced, but not deeply so. Ecchymoses on many parts of the skin, on the arms, neck, and legs. Brain and membranes healthy. Pericardium and heart natural, excepting some hypertrophy of the left ventricle. Much œdema of the bases of the lungs. Pleuræ healthy. Liver small, weighing 39 oz.; right lobe especially shrunken, so as not to be much bigger than the left; surface very nodular; on section tough, traversed in all directions by fibrous bands; no iodine reaction. Spleen large, soft, and weighing 1 lb. 5 oz. Stomach full of blood; not much changed; mucous membrane everywhere intact, but stained deeply black by the iron administered; the tissues black, the staining not being merely superficial. The small intestines contained blood wellnigh throughout; mucous membrane dark, but less so than that of the stomach. Large intestines natural. Kidneys large; capsule adherent—tears away tissue; surface quite smooth; on section pale; cortical substance broader than natural. No scar discovered on penis.

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UNIVERSITY COLLEGE HOSPITAL.

DIAPHRAGMATIC HERNIA; TETANOID SPASMS; DEATH.

(Under the care of Dr. RUSSELL REYNOLDS.)

THE following case presents many points of extreme interest: first, in its duration and symptoms; next, in the curious spasmodic condition which preceded death. This latter symptom was such as would suggest poisoning by strychnia; but such a contingency has been excluded by a careful examination of the substances that had been administered to the patient, the hypodermic solution of morphia especially being carefully analysed, and found to be quite pure. Lastly, we have the extensive lesions seen after death, none of which had been suspected. Although the history would appear to point to a traumatic origin, yet there was no evidence of this to be found on examination of the affected parts after death.

Mary P——, aged twenty-nine, was admitted into the hospital on Nov. 4th, 1871. From childhood she was always robust and healthy, and had always been well clothed and fed. She was married ten years ago; and, with the exception of one miscarriage, her health was good for the next three years. Seven years ago her husband in a drunken fit stabbed her in three places: one in the left cheek, another in the left axilla about the seventh rib, and the third in the back to the right of the eleventh dorsal spine. She was told that the wound in the side had penetrated the lung. She recovered from this, and regained her usual health, which lasted till her confinement, a few months after. The labour was a long and tedious one, lasting from the Monday till the Wednesday. She also lost a large amount of blood at the time. She never recovered her strenth after this, and began to suffer from repeated vomitings after everything that was taken into the stomach. The vomiting has continued unabated during the last six years, causing gradual loss of strength and flesh. She has never menstruated since her confinement.

On admission the patient was in a state of extreme emaciation, but did not complain of any pain. She said that before her illness she weighed over 9 st.; her weight now is 4 st. 7 lb. The chest was small; the left clavicle more prominent than the right; the angle of junction of the second rib with its cartilage greatly increased. The whole left front above the third rib was flattened; costal angle long and narrow. Below the ensiform cartilage was a groove about two inches long, bounded by the cartilages of the false ribs. The veins on the front of the chest normal; right upper front expanded more than the left. There was an abnormally full percussion note over the whole front. Respiration-sounds at the front, and long-divided; expiration prolonged. But at the left supra-clavicular region the breathing had a bronchial character. Superficial veins of the abdomen very prominent. No tumour could be felt in the abdomen. Liver dullness somewhat increased. The stomach-note was quite distinct in the left hypochondrium, but decidedly deficient towards the epigastric region. There was an abnormally full percussion note over both backs. The urine was alkaline and contained a trace of albumen. Ordered the white of six eggs, iced.

Nov. 9th.—Had an enema of egg, Liebig's extract, and milk, which was immediately returned.

10th.—To have custard pudding at noon and arrowroot at 5 P.M.

13th.—Custard puddings have been retained, but the white of eggs has been once or twice rejected.

15th.—Can keep down the milk and eggs.

26th.—Ordered a quarter of a grain of opium every three hours. To have nothing but milk and lime-water, but this was vomited; to return to the eggs and custard, which can be retained.

Dec. 25th.—Much improved; the sickness is much less, and patient can now retain larger quantities of food. She now sits up half the day.

Jan. 19th.—Still sick occasionally, but is much stronger, and has gained 19 lb. in weight since admission; weight now 5 st. 12 lb.

Feb. 1st.—Sick generally twice a day.

3rd.—Has had several attacks of diarrhœa, lasting for a few hours, checked by catechu and opium.

5th.—Weight 6 st. ¼ lb.

21st.—Has lost weight (5 st. 11 lb.); still has occasional attacks of sickness.

March 1st.—Had a distinct attack of gout; the left great-toe was alone affected. To have half a grain of extract of colchicum, with her opium pills.

11th.—Colchicum discontinued.

28th.—Opium pills discontinued, but a hypodermic injection of a quarter of a grain of acetate of morphia to be given every night over epigastrium. The first injection caused an unusual amount of pain. There has been a good deal of sickness.

29th.—Evening: Pulse 104. Patient complained of uneasiness and flushing of the face.

30th.—At 6 A.M., contraction of the muscles of the forearm and arm was noticed. Patient complained of no pain. The hands were adducted and fingers and forearm flexed, resisting extension, and returning at once to their former condition. Patient says she cannot move them. Has been sick several times in the night. Pupils contracted, and react very imperfectly to light; no difficulty in opening the mouth or in speaking; no rigidity of neck. Ordered half an ounce of brandy every two hours.—9.30 A.M.: Condition worse; patient cannot open her mouth; has pain in the back and mouth; the forearms, especially the left, firmly contracted; speech is thick; there is considerable dyspnœa; temperature 103·6° F. in the axilla.—10 A.M.: The patient died in a state of general tonic spasm. No loss of consciousness between 6 A.M. and death.

Autopsy forty-eight hours after death.—On opening the thorax the heart was found to be considerably displaced to the right of the middle line, whilst a portion of the stomach, distended with gas, projected in the situation of the heart, and to the left as far as the thorax parietes. The projection was about the size of two fists. The greater part of the stomach formed a hernia through the diaphragm, pushing up the base of the lung, which was only slightly adherent to it. The liver was enlarged. Right lung normal, pleura dry; left lung pushed upwards, and did not extend below the third rib; universally adherent by thick, firm adhesions. The pleura at the base of the lung was much thickened; the pericardium universally adherent; the heart walls flaccid and thin; valves healthy. On close examination of the hernia, it was found that the stomach and several inches of the transverse colon, with a portion of the omentum, had been protruded through an opening in the diaphragm near the left edge of the central tendon. Firm adhesions existed between the omental mass and the upper surface of the diaphragm, and also between the protruded mass and the under margin of the diaphragm, extending all round except at the inner side corresponding to the situation of the upper extremity of the duodenum, between which and the inner margin of the aperture in the diaphragm two or three fingers could be passed with ease. The whole of the stomach was displaced into the thoracic cavity except a small portion at the cardiac end, which was lying in contact with the spleen. There were seven inches of the transverse colon in the thorax, and about an inch of the duodenum.

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KING'S COLLEGE HOSPITAL.

THE following operations were performed at this hospital on the 20th inst.:—

Ligature of Nævus.—The patient, a baby about six months old, was under the care of Mr. Henry Smith. The nævus, which was of the mixed kind, partly cutaneous and partly subcutaneous, was of large size and situated on the top of the head. The child had been under treatment elsewhere, and an attempt had been made to destroy the nævus by caustics, the strong nitric acid having been used. This had failed; for it was impossible to destroy by caustics such a growth. As could be seen, all that the acid had done was to destroy some of the cutaneous tissue, and the cicatrix resulting from its use was visible. The only treatment to be adopted for its removal was to ligature it, and this Mr. Smith then proceeded to do. The strong nitric acid could be used with advantage for the cutaneous form, but for the subcutaneous the ligature must be resorted to.

Excision of the Head of the Femur. — This patient, a girl about four years old, was also under the care of Mr. Smith. There was extensive disease about the right hip-joint, and an extraordinary amount of deformity; the leg was flexed and much adducted. The child was emaciated and had been long ill; and the disease was advanced so far that her life was imperilled. Abscesses had formed about the joint, on the outer side, and also on the abdomen; and this led Mr. Smith to think that the acetabulum was diseased or perforated, and that the mischief was not confined to the head of the bone. The only chance of saving the child's life was to excise the head of the femur, which Mr. Smith then proceeded to do in the usual way. The head and part of the trochanter was removed; it was carious and much altered, and the cartilage all gone. The head was lying on the edge of the acetabulum. The acetabulum was healthy, and Mr. Smith said he did not doubt that now the diseased head was removed all would go on well. He alluded to the time when the operation was first performed by Sir William Fergusson, now nearly thirty years ago. The interest aroused at the time was great: now the operation has become an established one in surgery, and those who then were most opposed to it have gone even further, and advocated its employment in cases which were then considered unfit.

Disease of the Hip-joint.—The patient, a boy about ten years of age, was under the care of Sir W. Fergusson. Both hips were diseased, and it was difficult to say which was the worse. The right is now apparently improving. Some time ago there was great deformity of the limb, but this had been remedied by cutting through a few fibres of the adductor muscles, and now the limb was in a better position, and almost straight. On the left side the disease had been more active, and the limb was much wasted. Sir W. Fergusson had excised the head of the femur a few weeks ago, but inflammation had kept up, and now he intended making some exploratory incisions to see if there was any dead bone which could be removed. This was done, and the sinuses of a large abscess were laid open freely, but no dead bone was met with.

Necrosis of the Ilium.—The next patient was a young man, aged seventeen, who fourteen months ago had some inflammation about the left hip, in the iliac region. Whether the disease had originated in the hip-joint or not Sir W. Fergusson could not say, but anchylosis had taken place between the femur and the ilium; a great deal of new bone had formed around the parts, causing great thickening. It was most probable there was some necrosed bone that required to be removed. Several sinuses were laid freely open, and Sir W. Fergusson then found that a large piece of bone, about two inches square, was loose and able to be removed; after this two other large pieces, together making up part of the iliac crest. This, Sir William Fergusson remarked, was a good case for operative interference, as now the dead bone had been removed the patient would soon be about, whereas had it been left to nature, several years would have been required before the bone could be got rid of.

Cleft Palate.—This patient, a young man, had been operated upon three or four times by Sir W. Fergusson. First the soft palate had been brought together, then the cleft in the hard had been closed over. There was now a small aperture just behind the alveolar ridge. The edges were pared and brought together by one suture. Sir W. Fergusson remarked, after describing the operation he had performed, that he was never very sanguine about such cases, as the part often failed to unite; but it was more favorable in this case as the man had been operated upon several times before, and through this the tissues had become indurated, and so presented a firmer hold for the suture.

Division of Third Branch of Fifth Facial Nerve for Tic.—On the previous Saturday Sir W. Fergusson divided the third branch of the fifth facial nerve on the right side, in an elderly man who for some years had suffered from tic doloreux. The man had been under his care for many years, living in the country, and coming up to town when the pain was excessive; he had operated upon him five times previously. It is now sixteen months since the nerve was last divided. On the present occasion not only was the nerve cut through, but a small gouge was put into the dental foramen, and worked about for some time, so as to completely destroy the nerve. Sir W. Fergusson remarked that relief was not immediate; the pain usually continued a short time, and then suddenly ceased, and did not return for a certain time; the intervals gradually lessened after each section. There were other modes of accomplishing the same object, but he preferred this one, and now anæsthesia could be used the operation was not a severe one. As a proof of the relief obtained he stated that a patient on whom he had operated some years ago gained two stone in weight in the six months' interval before the pain again returned.

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CHARING-CROSS HOSPITAL.

HYSTERICAL PTOSIS.

(Under the care of Dr. Silver.)

There has lately been under treatment at this hospital a young woman suffering from hysterical ptosis of the left eyelid. The patient has been cured in a somewhat novel manner by Dr. Poore, who is at present in charge of Dr. Silver's wards. She was told that if her right eyelied was closed at once the left would be raised; then Dr. Poore drew down the right eyelid, and the woman at the same time opened the left eye. Dr. Poore then said that all that was needed to perfect the cure was, that the right eye should be kept closed for a short time, and proceeded to do so with a strip of plaster and a bandage. After a few days the dressing was removed. The patient has not complained since of the drooping of her left eyelid.

A MODE OF INTENSIFYING HEART-SOUNDS.

(Under the care of Dr. Poore.)

In the out-patient room we also witnessed, among Dr. Poore's patients, a novel application of acoustic principles for the rendering of sounds made in the chest audible to those standing near the patient. The subject of this experiment was a man of middle age with a very loud basic diastolic cardiac murmur. The murmur was one of the loudest we have ever heard. The patient, being stripped, was made to lie flat on his back on a long wooden bench, or couch, without cushions. Dr. Poore then borrowed a walking-stick from one of the students and placed the lower end of it on the sternum between the third costal cartilages. On the opposite end was balanced the sounding-box of a guitar, the aperture being directed towards the thorax of the patient. The cardiac sound became immediately audible to everybody standing round (some nine or ten persons) provided the ear was kept below the level of the guitar. The patient himself became extremely conscious of his murmur, and seemed very much amused with the experiment. There was a considerable amount of cardiac thrill, the vibration of which was felt by everybody on placing a finger on the guitar or the walking-stick. Dr. Poore merely regards the experiment as a curiosity, and does not believe that an apparatus similar to the one he extemporised would be of any use for purposes of diagnosis.

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DEVON COUNTY ASYLUM.

TWO CASES OF ERYSIPELAS AMBULANS VEL ERRATICUM.

(Under the care of Dr. Saunders.)

The following notes and remarks have been forwarded us by J. Wilkie Burman, M.D. Edin., late assistant medical officer:—

Case 1.—No. in register, 2995; female, married, aged fifty-nine. Admitted in 1865. In September, 1870, erysipelas commenced in the face, spread from thence to the scalp, thence down the back of the neck to the chest and arms, and gradually extended downwards over the abdomen to the distal parts of the lower extremities. The eruption faded away, and was followed by desquamation, in the order of its appearance, and finally disappeared at the feet, where several small abscesses formed. The illness extended over five weeks, patient lying for some considerable time in a low typhoid state, and at times in an apparently hopeless condition, but by the free use of stimulants and nutrients a slow and tedious convalescence at last commenced, and eventuated in complete recovery.

Strong nitrate of silver was applied on several occasions at the margin of the progressing erysipelatous blush, but without effect. The medical treatment was principally by perchloride of iron, chlorate of potash, and simple salines, such as potus imperialis *ad lib.*

Case 2.—No. in register 3049; male, single, aged fifty-one; admitted in 1865. In February, 1871, he was attacked with erysipelas, which, commencing on the face and scalp, pursued a course very similar to that in Case 1, differing, however, in the non-formation of abscesses in the feet, and in the occurrence of general dropsy without albuminuria. His illness was prolonged for a period of two months, and his life was frequently despaired of, the typhoid condition being both more marked and persistent in this than the former case, and the patient remaining for many days with a dry glazed tongue, quick and weak pulse, and hot and dry skin. He was only "pulled through" by a lavish use of stimulants and concentrated nutrients. The medical treatment was the same as in Case 1, with the exception of special treatment for the dropsy. With the subsidence of the eruption the dropsy gradually disappeared, and, though much reduced in weight, the patient ultimately made a good recovery, losing, however, a considerable quantity of hair from the scalp.

Remarks.—This very peculiar and dangerous form of erysipelas has been little more than mentioned as one of its varieties by most authors on surgery. The best accounts of it are given by Dr. Pirrie in his "Principles and Practice of Surgery," p. 82, and by Campbell de Morgan in Holmes's "System of Surgery"; but in each of these cases merely a short paragraph is dedicated to its consideration. So far as the writer can ascertain, there are only two similar cases on record—viz., La Motte (Cooper's "Surgical Dictionary," article Erysipelas), and the other by M. Vidal ("Médecine Opératoire"). Besides the general progressive nature of the disease, it is peculiar on account of its very severe and prolonged constitutional symptoms, which are of a low type, and call for an eminently sustaining treatment. The local symptoms are not of much moment, though the whole thickness of the skin is involved; for the worst topical occurrences in the two cases above described were small abscesses in the foot of one, and small bullæ here and there on the skin of both as the disease progressed.

HOPITAL DE LA PITIE.

ON THE USE OF PLASTER SPLINTS IN REMEDYING DISPLACEMENTS IN FRACTURES IRREDUCIBLE BY OTHER MEANS.

(Under the care of Dr. Labbé.)

All surgeons know how difficult it is sometimes to remedy certain cases of oblique fracture of the lower third of the leg, in which the upper fragment projects under the surface. Various apparatus have accordingly been devised for the purpose of preventing this displacement. In France, Malgaigne in such cases used to employ his metallic point, which was fixed into the upper fragment so as to exert a certain degree of pressure, and prevent any fresh displacement. The fixing of the metallic point was attended with some inconvenience; besides, it is a special instrument and not easily procurable. We were therefore struck with the advantages afforded by an ingenious contrivance which we saw Dr. Labbé employ with success a short time ago in his wards at the Hôpital la Pitié.

The patient was a woman aged forty. Whilst in a state of intoxication she had been run over by a gig, and was at once conveyed to the hospital. Fracture of the lower third of the leg was found to have occurred, with very marked displacement of the upper fragment, which projected under the skin and threatened to tear through. A splint was immediately applied by one of the house-surgeons.

As the woman was laboring under delirium tremens, it was found necessary to use the strait-waistcoat, and strong doses of opium were administered. However, in consequence of the restlessness of the patient the upper fragment of the tibia projected more and more under the skin, so that it became necessary to use some means to prevent its issue through the surface. Accordingly the apparatus was modified, and a cushion placed under the heel, but to no effect, and there was imminent danger of the fragment lacerating the skin. It was at this time that Dr. Labbé first saw the case, and with the view of definitively remedying the displacement and preventing deformity, he applied a plaster splint in the following manner:—Reduction was first performed with the greatest care. As soon as this was completely effected the plaster splint was applied so as to cover the sole of the foot, the heel, and the whole of the posterior surface of the leg. The assistants were then requested to continue extension, counter-extension, and perfect apposition of the fragments, until the plaster had become hard. At the end of half an hour the apparatus was sufficiently solid. From that moment all anxiety about a breach of surface ceased, and notwithstanding the disorderly movements of the patient, which continued two days longer, consolidation took place so perfectly that it was quite impossible to make out the seat of the fracture.

The Physician-General of the Swiss Federal Army has been required by the military authorities to supply returns of cases of damage to the eyes and surrounding parts of the face caused by the discharge of breech-loaders.

Editorial.

ASYLUMS FOR DRUNKARDS.

The propriety of establishing a legislative control over habitual drunkards, analogous to that which has been long established over the insane, is a question on which there is great diversity of opinion. We ourselves have always supported the principle with which the name of Mr. Dalrymple has of late become identified; and, although we are quite conscious of the objections that may be urged, with some show of reason, against his proposals, and although we fully sympathise with those who would surround personal liberty with all possible safeguards, yet we are convinced that the opposition rests mainly upon two bases—upon ignorance of the magnitude of the evil that is to be overcome, and upon ignorance of the character and powers of resistance of the wretched examples of intemperance whom the law is asked to protect againt themselves.

Whatever may be the character of the opposition, there can be little doubt that it will be for a time successful; for public opinion is not yet ready to accept the recommendations contained in the Report of the Select Committee upon the question. The statements made before this committee must be repeated again and again, and must be pressed home to the public mind through various channels, before it will be generally perceived that there is no analogy whatever between the habitual drunkard and the chance victim of a carouse—no possibility that provisions designed for the restraint of the former could ever be put in force against the latter.

In this process of education, so to speak, a very important part might be played by asylums or institutions for the reception of drunkards desirous to reform. Such institutions, are by no means unknown in great Britain; but they have not assumed a development at all approaching that which Mr. Dalrymple has described as existing in the United States, and people in general know very little about their operations. They are strictly private establishments, and their proprietors or managers have to contend against the difficulty of having no legal control over the inmates. Now moral suasion is all very well in its way; but it wants some more powerful influence at its back. The normal state of the habitual drunkard is to spend his life in periods of craving and indulgence, followed by periods of penitence or remorse. While these last he is very amenable to treatment; but as soon as the craving returns he will disregard all promises of amendment, and will use any or all means of satisfying his desire. How far he is then a responsible agent is a curious and interesting question, but hardly a practical one; for it is quite plain that he can only be dealt with beneficially on the hypothesis that he is *not* responsible. Now, in order to render asylums really useful, this hypothesis must be confirmed, for a certain period at least, by law. A drunkard who, in his lucid or penitent intervals, desired to reform, should have facilities afforded him for making a declaration to that effect before a magistrate; and should be allowed to surrender his personal liberty, beyond recall, for the purpose of treatment, for a time not exceeding six or twelve months. During such time he should be liable to incarceration under lock and key, or within the limits of an establishment, if such a measure were deemed necessary for his recovery.

If such powers as these were conferred, the public would necessarily require some guarantee against their abuse. As the drunkard would himself be required to sign the document on which his detention would be justified, and would be interrogated by a magistrate with regard to his comprehension of its meaning and effect, there would be no room for fear that it could ever be made a means of putting an inconvenient person on one side for a few months. The practical danger would be lest the establishment should fall into the hands of teetotallers or other enthusiasts, and lest the inmates should be injured in health by the measures taken to reclaim them. The arrival of the period of craving, especially for the first time, would call for the most careful medical as well as dietetic management; and we doubt whether the cases in which it occurs are yet sufficiently well understood for even the principles of such management to be matters on which there is complete accord in the profession. The obvious corollary is, that institutions for drunkards would require a resident medical superintendent, capable of observing and appreciating the actual state of every patient, and of profiting by the experience which he would in time acquire. The uses of opium, of chloral hydrate, of various nervine tonics, and of physical fatigue, would need full and careful investigation; and it cannot be doubted that much would soon be learned with regard to the best means of warding off the craving for drink, of which we believe the drunkard is always conscious before it acquires sufficient power to overcome his feeble efforts at self-restraint.

We would therefore urge upon Mr. Dalrymple, if Parliament should be indisposed to concede such legislation as his own knowledge and experience would suggest, that the proverbial "*reculer pour mieux sauter*" would admirably meet his case. We would urge upon him to introduce a Bill giving powers to a company or body of subscribers to establish a drunkards' asylum on a proper scale, under medical management and control, and fitted with every appliance for its work. The same enactment should enable the drunkard, during an interval of sobriety, to deprive himself of his liberty of action, and to submit himself to the authority of the asylum superintendent. There are large numbers of inebriates, originally of the higher classes of society, now a burden and terror and disgrace to their relatives, many of them pensioned off in remote Welsh and Scotch villages, or otherwise put out of the view of those among whom they were once entitled to move. Upon many of these people, and upon many now descending towards their level, sufficient pressure would be put to induce them to enter an asylum such as we have indicated; and, if any fair percentage of them could there be cured and restored to the world as sober and trustworthy persons, the argument in favor of the compulsory seclusion of such as were unwilling to submit would be immensely strengthened. It is quite certain that our drunkards cannot be suffered to remain, as at present, utterly uncared for by society; and we fear society is not yet prepared to discharge its full duty with regard to them.

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RESEARCHES ON THE MOVEMENTS OF THE UTERUS.

D. L. Oser and W. Schlesinger have undertaken a series of investigations into the characters of the uterine movements, which, they observe, have been but little studied of late years, and with improved means of research, whilst those of the intestines, ureters, and other organs have been carefully examined. Their experiments were performed upon for the most part 20 curarised animals, and are recorded in the last part of Stricker's *Med. Jahrbucher*. At the commencement they found not unfrequently that a stimulus which in one animal excited the most violent uterine contractions, in another was apparently wholly inoperative, and this not only in animals of different genera, but in those of the same species. In rabbits, for instance, it sometimes happened that the slightest mechanical stimulus called forth active contractions, whilst in another animal no response occurred to the strongest electrical currents. They find that the animals best adapted to exhibit the movements are young but sexually mature rabbits which have not yet been impregnated. In these the uterus, when the abdominal cavity is laid open, appears as a flat, band-like, pink-colored organ, at perfect rest, and but rarely exhibiting spontaneous movements. Their first experiments were made to determine the effect of arrest of the respiration. Krause, Mayer, and Basch have all shown that asphyxia induces movements of the intestines; but it has not hitherto been shown that a similar effect is produced upon the uterus. Oser and Schlesinger's experiments, however, show that in from ten to thirty seconds after suspension of the respiration contractions are perceptible, commencing from the tubæ and cervix, which in a few seconds more extend over the whole uterus. This organ becomes pale, cylindrical, and rigid, and moves downwards and towards the middle line; the cornua raise themselves in an arched manner, and, intercoiling, almost form a ball. The contractions increase in vigor with continued arrest of the respiration for some minutes. In a second set of experiments the effects of compression of the aorta were investigated, and it was ascertained that general contractions of the uterus occurred in a period varying from eighty to one hundred and twenty seconds after. Rapid abstraction of blood from the system generally produced the same effect. It was remarkable also that contraction of the whole uterus resulted in a few seconds from suddenly applied compression of the four cerebral arteries, which showed that the presence of imperfectly arterialised blood in the uterus itself was not a necessary condition for the production of the contractions. Küssmaul and Tenner long ago described a somewhat similar experiment, but gave a different explanation of the phenomena observed. In a pregnant rabbit they exposed and compressed the carotids and vertebrals. Extrusion of the fœtuses occurred, but it was accompanied by convulsions, and they attributed the delivery to these rather than to any contractions of the uterus. MM. Oser and Schlesinger further found that, after section of the spinal cord in the cervical region, the contractions of the uterus no longer occurred more rapidly after arrest of the respiration than after compression of the aorta, in each case supervening in about the same space of time—that is to say, in about one hundred seconds. After section of the cord, neither general loss of blood nor arrest of the flow of arterial blood to the brain produced any perceptible contractions. It was ascertained that no motor stimulus is conducted to the uterus from the brain through the vagus or sympathetic nerves, since section of these nerves was without influence on the movements, whether the cord was divided or not. The authors of the paper do not attempt to fix the situation of the centre for the uterine movements, but they point out that their experiments prove that it lies at a higher point than that at which they divided the spinal cord—namely, between the occiput and atlas; and they think it is probable that it occupies some part of the medulla oblongata. At all events, it is unlikely that it is situated in the spinal cord, since the experiments of Küssmaul and Tenner and Schiffer show very clearly that the circulation of dyspnœic blood (a term that implies no theory as to whether the phenomena produced are due to the absence of oxygen or the presence of carbonic acid) through the cord is not followed by any symptoms of irritation—as by convulsions,—but by rapid paralysis of the posterior extremities, the paralysis appearing in the course of a minute or a minute and a half. But in Oser and Schlesinger's experiments the uterine contractions did not occur in any case till after the lapse of a minute; and hence, if it were maintained that they were due to the passage of dyspnœic blood through the cord, it must also be admitted that such blood acts coincidently as a stimulus to one spinal motor centre and as a paralyser to another. It is much more probable that they are due to the passage of such dyspnœic blood through the medulla oblongata. Other considerations lead them to the same conclusion.

Medical Annotations.

"Ne quid nimis."

DR. PETTIGREW ON CIRCULATION.

Dr. Pettigrew delivered his fourth lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," on the 21st May. The lecturer recapitulated at considerable length the structure and forces engaged in the circulation of plants. The interlacing and alternating syphon tubes through which the nutritious juices pass in plants, he explained, differed from ordinary syphons in this, that they were capillary and had porous walls—that is, walls which permitted fluids to pass through them either from without or from within (as in absorption and evaporation), but effectually prevented air entering from without, which would destroy their syphon action. He illustrated this by bending a glass tube upon itself several times to form a compound syphon, the extremities of the syphon being directed in opposite directions, like a number of s's united together. He then perforated the syphon so constructed in several places, and covered the apertures with a portion of vegetable or animal membrane, through which evaporation or absorption could go on, but which effectually prevented the entrance of air from without. This novel form of syphon, he explained, corresponded in its details to the syphon formed by the vessels and intervascular spaces found in the higher plants. By its aid he was enabled to show that evaporation might go on at one point, say in the leaf, and absorption at another, say in the root (or the converse)—evaporation drawing up the current in the vessel or intervascular space, and acting as a *vis a fronte*, absorption and endosmose pushing up the current in the same vessel or intervascular space, and acting as a *vis a tergo*. (The heart also possesses this *pulling* and *pushing* power.) He thus proved that the condition of *dryness* and *wetness* can be made to influence the circulation, the two forces working in the same direction. He also pointed out that when either moisture or dry air is applied to the leaves or roots of a tree, it facilitates the circulation so long as the tree contains tenacious juices, the dry air in seasons of drought *drawing* the juices out of the tree in opposite directions by evaporation, the moisture in wet seasons *pushing* juices into the tree in opposite directions by absorption and endosmose. The lecturer proceeded to explain that evaporation and absorption enabled fluids to invade and pass through the tissues, these being nourished and washed out at the same time. He stated that precisely the same thing happened in the animal tissues; that the tissues of an animal had a circulation of their own, apart from the general circulation, over which the heart presides—the heart being an engine provided for conveying the blood long distances, and in this sense only an auxiliary of the circulation. His remarks under this head were particularly interesting. He observed—

"It is an error to suppose that the circulation in animals is carried on exclusively by the heart. The tissues do their own work, or rather the particles of the tissues are so arranged that they permit certain physical forces, such as capillary attraction, osmosis, evaporation, respiration, chemical affinity, &c., to do the work for them. The organic kingdom avails itself of inorganic power. The capillary vessels of animals form syphon loops, as in plants, the animal syphon loops being porous in the same sense in which the vegetable ones are. They are therefore capable of transmitting a continuous stream of fluid in a given direction, and of absorbing and evaporating at innumerable points, the absorption and evaporation facilitating instead of retarding the general circulation. That the tissues respire has been abundantly proved by Spallanzani. He finds that all the tissues take in oxygen and give off carbonic acid precisely as the lungs do; so that animals (and plants) breathe at every pore; this universal respiration contributing to the general circulation. That the tissues effect the circulation within themselves is proved by the fact that the circulation within them continues so long as they live and maintain their temperature—that is, after the heart has ceased to beat and the body is to all intents and purposes dead."

The lecturer next proceeded to show that there was no such thing as absolute rest in the universe, and that there was what is equivalent to a circulation in metals, Seebeck having shown "that when dissimilar metals are made to touch, or are soldered together and heated at the point of contact, a current of electricity flows through the metals employed, which current continues as long as an *increasing* temperature is gradually pervading the metals, ceases when the temperature is *stationary*, and flows in a contrary direction with the *decrement* of temperature." This afforded him an opportunity of explaining that, in order to understand the movements of the heart, it is necessary to understand the movements of its ultimate particles. He concluded by introducing his audience to the circulation as it exists in the lower animals, this forming the subject of his next lecture. The lecture was illustrated by a large number of diagrams and experiments.

On the 31st May, Dr. Pettigrew delivered the fifth of a course of lectures on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man." Of absolute rest, the lecturer observed, nature gives us no evidence. All matter, as far as we can ascertain, is ever in movement, not merely in masses as in the planetary spheres, but also molecularly or throughout its most intimate structure. This led him to speak of force as visible and invisible; visible force being that which can be detected by the eye—invisible force that which alike eludes the naked eye and the microscope. He referred at this stage to the so-called Brownonian movements, and to the movements of bacteria, vibrios, the amœba, and cilia, the latter displaying rhythmic movements, which remarkably resemble those of the heart. The visible forces, he remarked, manifested themselves in vessels, pulsatile cavities, and hearts; the invisible force in nutrition and other changes, in absorption and evaporation, in osmosis, capillary attraction, &c. He attached considerable importance to the effect produced on the circulation by the invisible forces, sagely remarking that they are entitled to take precedence of the visible ones, as the circulation can be carried on in plants and in certain animals where neither contractile vessels nor hearts are present. He wished his hearers to keep the invisible forces constantly in view, as there was a tendency to overlook them in the gradual development and differentiation of the visible forces. He gave due prominence to the invisible forces by stating that by much the larger portions of plants and animals consist of fluids varying in density, and that the fluids, &c., taken in by plants and animals as food could only be made to commingle with the original fluids by absorption, evaporation, osmosis, capillarity, &c. This circulation in the tissues has an independent existence, and precedes the general circulation. The blood moves before the heart is formed, and towards that organ instead of away from it. The capillaries have also a circulation of their own; the circulation in these minute vessels going on when the heart has ceased to beat, and sometimes slowing, stopping, and reversing when the heart is beating vigorously. The vessels and heart are therefore to be regarded as auxillary forces. The invisible forces (absorption, evaporation, osmosis, capillarity, &c.) work at short distances; the visible forces (vessels, pulsatile cavities, hearts, &c.) at long distances—indeed, the visible forces are only employed when the animal has attained a certain degree of differentiation; the invisible forces being employed equally in the lowest and highest. The lecturer drew a parallel between the circulation in the plant and leech, and showed that in both the circulation consisted of ascending, descending, and transverse currents. The circulation of the leech differs from that of the plant in being carried on to a great extent by the rhythmical opening and closing of its longitudinal vessels. The lecturer insisted upon the study of the soft tissues. He believes that protoplasmic movements, the movements of jelly-like masses and those of involuntary muscles, foreshadow all other movements, those of voluntary muscles included. These movements take place in all animals, and are of primary importance. The involuntary movements occur in the œsophagus, stomach, intestine, uterus, heart, and blood-vessels; they do not require the presence of bone for their manifestation. The lecturer gave it as his opinion that the heart of the mammal forms the type for the whole voluntary system of muscles. He illustrated his meaning by stating that if the element of bone was eliminated from the chest, the muscular arrangements of the chest would be found identical with those of the ventricles of the mammal. The muscles of the chest correspond in their general directions with those of the abdomen and extremities. He then spoke of the heart beating within a greater heart, the chest or thorax; and so also of the viscera within the abdomen. The function of the heart and chest (or the lungs within the chest) was to bring the blood and the air face to face, and to permit a free interchange. He explained that the circulation in animals must be studied in connexion with digestion and respiration; the ingested food forming blood, which to be nutritious had to be purified by the air. The lecture was illustrated with numerous diagrams and a large number of experiments—notably one, showing that the rotation or gyration of the cell contents of plants may be produced artificially, and by purely physical forces—viz., osmosis, capillarity, and evaporation.

Dr. Pettigrew delivered the sixth of a course of lectures on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," on the 7th June. The lecturer described in succession the circulation as it exists in the star-fish, the holothuria, the spider, the insect, the lobster, the brachiopoda, the gasteropoda, the cuttle-fish, the fish, the proteus, axolotl, siren, frog, lacerta ocellata, python, crocodile, &c. In the star-fish, the intestinal vascular system consists of loops, from which arteries and veins are given off, these being connected with a dilated pulsatile canal, which may be regarded as a rudimentary heart. Similar remarks are to be made of the holothuria. The lecturer observed that hitherto he had been dealing principally with the invisible forces concerned in the circulation—viz., absorption, capillarity, osmosis, evaporation, chemical affinity, &c. These occasion a circulation of nutritious juices, independently of vessels and hearts, alike in plants and animals. Vessels and hearts are the minor rather than the major factor in the circulation. They, however, constitute the visible forces of the circulation in animals, and have from this circumstance attracted a degree of attention to which in reality they are not entitled. The lecturer explained that the circulation within the tissues constitutes the only real circulation; that within the vessels and heart being accessary. This is in some senses a new doctrine, but it has much to recommend it, and is deserving the attention of physiologists. The gradual development of bloodvessels, pulsating sacs, and hearts in the lower animals is especially interesting when viewed in connexion with the differentiation of the other structures; the bloodvessels and hearts being to the animal economy what the waterworks and waterpipes are to a city and its inhabitants. A drawwell or a pump suffices for a cottage, but an engine, waterworks, and feeders are necessary for a populous city with its numerous manufactures. In the spider a large dorsal vessel, which is dilated posteriorly, makes its appearance, this giving off branches which ramify through the body. The venous blood is collected in open sinuses on the abdominal surface of the animal. As yet there is only an arterial system. In insects the dorsal vessel is divided into a series of swellings, each of which is furnished with valves and endowed with a pulsatile power. The swellings open and close alternately, and force the blood forward by a swallowing or wave-like movement, similar to that performed by the œsophagus of the leech when sucking and swallowing blood. The heart of the embryonic chick resembles the dorsal vessel of the insect, inasmuch as it consists of a tube with three constrictions in it—one constriction representing the future auricle, another the future ventricle, the third corresponding to the arterial bulb. The heart of the lobster appears in the shape of a muscular oval-shaped cavity or ventricle, situated on the dorsal aspect of the animal. It forces the blood directly through the arteries, and indirectly through the veins, so that it is to be regarded as a systemic heart, as contra-distinguished from a pulmonic heart. The brachiopoda have two aortic hearts—i. e., two pulsating cavities—situated on the principal artery of the body. In the cuttle-fish there is a muscular systemic heart, to which may be added two dilated portions in the bloodvessels which lead to the branchiæ, and which may be regarded as the harbingers of the pulmonic heart. The connexion between the circulation and the respiration is now becoming defined. In the fish, the heart consists of two cavities (an auricle and a ventricle)—the auricle receiving the venous blood from the liver and system generally; the ventricle forcing the blood through the capillaries of the gills, where it is aerated and converted from a purple to a bright-red color. In the axolotl (frog and aquatic reptiles) the heart consists of cavities—two auricles and one ventricle; the left auricle receives the arterial or red blood from the lungs, the right auricle the venous or dark blood from the system, the two kinds of blood being mixed in the ventricle. This constitutes the mixed circulation of reptiles as a class. In the lacerta ocellata, a rudimentary septum makes its appearance in the ventricle, so that in this curious creature the heart virtually consists of four compartments—two auricles and two ventricles. The systemic and pulmonic heart as it exists in the bird and mammal is now distinctly foreshadowed. In the heart of the crocodile (Crocodilus Lucius) the septum ventriculorum, as was pointed out by Hentz and Meckel, is quite complete, so that the ventricles of this animal are as perfect as either those of the bird or mammal. The lecturer endeavored to show that in proportion as the tissues become differentiated, so the machinery of the circulation becomes more and more complex. The lecture was illustrated by a large number of beautiful diagrams, and by several original experiments. Dr. Pettigrew intimated that his seventh lecture will be on the fœtal circulation.

THE STRUCTURE OF NORMAL AND VARICOSE VEINS.

Dr. S. Soboroff has lately undertaken a series of researches on the structure of healthy and varicose veins, the results of which possess considerable interest. He had at his disposal eight specimens of varicose veins, which, with only one exception, were accompanied by pathological changes in the vena saphena. These were all capable of being arranged in two groups: in both the lumen of the vessels was greatly enlarged; but in one the walls were thickened, in the other they were attenuated. The preparations seem to have been prepared with great care, some being silvered, to show the epithelium; others subjected to the action of chloride of gold, chloride of palladium, and chromic acid. The principal fact that he has definitely ascertained is that a dilated venous trunk, even when possessing almost perfectly transparent walls, contains colossal muscular fibre-cells. These are so large that when first observed they were regarded as fasciculi of unstriped muscle, but more careful examination, aided by staining experiments, showing the well-marked outline of the cells and the rod-shaped nuclei, clearly demonstrating that fibre-cells alone were under the microscope. The adventitia of the thin dilated veins was thin and but sparingly supplied with vessels, and the epithelial lining of these attenuated vessels was apparently continuous and unaltered. In the thickened and dilated veins, on the other hand, the adventitia was hypertrophied, and the vessels distributed to it were large and numerous.

THE RATE OF GROWTH OF THE NAILS AS A MEANS OF DIAGNOSING CERTAIN FORMS OF PARALYSIS.

Dr. Weir Mitchell, in a short paper communicated to the *Philadelphia Medical Times*, states as the result of his observation that the growth of nails is usually retarded more or less in all palsied limbs, whether the palsying cause be spinal, cerebral, or belonging to a nerve trunk. He is not certain that there is any *complete temporary* arrest of nail-growth in spinal or peripheral disease and injury. In cerebral palsies, whether from clot or embolus, there is an *entire* cessation of nail-growth on the palsied side. Usually, when they begin to grow again, it is a sign that the power of movement will also improve in a few days. The rate of growth slowly increases, but it generally requires four of five months for such nails to produce an entire length from matrix to free edge. To study the change he stains the nails of both sides with nitrate of silver or nitric acid, the latter being preferable, as it soaks through the whole thickness of the nail. The process of staining, however, is not essential, except for comparison, because the line of arrested growth is marked by a deep groove, which may be seen for months as it passes down the nail. The palsy need not be complete to cause this arrest of growth. In opposition to the fact that in all sudden cerebral palsies the nails cease to grow, is the fact that in hysterical palsies of one limb or both, whether paraplegic or hemiplegic, the rate of nail-growth is unaltered.

VARIATIONS IN WOMEN'S MILK RESULTING FROM INSUFFICIENT FOOD.

Dr. Decaisne lately reported to the Académie des Sciences the results of his observations on forty-three women who suckled their infants on insufficient food during the siege of Paris. Such women either produced abundant and good milk and their children throve, whilst they themselves became greatly emaciated; or they produced but little milk and that of a bad quality, so that the children throve badly, and for the most part suffered from choleraic diarrhœa; or, lastly, they produced scarcely any milk, and the children died. The following table shows analyses from the three various types:—

		Butter.	Casein.	Albumen.	Salts.	Sugar
1.	Fasting	3·10	0·24	2·20	0·20	6·24
	Well-fed	4·16	1·05	1·15	0·30	7·12
2.	Fasting	2·90	0·18	1·95	0·16	7·05
	Well-fed	5·42	1·15	0·95	0·25	7·05
3.	Fasting	2·95	0·31	2·35	0·31	5·90
	Well-fed	4·10	1·90	1·75	0·31	5·95

It is interesting to notice that the albumen and casein taken together are nearly the same both in the fasting and the well-fed state, though they seem to have an inverse relation to one another.

TESTS FOR QUININE AND MORPHIA IN THE SAME FLUID.

A RECENT number of the *Pharmaceutical Journal* contains a translation of an important paper by Professor Flückiger on the Tests for Quinine and Morphia when contained in the same fluid. The most characteristic test for quinine is the formation of the green compound called thalleioquine, which is produced if solutions of the alkaloid or of its salts are mixed with chlorine water and then a drop of ammonia is added. Morphia treated in the same way shows a red hue, very quickly turning dark brown. The green color with quinine appears when there is not more than 1-5000 of the alkaloid in solution. The brown color with morphia requires that at least 1-1000 of that alkaloid should be contained in the fluid. But Prof. Flückiger points out that the test for quinine fails if there be more than 1-1000 of morphia present, even if the quinine be in considerable quantity; the green color of the latter being concealed by the dark dingy-brown color of the morphia test. Prof. Flückiger recommends, in the first place, that the one test with chlorine and ammonia should not be alone relied upon, but that the nitric or iodic acid tests for morphia should always be tried; and, in the second place, his experiments have shown that in very dilute solutions of quinine, bromine vapor brings out the green tint much better than chlorine.

INTERNAL USE OF CARBOLIC ACID.

Now that this acid is being tried internally, we should ascertain in what doses it may be poisonous. MM. Paul Bert and Jolyet, of Paris, have undertaken experiments to make out this point. Between forty-five and sixty grains will kill a dog of large size; nor should it be concluded that man could bear a dose in proportion to his weight compared to that of the dog, as thirty grains of hydrochlorate of morphia have been injected into the jugular vein of a dog without killing him. Of course one-fourth of this dose would kill a man. The above-mentioned authors state that carbolic acid is a powerful poison, which, very imprudently, is left in the hands of anybody, either in solution or in the solid state. The former is the most dangerous, as some weak solutions for internal use are sold, as well as very strong ones intended for external use. Thus mistakes may easily occur. MM. Bert and Jolyet find that carbolic acid acts like strychnine on the excitability of the spinal marrow. It increases its sensibility, like strychnine, at first; but it diminishes that sensibility, or completely abolishes it, when the convulsive stage has exhausted the medulla. The phenomena resulting from carbolic acid are said by our authors to be quite similar to those produced by chloroform, chloral, ether, woorara, and the section of a motor nerve.

SIR WILLIAM GULL ON TYPHOID FEVER.

THE announcement that Sir William Gull would deliver a lecture at Guy's Hospital on Typhoid Fever naturally brought together a large audience on Thursday, June 20th, composed chiefly of present and past students of that school. The following is a full abstract of the discourse.

Sir William began with the remark that it is about two hundred and fifty years since one of the kings of England died of ague; but now the improved condition of agriculture and drainage in this country is such that ague has become rare, and certainly very few die of it. He then expressed his belief that two hundred and fifty years hence few will die of typhoid fever, inasmuch as this disease is as preventable as ague. Typhoid fever is, *par excellence*, to be ranked amongst "diseases," because it is caused by a virus—a virus of nature—which may get into the healthy body, and increase in it, and destroy it. It is an accidental condition, and not one of the ordinary processes of nature. Therefore we ought to rise up in arms against it, as we Englishmen would rise up in arms against any enemy that should attack us or threaten us.

Sir William said that he was not going to give a lecture on typhoid fever, as such a thing would be impossible in the time allotted; but he appealed to those present, as Guy's students, to sympathise with him, as a Guy's man, in late conditions which have given rise to the increased attention to the subject of typhoid fever. Many diseases that were formerly very fatal are recoiling before the advances of civilisation—to wit, the plague, ague, and even cholera. Typhoid fever is stated to kill 17,000 a year in England; how great, then, must be the number attacked! It stood among the preventable diseases, and it was important, therefore, to know how it originates. There is no scientific theory, but there is a good working theory on the point. The origination of the disease is, somehow or another, connected with drainage. It has, therefore, been called the filth fever; hence to get rid of the filth is to get rid of the fever. It seems as if this really is so, for Millbank Prison was infested with typhoid and dysentery, but now the water-supply has been changed, and the drainage attended to, and these diseases have almost entirely disappeared. No one can approach a case of typhoid fever without paying some attention to hygiene. It is no use tinkering with the disease if one does not try to prevent it, and it no doubt may be prevented. The theory is that it is connected with germs which get into the blood; we know nothing about these germs—the air is full of them. There is an idea that they are imbibed by drinking water, and that they increase and multiply within the body. Although this has not been demonstrated, yet it is a good working theory.

The first thing then to be done is to find out what will prevent the disease. Some may say, "Tell us what will cure it;" but this is not of such importance as the prevention. Sir William next said that he liked the name *typhoid* because the disease resembled *typhus*, and yet it was not typhus. It is an error to suppose that there are symptoms of typhoid fever essentially, for the poison may get into the body, and destroy life, without setting up any symptoms of typhoid fever proper. Some years ago a case was taken into

Guy's Hospital, the patient having become suddenly ill; and shortly after admission he died, but the cause of death was not suspected till a post-mortem examination revealed perforated typhoid ulcers of the intestines. This patient had gone about his work regularly up to the day of his death. The next subject is the diagnosis of this disease. This is sometimes at first uncertain; patients rarely come till they have been ailing some time. The furred tongue, headache, malaise, &c., belong to other diseases besides typhoid fever, therefore the term typhoid is a good one, because the symptoms somewhat resemble those of typhus. Many other names have been given to this disease, but the one most generally adopted is typhoid fever. It has been called pythogenic fever, from its supposed origin from filth; but it is not advisable to accept this name, as at some future time it might be necessary to change again.

The history of an attack of typhoid fever is somewhat obscure. The poison lurks in the body some time before the real onset of fever; it may be as long as five or six weeks. The disease then begins insidiously and irregularly; the prodromata are uncertain. Where does this poison work? Is it in the intestinal tissue, or in the mucous membranes, or in the absorbent system? We do not know; we say that it is in the blood. Where are these lurking beginnings? These are probably in the intestinal system, for we find digestive disorders and disturbance of the bowels in the first instance. It is, however, very difficult to say when the disease begins; but if the ordinary course of health be represented by a straight line, we shall find at the commencement of the disease an abrupt deviation from this straight line, which deviation goes on increasing till the fourth to the sixth day; consequently, we find that on the fourth day the patient is ill, and on the fifth or sixth day very ill and obliged to keep his bed. It is about this time that we first see these patients as a rule, and how are we to tell that they have got fever? Well, by the little instrument called the thermometer. Now, if on even the first day the temperature be normal in the afternoon, there is no fever. In typhoid the temperature increases 1·5° to 2·0°, or even 2·5° F. in the first twenty-four hours. If a person who has been exposed to typhoid fever have a headache, and his temperature be 101·5° on the first day, it may be typhoid; but if, as is sometimes the case in persons who have been nursing, the temperature be below normal, or even if it be normal, you may be certain that there is not fever present. On the other hand, if on the first day the temperature be 105° you may be certain that the disease is not typhoid. We see, therefore, that whether the temperature be too low or too high the thermometer will greatly assist us in making a diagnosis. The temperature goes on rising about 1·5° per diem till about the fifth or sixth day, when it reaches its maximum of about 103·5° or 104·0°, or even more. This is not absolutely true, because some persons may go through typhoid fever and know nothing at all about it. In this disease, therefore, we see that there is first a long and uncertain prodromal stage, and then the onset of fever, which is insidious. Although the degree of temperature is reliable in determining the presence or absence of fever, the same cannot be said of the condition of the pulse, for, in nervous patients, the rapidity of the pulse may be greatly increased, and yet no fever be present. It therefore becomes an important thing to use the thermometer in practice; it will save many errors if used at the proper hour of the day—that is, in the afternoon.

To return to the course of typhoid fever, we find (still adhering to the chart) that on the twenty-first day the curved line suddenly runs into the straight line of ordinary health, and the fever is completed. In some cases, however, this does not take place till the twenty-eighth day. During this course we cannot give an answer to the queries of the patient's friends, "Is he better?" We must refer them to the chart, and tell them what is the course through which the disease must go. "We shall find no word 'better' written anywhere on the curve." Without this is borne in mind much annoyance may be given to the friends, for in the morning the patient may be cooler and appear better, but in the evening he is hotter and appears worse, and if the medical man were to alter his opinion according to the degree of temperature the friends might say that the attendant knew nothing whatever about the disease. It is necessary to wait the natural term of the disease before we can say whether the patient is better; moreover, we must bear in mind that two charts of the course of the disease may be drawn out, one in which the disease is represented as lasting twenty-one days, and the other in which it lasts twenty-eight days. But it is not always easy to say which is the twenty-first or twenty-eighth day, as it is so difficult often to fix the date at which the disease began; but by carefully recording the temperature we may tell the time to a couple of days. On the other hand, it is not always that we can say positively the patient is better on these days, for the disease may, as it were, begin again, and exactly repeat itself, and this repetition may last twenty-eight days more, or it may even triple itself, as Sir William taught many years ago, when lecturing on this subject to the students at Guy's Hospital. This triple attack may last as long as the previous ones. These repetitions have been found on post-mortem examination to correspond with fresh outbreaks of the intestinal affection; for in these cases, should death occur, some of the ulcers will be found almost healed while others are just beginning to be affected. It is always necessary, therefore, to give a prognosis which shall allow of the possibility of these secondary attacks. But these repeated attacks do not necessarily last as long as the previous ones; they may be much shorter, lasting only one, two, or three weeks. So that typhoid fever may have a simple course of twenty-one days, or a prolonged course of twenty-eight days, or a double or triple course, the duration of which equals that of the previous attack; or lastly, a complex course—one or two attacks of ordinary duration, with another of shorter duration.

Now, in whatever manner the poison of typhoid is introduced into the system there is a concentration in the ileum. The presence or absence of diarrhœa depends on the implication or not of the large intestine. Although the ileum is the focus of the operation of the poison, yet it does not end here, for it is reflected to every organ of the body. The incidence of the poison is on the ileum, but the reflection is over the whole body. The body reacts to these conditions, and the fever is a reaction to the local condition of the bowels; but this does not imply that typhoid fever is a local affection of the bowels as was formerly taught. This poison reacts in other ways, as by setting up ulceration in various parts of the body. He was once called to the case of a young girl who was sickening of an illness. She had a discharge from the vagina, which led the medical attendant to doubt the girl's chastity; but he (Sir William) said that it was typhoid, and that the discharge was due to ulceration of the vagina. Another case, in Guy's Hospital, was treated as a gonorrhœa; but in reality the patient had ulceration of the prepuce from typhoid fever. In a third case, the patient had perforating ulceration of the larynx, and he suddenly became emphysematous over the body, which "floored" him (Sir William); but this case was shown after death to be one of ulceration of the larynx occurring in the course of typhoid fever. This disease then is not a local one, although the focus of the action is in the ileum. But to return to the chart. About the ninth day of the disease mischief begins to show itself in the intestines, and at this time the deposit may open up a large bloodvessel, and thus give rise to severe hæmorrhage, or, if it go still deeper, perforation of the bowel may take place; or the bronchial membranes may become affected, or the bronchial glands enlarged, which may give rise by reflex action to spasm of the larynx. Every organ in the body is affected if the disease be severe. Every part of the body may be affected through the nervous system. There is a tendency to reaction. There may be pleurisy; or the enlarged mesenteric glands may soften and suppurate, which may be followed by purulent infection; or the kidneys may become diseased. In fact, any organ may become affected through the action of the nervous, venous, or other systems.

Now as to the treatment. This disease cannot be cut short, even in its early stage. Some have thought that it could be done by emetics, but few think so now. Others have thought that a purge of calomel and colocynth would cut short the attack, but this has been almost entirely given up. Others say that large doses of quinine will shorten the attack, or, at least, prevent serious complications; but Sir William related a case to which he was called, where large doses of quinine had been given, but the man was almost dead from hæmorrhage, so that, in this case, the disease was neither cut short nor complications prevented. At the present time it must be granted that when once the patient has begun to go down the curve we must not dose him with medicines—we must not give him physic. The best thing is to place him in bed in a horizontal position, and reserve his nervous power, remembering that he must, if he live, go through the whole course of the disease. He has to go through the reactions of his nervous, absorbent, and nutritive systems; he has, in fact, to go through "a great physiological storm." Let the patient alone then in this stage; "an early rest will save his power." But how is he to be treated through the remainder of the course? Is there anything to make the course less severe? Probably not. Quinine has been tried for this also, but has failed here as signally as when given to cut short the disease. It must be said that all the treatment consists of simple measures and simple diet. Medicines are only required for special conditions: for instance, if diarrhœa occur, some would give what is called "chalk mixture"; but we must remember that this has to go through about twenty-five feet of intestine. The best thing is to fill the rectum with five or six ounces of starch; it is not even necessary to put any opium with the starch. The diarrhœa only takes place when the rectum, sigmoid flexure, or descending colon is affected or irritated by acrid matters; hence, if you fill these the contact of noxious matter is prevented, and the diarrhœa checked. Time would not allow of more being said on this. [Here Sir William said he was not lecturing on typhoid fever, but only giving a few thoughts.] If hæmorrhage occur, many would give gallic acid, or tannic acid, or lead, or some powerful astringent. But do not so; "don't begin with all your great guns at once." It is best to trust to the hæmorrhage to cure itself, which it will do, and keep the patient at rest in the horizontal posture, giving a little opium (half a grain) by rectum or mouth if necessary to quiet the intestines. Ice may also be applied to the abdomen. This is sound advice, although it may appear terrible. The objection to giving lead or gallic acid or other powerful astringants is that they are apt to make the patient sick, and in that manner perhaps to make the hæmorrhage worse. Often delirium comes on in the course of the disease. The patient may get out of bed, or even commit suicide by jumping out of the window. What is to be done for the delirium? In the first place, never leave the patient alone day or night after he has begun to descend the curve, for delirium may come on at any time; therefore, he must be constantly watched. In the treatment of this complication remember that it is due to brain-irritation, and not to inflammation, as some have imagined. There is one great remedy for this, and it is alcohol; alcohol is the remedy for nervous irritation in typhoid fever, and, in fact, in any fever. How it acts is not certainly known, but it may be laid down that it is an important point in the treatment of all fevers. It allays nervous irritation and soothes the nervous system. Whenever, therefore, there is delirium or reflex irritation, give alcohol in some form or another, as brandy or wine; but when the result has been obtained, stop it; only give it when it is necessary. In fevers the use of stimulants is for the delirium, and it in no way alters the local processes, but only the reflex condition. All cases do not require alcohol, and some only require it occasionally, and for a short time. Delirium even in the early stages may be checked by brandy, and never again come on during the whole course of the disease.

There is another point. Often the patient cannot or ought not to be moved out of bed. How is he to pass his stools? There is a danger of bedsores if care be not exercised to prevent them; but no man who has a due care ought, under ordinary circumstances, to have a patient suffering from bedsores; sometimes, however, the circumstances are such that these sores cannot be prevented. The bed-pan may become the source of a bedsore which may carry off the patient. Avoid, therefore, every possible cause of irritation or abrasion of the skin. This is a point which is worthy of the consideration of every clinical physician. Again, it may be necessary to pass a catheter to draw off the patient's urine. Now this may be done in such a manner as to injure the urethra, which may be in a very tender state, and cause ulceration and severe symptoms, and even death. Use, then, a soft catheter, with all possible care and delicacy. In summing up he observed that the recovery from typhoid is dependent on attention to the smallest matters.

The time will come when this disease will become rare, or even die out.

We must save the patient's strength from the beginning, and not trust to medicines to cure the disease. The materia medica of fever is lessening every year.

Sir William would prefer to carry anyone through typhoid fever by wines and soups and fresh air, rather than by the use of drugs.

After the lecture the Treasurer thanked Sir William Gull for the clear, simple, and logical manner in which he had spoken of typhoid fever—so clear that the subject was intelligible even to a non-professional person like himself.

Sir William, in the course of his short reply, remarked that he owed all his position to being a Guy's man, and that therefore he would be glad to assist and help the school all he could, and further that he would be pleased to meet the students again next year.

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THE ROYAL SOCIETY.

At the meeting on the 30th May, (Mr. G. B. Airy, C.B., president, in the chair), Mr. T. B. Curling, F.R.S., introduced a paper "On Ammonia in the Urine in Health and Disease," by C. M. Tidy, M.B., and W.B. Woodman, M.D.

The authors differ from Neubauer regarding the average normal excretion of ammonia. They find it to be about two grains, while Neubauer gives 10·8 grains per diem. The total number of cases upon which observations were made exceeded 200. The method adopted by the authors is as follows:—

The freshly passed urine is to be first diluted with a given bulk of distilled water sufficient to destroy all apparent color, a known quantity of the urine being taken in each case. To this is to be added an excess of Nessler's solution, and then compared with the tint depth produced by known quantities of ammonia in a similar bulk of liquid, treated with the same solution. The trace of ammonia in the water itself must be always allowed for. The external temperature in the observations was always about 62°. They regard 60 oz. of urine as the normal amount daily excreted by adults.

The authors found that in health the excretion of ammonia is largest under thirty-five years of age; increased after meals and during exercise. They consider that this excretion by the kidneys is governed by a similar law to that which regulates the formation of dew.

In disease the lowest quantities were met with when the pulse was rigid, the highest average being met with when the pulse was nearly normal. The excretion was increased when the skin and tongue were moist, when the diet was full and included stimulants, and when the bowels were open. Acid medicines also increased the secretion.

A number of special diseases are alluded to. The cases taken just before death are very remarkable, showing a vast decrease in the amount of ammonia. In two cases it was entirely absent, the only cases of entire absence known to the authors.

Foreign Gleanings.

TWELVE CASES OF TRACHEOTOMY, WITH REMARKS.

M. E. Dudon, of the Hôpital St. André, Bordeaux, has just published the account of twelve cases of tracheotomy which he performed, and in six of which cure was effected. From the careful study of his twelve cases M. Dudon draws a moral which has so often been pointed out, especially by French surgeons, but which cannot be too often repeated—namely, the necessity of not waiting too long before operating. M. Dudon is convinced that had he been able to operate sooner, some of the unsuccessful cases would have ended favorably. The less the false membranes have extended to the trachea and bronchi the better respiration is restored; the less asphyxia is marked and the blood altered, the more the strength of the child will be preserved to resist the disease. "Considering," says the author, "tracheotomy to be in itself a benign operation, when viewed independently of the morbid causes which may necessitate it; convinced that if regularly performed upon healthy children it would scarcely ever occasion death; persuaded, as I am, that it is advantageous for children affected with croup to be operated on at an early stage of the disease before the malady and asphyxia have made any great progress; and backed by the results of my twelve cases, I believe I am justified in saying that tracheotomy is an operation which, to produce all the good results which are expected from it, must be practised early—I mean as soon as the larynx is invaded by the false membranes and that medical means have failed. It may well be compared with the operation for strangulated hernia, which, when performed at the beginning of strangulation, after taxis under the influence of chloroform has proved unsuccessful, affords much more favorable results than when ulceration or gangrene of the intestine and the development of peritonitis have been allowed to take place."

TREATMENT OF SYPHILIS BY MEANS OF HYPODERMIC INJECTIONS OF CORROSIVE SUBLIMATE IN THE FORM OF A CHLORO-ALBUMINOUS SOLUTION.

Dr. Staub, of Paris, has published, under the above head, the results of a most interesting series of experiments. The object of Dr. Staub was to avoid the local accidents caused by hypodermic injections of sublimate, and also to be able to employ stronger doses. This he effected by means of an albuminous solution of sublimate in alkaline chlorides, according to the principles laid down by Gubler, Mialhe, Barensprung, &c. The formula for injection deserves to be carefully noted. Sublimate and chloride of ammonia, of each 20 grains; chloride of sodium, about 62 grains; distilled water, 20 grains. After filtration the whole is mixed up with an albuminous solution (white of an egg, water 4½ drachms). This mixture produces no local inconvenience; it contains 5 milligrammes (about 1–33 grain) to every 20 drops. The results of treatment, states Dr. Staub, have been very favorable. In the 44 cases related the duration of treatment varied from seventeen to thirty-four days, with one centigramme (⅙ grain) of sublimate injected each day. The author comes to the conclusion that the hypodermic method, with the chloro-albuminous solution of sublimate, can and ought to be transformed into a general mode of treatment of syphilis; that it will take the first place in the therapeutics of syphilis; and that it may be applied to all cases (syphilitic or not) where a mercurial treatment is indicated.

ŒDEMATOUS ELONGATION AND PROLAPSUS OF THE CERVIX UTERI.

In an able paper in the *Archives de Médecine* (July, 1872), Dr. Guénot has carefully investigated the above subject. The chief points of the paper may be briefly summed up thus: The author considers the disease as comparatively frequent, it being often confounded with hypertrophic elongation or pure prolapsus. Its symptoms are quite distinct, however; hyperæmia of the organ, the peculiar disposition of the cavity (transformed into a long and open canal), the rapidity with which the various symptoms are liable to disappear and their facility to return under certain circumstances, ulceration of the os, inversion of the vagina, attenuation and flaccidity of the uterine walls, pains around the pelvis, general weakness, and disorders of nutrition, being the characteristic traits of the disease. The causes are complex, and are anatomical or mechanical. The disease is more painful and inconvenient than dangerous to the mother, but it is highly serious to the child, by producing premature labour or abortion. The treatment suggested is to put back the prolapsed organ into the vagina, and maintain the reduction by means of a bandage to the vulva; horizontal posture; careful abstinence from the use of pessaries; and, after delivery, prolonged observance of the recumbent posture.

IS STRYCHNIA THE ANTIDOTE TO CHLORAL?

M. Oré, professor of Physiology at the Bordeaux School of Medicine, answers in the negative, or at least he denies that Liebreich had the right to state that strychnia was the antidote to chloral, as an inference from the experiments which he (Liebreich) had performed. M. Oré has been going over Liebreich's experiments, and has not attained the same results. It will be remembered that Liebreich's experiments went to show:—1. An hypodermic injection of two grammes (half a drachm) is deadly to rabbits. 2. An injection of one milligramme and a half of strychnia is equally deadly. 3. If an hypodermic injection of one milligramme and a half of strychnia be made into a rabbit, even when the effects produced by a subcutaneous injection of two grammes of chloral have begun to show themselves, these effects are rapidly arrested and the animal is restored to life. On the other hand, it dies if no strychnia is administered.

M. Oré's counter-experiments, eight in number, have been conducted on rabbits of various sizes. The results are: If two grammes of chloral occasion death in certain cases they do not in all; and the same may be said of one and a half milligrammes of strychnia. M. Oré concludes that strychnia may be the antidote to chloral, but that the fact has not been demonstrated by Liebreich in his experiments.

RAPID CONTROL OF EPISTAXIS.

Dr. Marin, of Geneva, considering that in this accident the blood comes usually from one nostril only, and mostly from the anterior third of the latter, has tried to compress the facial artery on the superior maxillary bone, close to the ala nasi. The afflux of blood into the cavities of the nose is thus stemmed, and the epistaxis soon stops. M. Marin says that he has thus relieved many persons in the street, on board steamboats, in railroad carriages, and even at the theatre.

News Items, Medical Facts, &c.

The Treatment of Malarious Fever by Hypodermic Injection of Quinine.—A late number of the *Indian Medical Gazette* contains an abstract of the results of this method of treatment in 100 cases of fever of the intermittent and remittent type. 68 per cent. of the cases are stated to have been cured. Ulcerations at the point of injection occurred in 16 cases. The subject of the hypodermic injection of quinine was referred last year to Prof. Maclean, and his report thereon has been published officially in India. Dr. Maclean states that among the invalids in the medical division at Netley at the time of his report, he found 4 on whom this operation had been performed, and in 3 of the number troublesome ulceration had resulted, owing in all probability to the fact that the solution of the quinine had been effected by the aid of a mineral acid. It was necessary, therefore, that some solution of quinine devoid of irritating properties should be employed for subcutaneous injection. With this end in view, the so-called neutral sulphate of the alkaloid was used, which is, in fact, not neutral, for it gives a decided reaction with litmus paper. However, it appears to have answered the purpose very well. With a little care it is quite possible to inject six grains of the salt dissolved in twelve minims of water, without the aid of any acid. The proceeding is very simple. Three or six grains of the neutral sulphate are placed in a watch-glass previously warmed; to this twelve minims of distilled water are added, and a moderate degree of heat applied with a spirit-lamp for a second or two. The syringe, previously warmed by placing it in water at a temperature of about 130° Fahr., is then filled, and the operation completed in the usual way. Warming the syringe is essential. If this is neglected, immediate deposition of crystals follows, and the injection becomes impossible. The ordinary hypodermic syringe used for morphia injections is not thoroughly adapted for this purpose, and it is well to have a separate syringe of larger size and with a freer aperture for injecting quinine. A weak solution of carbolic acid should be used to wash the perforated needle each time after its being employed.

"Viewing the Body."—At an inquest held by Dr. Lankester to inquire into the death of a lady who had died of apoplexy in consequence of the excessive heat, the jury, upon proceeding to view the body, found that it had been placed in a coffin and screwed down; but in the lid a piece of glass was inserted, which enabled them to view the face of the deceased lady, whose body, owing to the hot weather, was rapidly decomposing. The coroner remarked that he had suggested this for the better protection of the jury; and if this plan were adopted in all such cases, or in the case of a person dying from some contagious disease, it would be a great safeguard to the public, especially those who in the performance of a public duty were compelled to view the body. The question arises, why are jury compelled to view the body? Is it merely a matter of form, or is it that they may satisfy themselves as to the identity of the deceased, or that the external appearances of the corpse correspond with the evidence which is given as to the cause of death? If for either of the two latter purposes, it is certainly not sufficient to merely look through a pane of glass in the lid of the coffin. If "viewing the body" is merely a matter of form, the coroner is quite right to save the jury every possible annoyance; but if this ceremony is intended to serve any useful purpose, we hold that he has established a very dangerous precedent.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery, Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 11. NEW YORK, NOVEMBER, 1872.

Lectures

ON

THE SURGICAL TREATMENT OF ANEURISM

IN ITS VARIOUS FORMS.

Delivered at the Royal College of Surgeons, June, 1872.

By Timothy Holmes, M.A., F.R.C.S. Eng.,

Professor of Surgery and Pathology to the College.

LECTURE III.—Part I.

Mr. President and Gentlemen.—The conclusions on the subject of Brasdor's operation in innominate aneurism, which are justified by present experience, appear to me to be as follows:—

1. That the distal ligature of the carotid alone, or in conjunction with that of the third part of the subclavian, cannot be trusted to produce the complete consolidation of the tumour.
2. That, however, the natural effect of the ligature of the carotid artery is to produce coagulation in the part of the sac directly connected with the mouth of that artery.
3. That this may suffice practically for the cure of the aneurism, when the subclavian portion of the sac is small and shows no disposition to grow.
4. That in other cases, where the mouth of the subclavian artery is previously obliterated by impacted clot, the ligature of the carotid only may effect a radical cure.
5. That for these reasons it is better, in any case which appears to require distal ligature, to commence with the operation on the carotid alone, and afterwards to consider the propriety of securing the subclavian either in its first or third part.

With respect to the justifiability of the operation on the first part of the subclavian artery, I beg to submit the following considerations bearing on the question whether it is possible to avoid in future the occurrence of secondary hæmorrhage after the ligature of a large artery. The subject is a new one comparatively; and I must be understood as speaking with all possible reserve on a point where our present experience does not permit us to dogmatise.

In determining the justifiability of repeating an operation which has hitherto never succeeded, the reason of that want of success is, of course, the first subject for our consideration. The ligature of the first part of the subclavian has hitherto failed on account of the almost uniform occurrence of secondary hæmorrhage. There are, it is true, anatomical difficulties and dangers in the operation. In tying this artery for innominate aneurism there would be much risk, in some cases, of wounding or injuring the sac; in others, of finding the vessel diseased and somewhat aneurismal. Still I think most surgeons would allow that, if we could be sure that the ligature of a large artery involved little or no risk of secondary hæmorrhage, the operation might be repeated under similar indications to those which would justify any other operation of equal gravity.

Now, an audience such as this need not to be reminded that the ligature of an artery, without its subsequent division, has been an object which has been pursued by surgeons ever since the very first case in which John Hunter tied the femoral for popliteal aneurism.

In one of the earliest cases operated on by Mr. Cline, the attempt was made, by means of a temporary ligature, to produce pressure enough to close the vessel, but without dividing it by ulceration. The same attempt has been frequently repeated, but it does not seem that the usual way of making the attempt is a safe one. This consists in laying on the artery a substance (such as a plug of lint), over which the ligature is tied, and then cutting the knot of the ligature at such a time after the operation as in the judgment of the surgeon is sufficient to ensure coagulation in the tube of the vessel, without producing enough irritation in its coats to destroy their vitality. As this plan has been sufficiently tested, and is allowed to be more dangerous than the common single tight silk ligature, nothing further need be said about it. But the same object has been sought by temporary deligation or acupressure of the artery—i. e., by compressing it with some metallic substance, which is removed when it is judged safe to do so. Allow me to exhibit this specimen, lent to us by the Royal College of Surgeons in Ireland, and showing the obliteration of the femoral by means of the presse-artère, and the cure of the popliteal aneurism, for which the operation was performed. The presse-artères which have been introduced hitherto do not seem trustworthy; still this preparation shows that an artery may be obliterated, and an aneurism cured, without division of the vessel.

I would refer also to a very interesting case in which my friend Mr. W. Stokes[*] used acupressure or temporary deligation to the abdominal aorta for the treatment of ilio-femoral aneurism, by a wire compressor invented by Mr. Porter. The patient only survived the operation twelve hours, his death being referred to the shock of the operation on a weak fatty heart. The aorta, though held tightly enough to prevent the passage of any water injected into its upper end, showed no trace of injury to any of its coats. In the case, however, in which Mr. Porter performed a similar operation on the innominate for the treatment of subclavian aneurism, by means of an instrument something like a small lithotrite, after three days of pressure the coats of the artery sloughed and fatal hæmorrhage resulted, and when the pressure was removed the pulsation of the tumour recurred. This very valuable and interesting preparation I am also enabled to produce through the courtesy of the College of Surgeons in Ireland. Mr. Porter was of opinion that in this case failure may have been due in part to the comparative clumsiness of the instruments, and has figured in his "Surgical Reports" the more manageable and delicate form of instrument which Mr. Stokes used. The time during which Mr. Stokes's patient survived hardly allows a judgment as to the action of the compressor.

Sir P. Cramptom, in the seventh volume of the Medico-Chirurgical Transactions, argues that the division of the two internal coats of the artery is unnecessary; and he recommends that the walls of the vessel should be brought into contact by means of a loop, whose tension is to be regulated by a presse artère with just sufficient force to stop the pulsation of the aneurism—the ligature to be removed as soon as, by experiment, it is found safe to do so. Two examples of the successful use of this plan are given, and it has been imitated by various surgeons, while others have invented processes of their own essentially similar. But it has been found that the proceeding is so uncertain, the injury done to the artery so difficult to estimate, and the withdrawal of the ligature therefore so likely to provoke secondary hæmorrhage, that it is wellnigh abandoned. Mr. Bickersteth, however, a short time since, used a plan of the kind on the innominate artery.

Since the use of silver sutures has become general, and the absence of ulceration has been noticed which sometimes attends on their remaining even for a great length of time buried in the body, it has been attempted to ensure the same absence of ulceration by tying arteries with ligatures of this substance. But many cases have been put on record showing the uncertainty of this proceeding. The only thoroughly satisfactory case which I am acquainted with, in which the silver ligature has been used to a large artery, was one that occurred in my own practice, and in which I tied the femoral artery for popliteal aneurism. The wound healed, though not without suppuration, in less than a fortnight. No trace was ever seen of the ligature, and the man recovered rapidly and completely. He is, I believe, still alive, and was in good health I know a short time ago. In this case a most careful watch was kept for the appearance of the ligature if it should come away; but nothing was seen of it, and I believe that it buried itself in the tissues of the vessel, and in all probability without dividing the continuity of the latter. In an operation on the carotid of an ass, I found the artery undivided and obliterated six weeks after the application of a silver ligature. The ligature still surrounded the artery loosely. But this substance is, I think, a somewhat dangerous one, for if it is tied at all tightly it will cut the coats of the vessel more deeply, and, I believe, produce more and more rapid ulceration than the silk thread does. Such was the case in this preparation, taken from a patient in whom Mr. Brodhurst tied the femoral with silver wire. In this other instance of ligature of the external iliac by Mr. Pollock the patient died from another cause before the effect of the ligature could be ascertained.

From what I have seen and read, then, of the effect of the silver ligature to an artery, I conclude that its use will in some cases probably preclude the occurrence of secondary hæmorrhage, but that it is too uncertain in this respect to be preferable to silk.

A far more reliable material is catgut, and I hope to show you anatomical proof that this substance, when properly prepared, may be used for the ligature of a large artery with perfect success, and without dividing the external coat of the vessel, or interrupting its continuity, while the ligature itself is absorbed; and also to adduce practical proof that the wounds made over vessels so tied often unite by first intention. But I would wish to impress on you the conviction that I entertain, that in such cases it is not merely the material used for the ligature which is of importance, but that the rapid union of the deep parts of the wound is also a necessary condition.

The catgut ligature for the purpose of securing a large artery, was first proposed, I believe, by Sir A. Cooper, and he carried out his own proposal in a case in every way so remarkable that it is astonishing to find it attract so little notice or imitation. In 1817 Sir A. Cooper thus wrote:—"My friend Mr. Lawrence has proposed that the silk usually employed for ligatures should be cut off close to the knot, so as to heal the wound over it. It has occurred to me that catgut would answer the purpose better, and I shall give the result of the trial I have made, wishing it to be understood that I consider the subject at present as undecided, and only as one for future investigation. Catgut employed as a ligature, being more of the nature of the animal matter in which it is embedded, will be more easily absorbed than silk; or even if not absorbed, will be less likely to excite irritation in the parts."[*] He then proceeds to relate the case of a man, aged eighty, who came under treatment at Guy's Hospital on the 15th of October, 1817, for popliteal aneurism, and in whom he tied the femoral artery on the 24th of October, with "a single ligature applied round the artery, both ends of which were cut off close, and the edges of the wound brought together by adhesive plaster; the substance made use of for the ligature was catgut, which had been previously soaked in water about the temperature of 100°." The wound was not again inspected till the fourth day (October 28th), when it was found "*completely* united"; and on the 7th of November it is noted, "The wound remains perfectly united and free from irritation." Again, on the 24th of November, "No appearance of irritation from the ligature." "In three weeks after the operation he walked in the ward with the aid of a crutch." The notes are continued down to the 17th of December, when it is said, "His health is perfectly good, he walks without the aid of crutch or stick, the swelling is reduced to a small size, and the part at which the incision was made has been and now is quite free from irritation." No wonder that a result so unusual, or rather so unprecedented, to the operation for popliteal aneurism, drew from Sir A. Cooper the following remarks:—"I confess that this case gave me much pleasure. The great age of the patient, the simplicity of the operation, the absence of constitutional irritation, and consequently of danger, and his rapid recovery, lead me to hope that the operation for aneurism may become at some future period infinitely more simple than it has been rendered to the present moment."

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[*] Dublin Quarterly Journal of Medical Science, Aug. 1869.

[*] Surgical Essays, by Cooper and Travers, i., 125.

Sir Astley's subsequent experience was, however, disappointing. In the 12mo edition of his Lectures, 1829, p. 168, is as follows:—"I thought that a ligature made of catgut would admit of solution, and I tried it on a man eighty years of age, on whom I performed the operation for popliteal aneurism; the catgut ligature was cut down to the vessel, the wound healed over, and no bad symptom followed. The experiment succeeded it is true in this case, but I have used the catgut ligature in three cases since, and did not find it at all superior to the common ligature; in each of the three it came away by suppuration and ulceration as in common cases; it did not therefore succeed. In the old person there was less tendency to inflammation, and that was the reason of its succeeding. I made some experiments on dogs to ascertain the solubility of catgut. I tied the carotid of a dog with catgut ligature; in a fortnight after I killed the animal, and found that the ligature had not been dissolved, but that it had cut through the artery and was situated in a cyst, like that which is formed round a ball, between the divided ends of the vessel, in a quiescent state. Therefore this substance does not admit of solubility, but will remain sometimes without producing irritation. On the whole, catgut ligatures are not at all superior to the common ones. Some animal matter of the form of gluten made into ligature might do, but this is mere conjecture. At present there is no ligature known which is capable of being dissolved and removed by the absorbents."

In Cooper's Surgical Dictionary (art. Ligature) may be found an account of what has been done down to recent times in experiments on animal ligatures. Those introduced into practice by Dr. Physick, in 1814, were made of chamois leather, and the same substance was made use of by Dr. Jamieson, of Baltimore, who "tied the carotid, the iliac, the femoral, the radial, the posterior tibial, the spermatic, and other arteries with 'buckskin ligature,' and in no instance had secondary hæmorrhage occurred; and he states that he has never seen anything of his ligatures, and of course the wounds have generally healed by first intention." The quotation is from the American edition of Cooper's Dictionary.

Porta's experience in tying arteries in the human subject with catgut is the most considerable hitherto published. His work gives short notes of nine cases in which the operation was performed by himself, and two in which Guerini was the operator, assisted by him. The arteries were the carotid, femoral, and brachial, each three times, the external iliac and the posterior tibial. In none of these cases was there any secondary hæmorrhage; and the results were certainly as satisfactory as those of the ordinary ligature. But I do not see that Porta's practice afforded any conclusive proof of the superiority of catgut as the material for ligature. In the first case (carotid) the patient did well, but the wound took two months to unite. In the second (brachial), the patient died, sixty-five days after the operation, of pneumonia (pyæmic?). The wound had suppurated up to the time of his death, but this suppuration was proved to be only superficial on post-mortem examination. In the third (femoral) the patient died of gangrene in seventeen days; the wound suppurating up to the date of death. In the fourth (carotid) the wound healed soundly and permanently in four days. In the fifth (femoral) the patient died in fifty-five hours. In the sixth (femoral) the patient died on the tenth day of encephalitis. The external wound had healed, but there was a small abscess below the fascia lata, not, however, in contact with the artery. In the seventh (carotid) the patient died in forty hours. In the eighth (external iliac) the patient died on the third day of encephalitis. In the ninth (brachial) the wound was not closed for a month. In the tenth (brachial) the wound did not close for five weeks. In the eleventh (posterior tibial) there was a large wound caused by laying open a traumatic aneurism in the foot. It closed in about six weeks. In none of these cases was the ligature observed to come away, but in all those examined after death, when a sufficient time had elapsed, the vessel was found to have been divided.

It will thus be seen that none of the cases pursued a course remarkably different from the usual one, except the fourth case, in which the wound of ligature of the carotid healed permanently in four days. This is a gratifying result of operating with close-cut ligatures; but it does not, by itself, prove anything as to the catgut ligature, since a similar result has been obtained with silk, as the following quotation from Porta* shows—"In Wright's patient, who died five months after the operation on the external iliac, the silk ligature, which had been buried in the wound, was found, on dissection, lying close to the artery, enclosed in a small cyst, something like an inguinal gland."†

Knight relates that Lizars tied the subclavian for axillary aneurism, April 26th, 1834, on a patient named Hain, with a flax thread, the ends of which were cut short, and the wound was united; the latter healed entirely on the third day, and the patient recovered completely, nothing more being seen of the ligature.‡

Porta, however, though he strongly recommends catgut as a material for ligature, nowhere, as far as I see, contemplates the occlusion of the vessel without its division; and speaking of secondary hæmorrhage, he says (p. 133): "Hæmorrhage being inherent in the nature of the operation and of the organ operated on, and the condition being far more propitious to it in the human subject, we must not flatter ourselves that we shall ever be able to annihilate its possibility by any method, or to reduce it within the limits which we observe in the lower animals."

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* Page 32.

† Repository and Review. New Series, vol. vi., p. 231.

‡ Froriep, Notizen, vol. xliii., p. 12.

Mr. Norman, of Bath, also tied the femoral artery with catgut, cut short, March 7th, 1818. The case was successful; but the wound, which "appeared at first to have united in two days, separated. A long and troublesome suppuration ensued, and the wound was not healed till the latter end of April. The ligature was never seen to come away."*

Sir P. Cramptom also used a ligature of moistened catgut in tying the common iliac artery in the year 1828.† The ligature was not cut close; it came away on the eighth day, and the patient at the same time complained of very severe pain. He died of a sudden gush of hæmorrhage, from the upper end of the artery, in less than a minute, on the tenth day—i. e., two days after the fall of the ligature. Pulsation had been perceptible in the tumour from the fiftieth hour after the operation, and had so far increased as to justify the suspicion that the ligature had softened and given way. The note on the day on which the ligature came away runs as follows:—"Has the blood returned by the free anastomosis between the internal iliacs?—or has the catgut ligature become macerated and given way? The pulsation is stronger than could be expected if the tumour were supplied only by the collateral source of anastomosis. The prevalent opinion, therefore, was that the ligature had given way."‡ This opinion was strengthened by the appearances found on post-mortem examination. The artery was not cut through; but its internal and middle coats were completely divided, and there was an opening in it (not described in Sir P. Crampton's account) from which air or water injected into the proximal end of the vessel issued. This opening communicated with an abscess surrounding the artery, and thus a way was afforded for the blood from the artery to the wound.

Now on this case I would remark—1. Had the ligature been properly prepared, it would have held the walls of the vessel together, and prevented circulation through it until its divided internal coats had been sufficiently united to resist the impact of the blood. As it was, the artery, weakened by the violence done to it and the division of a great part of its wall, was doubtless torn by the distension which a great stream of blood caused in passing through it. 2. Or, if the parts around the vessel had become firmly consolidated and united in the first few hours after the operation, they might have answered the same purpose. This seems to have happened with the carotid artery in my case of double distal ligature. Two small depressions are seen close to the track of the ligature, on the inner face of the vessel. One does not lead outside the tube of the artery; but the other leads into a small sac (formed out of the cellular tissue or by the expanded external coat of the artery), and containing a little old blood-clot. If the formation of this little sac had been interfered with by suppuration around the artery, very probably the minute opening might have enlarged and hæmorrhage supervened. If Sir P. Crampton had prepared the catgut properly, had tied the ligature firmly, cut it off correctly (both ends close), and had so treated the wound and the patient as to produce speedy consolidation of the deep parts, there would probably have been no hæmorrhage.

In a case recently under my own care the patient died on the twenty-seventh day after amputation of the forearm, of chronic pyæmia, which was, as I suspect, developed before the operation. The stump was healed all but a small opening which led into an abscess, holding about two drachms of foul pus. No trace was found of any of the catgut ligatures which had been applied. These were at least four in number—on the radial, ulnar, and interosseous. Some other arteries might have been tied, but no particular note has been preserved of this. One of the larger arteries, having been divided obliquely, required two ligatures. There was no trace of any irritation around the part where these ligatures had been applied, nor any pocketing of the matter, or attempt at formation of a cyst. The existence of an abscess in the stump was due evidently to failure in vital power. During the first fortnight the stump had healed with great rapidity; but afterwards, as the patient's powers failed, the closing of the wound was suspended, and the matter became foul. As I always dressed the stump myself, I think I can be certain that the catgut never came away. Besides, in a great part of the stump the edges had healed by first intention, and several of the sutures, which were also of catgut, had never been removed. They had been left to themselves, the part below the skin melting away, and the exposed part dropping off, when this process was complete. [I produce the arteries from this case.]

This is the only opportunity I have yet had of examining the site of an operation of any magnitude at a time which would test the disappearance of the catgut ligatures, except in one other amputation (of the thigh), in which the patient died about the sixth day. The catgut ligature on the femoral artery was found much softened and absorbed, so as to be quite relaxed, and to be drawn off the artery with perfect ease. No secondary hæmorrhage had taken place, but there was not much coagulum in the vessel, and I am not certain that secondary hæmorrhage would not have occurred had the patient survived. This case, however, happened very early in my use of the catgut ligature, and I doubt whether the gut was properly prepared. It quite coincided with the former case in showing the reality of the absorption of the catgut when buried in the tissues.

Another obvious proof of this same absorption is the fact noticed by Professor Lister, and which I have over and over again verified—viz., that when this same material is used as a suture for a wound, if it be withdrawn in a few days, the part which has been buried below the wound is reduced in volume to a fine thread, its tenuity depending of course on the length of time it has been buried; until, as I mentioned in my case of forearm amputation, the deep part of the suture melts away, and its superficial part drops off. Thus an incidental advantage in these sutures is that they do not require to be withdrawn. Further, the absorption of the catgut-ligature is proved by the occurrence of primary union over it, of which I have had at least one very striking instance in my own practice: In a case which occurred some years ago I removed the breast and some axillary glands affected with scirrhus by an incision more than six inches in length. The whole wound united by first intention over the catgut ligatures, several of which had been employed. I kept the woman in hospital for a time to see whether any irritation was set up by the buried ligatures. But nothing of the kind took place; and I saw her again long after, and ascertained that no irritation, far less any abscess, had ever existed in the neighborhood of the wound. I mention this case especially on account of the opportunity I had of seeing the patient long after the healing of the wound.

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* Med.-Chir. Trans., x, 120.

† Ibid., xvi., 163.

‡ Porta (op. cit., p. 34), in commenting on this case and that of Bertolazzi, says that there was probably in both some error in tying the knot or in cutting off the end of the ligature; and any-one who reads Sir P. Crampton's account of the way in which the knot was tied, will see that it is very probable that the artery might never have been quite firmly constricted.

But since the employment of this material primary union after ligature of a large vessel has become extremely common. Thus Mr. Bickersteth* says: "During the last three years I have tied the femoral artery five times, the common carotid once, and the common iliac once; and in every case, with one solitary exception, the wound has healed at once, and without suppuration."

In the case in which Mr. Heath lately tied the left carotid on the distal side of an aortic aneurism with carbolised catgut cut short, the wound was not looked at till the eighth day, when it was found to be entirely healed by the first intention. The man is at present in our hall, and you can see after the lecture how simple and perfect the union of the wound has been.

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ABSTRACT OF

Lectures

ON

DISEASES AND INJURIES OF THE EAR.

Delivered in St. George's Hospital,

By W. B. Dalby, F.R.C.S., M.B. Cantab.,

Aural Surgeon to St. George's Hospital.

LECTURES I. & II.

In the course of these lectures any remarks upon the anatomy of the ear will be limited to those points which are of practical importance in estimating the changes produced in it by accident or disease, so far as they can be observed by examination both in the living and dead subject, and so far as they interfere with the function of hearing and endanger life.

Affections of the ear naturally divide themselves into those of the external, middle, or internal ear. The external ear, which consists of the auricle and external auditory meatus, is separated from the middle ear by the tympanic membrane. It is sometimes the seat of malformations, as instanced in an imperfectly formed auricle and meatus, the latter being sometimes so small as only to admit of a probe being passed into it, and occasionally being absent altogether. Children have been born with four ears, two on either side; and I once saw an adult, in other respects properly formed, in whom the auricles were so small that they measured barely one inch from the tip of the helix to the extremity of the lobe. Surgical interference in these cases is not often demanded, and where it has been exercised, especially in instances of rudimentary meatus, the results have not been encouraging; this being chiefly due to the fact that where the external division of the ear has been defective, the middle or internal one has been so as well.

The length of the external auditory meatus, although varying in different individuals, is about one inch and a quarter, and the calibre is greater in some than in others. It consists of two parts, the external cartilaginous comprising one-third, and the other two-thirds being an osseous canal in the temporal bone. At the bottom of this canal is the tympanic membrane, forming an angle of 45 deg. or nearly so with the floor. With infants the osseous meatus is not developed, and the membrane is fixed at the outer part of the skull, being thus nearly horizontal in position. At this time of life the portion which eventually becomes bony is membranous, and later in life the freedom of motion which is permitted between the cartilaginous and osseous part of the meatus when the auricle is raised, is due to some slight remains of the membranous part. Great care, then, should be exercised in dealing with the meatus of very young children, especially in the case of a foreign body impacted in this situation. In shape the meatus is somewhat oval, the longitudinal diameter at the external part being the longer, but at the other end the transverse. The whole meatus takes a gentle curve forwards. The upper wall of the osseous portion is nearly horizontal; but, proceeding inwards from the middle, the floor dips downwards, and on this account, and from the angle which the tympanic membrane makes with it, it follows that the floor is longer than the roof. It is partly for this reason, and partly because the calibre is smallest about the middle, that a foreign body which has passed this point is so liable to be pressed onwards in the efforts which are sometimes made to extract it; and, again, it is for this reason that, in using the speculum, it must be tilted a little in order to obtain a complete view of the membrane.

The method of examining the external auditory meatus and the tympanic membrane now almost universally employed in Germany and in this country was originally introduced a few years ago by Dr. von Tröltsch, the Professor of Aural Surgery in Wurtzburg. It seems almost strange that so simple and effective a plan had not been thought of before, in place of the imperfect methods previously in use. It consists in reflecting light from a concave perforated mirror down a funnel-shaped speculum of the kind first known as Gruber's. In consequence of the variety in the size of the meatus, several specula should be at hand (perhaps half a dozen), the straight tubular part being oval in shape. This part of the speculum is generally made too long; it should never measure more than half an inch, and the whole length of the speculum should not exceed an inch and a half. If longer than this, not only has the light to be sent down a greater distance, and therefore the reflector be held farther off, not so good a view being in this way obtained, for the eye is thus placed so much farther from the object, but it is found to be in the way when an instrument is being used, as in removing a polypus. It is unimportant whether the speculum be made of silver or vulcanite: the latter kind, perhaps from the contrast in color, renders slight changes in this respect on the tympanic membrane more strongly marked. Both kinds remain in the ear without holding. With regard to the specula which are made to dilate with a screw, and the speculum of Kramer which opens out with handles after it has been introduced, it is idle to suppose that the meatus under ordinary circumstances will admit of dilatation, and therefore for general occasions they are useless. There are, however, some cases where Kramer's speculum may be serviceable, and these are those in which the tragus is situated in a more advanced position than natural, and the anterior and posterior boundaries of the cartilaginous part are approximated more than usual, giving rise to a narrowing of this part, so that the orifice is more like a slit than the ordinary shape. These are, however, exceptional instances; and, speaking generally, the object in using a speculum is, not to dilate, but to make a straight tunnel for light down the meatus. In introducing a speculum great gentleness should always be observed, as, even with a healthy meatus, a feeling of faintness will occasionally be induced. Sometimes it will give rise to a fit of coughing which is stated to depend on reflex action due to irritation of the auricular branch of the pneumogastric nerve. This appears to be the correct explanation of a symptom which has been spoken of as ear-cough, and has been noticed in cases where a foreign body or some other source of irritation has been present in the meatus. In simple examination the reflector is held in the right hand, and when it is desirable for both hands to be free, as in any operative proceeding, it is worn on the forehead and fastened round the head with a band, in the same way as in examining the throat with a laryngoscope. Thus two kinds of reflectors should be at hand. The best light for minute examination of any object is a bright diffused daylight, and this applies as much to the external auditory meatus and tympanic membrane as to any other. The patient should either be seated or stand close up to a window, with the head inclined a little downwards, and the side to be examined turned away from the light, but somewhat sideways. If the day is too dark for a good view, as unfortunately it often is in this country, the light may be reflected from a bull's-eye lamp, lit with gas, fixed to a stand, and arranged that it can be moved up and down so as to be placed on a level with the patient's ear when he or she is seated near. This artificial light is not nearly so good as daylight, as, from the translucency of the membrane, a somewhat yellow tinge is imparted to this structure which is not natural to it. If the patient be in bed, a moderator lamp is a convenient source from which to obtain light. In the absence of this a Miller's lamp may be used.

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* Recent Progress in Surgery (an Introductory Address delivered at the Liverpool Medical Institution, Liverpool, 1871), p. 20.

Foreign bodies in the meatus are generally put there by children at play, and are chiefly stones, beads, peas, and the like. With regard to their removal, two points should be borne in mind. In the first place, that as the meatus is lined by skin continuous with that of the auricle, provided the edges of the object are not sharp, they may remain there for an indefinite period and do no harm. In the second place, that very considerable injury is often done by injudicious efforts which are made to extract them. It is not, however, to be understood from this that no attempts should be made to remove them. But no instruments should be used, excepting when, with the mirror on his forehead, light is reflected down the meatus, and the operator, with both hands free, can see what he is doing. The first thing to be done in any case is to syringe the ear; and in doing this the nozzle of the syringe, which should be fine and probe-pointed, is to be kept at the upper part of the meatus, so that the returning stream of water may bring it away. This not happening, if it is near the external orifice, or at any rate not more than half way down the meatus, and especially if it be of a soft nature or of an uneven shape, it may often be laid hold of by a pair of ring polypus forceps. A loop of silver wire may sometimes be passed beyond it, and by pulling at this it may be moved outwards; or the noose of a Wilde's snare may be used in the same way, and the body sometimes by this means be secured and extracted. Young children must have chloroform, not because they are being hurt, but because they will not keep quiet; and without this it is impossible to do any good. Adults will sit still in a chair, as they are not put to any pain. If the foreign body cannot be removed in one of these ways, or if it have passed more than half way down, for the reason shown when speaking of the shape of the meatus, any further attempts will assuredly only have the effect of sending it further in, and will frequently result in a rupture of the tympanic membrane. The ear should be occasionally syringed, and the foreign body will gradually approach nearer and nearer the orifice, until it finally drops out. It may be weeks, or even months, before this happens, but, if it be left alone, it will do no harm. If the tympanic membrane be unfortunately ruptured, this is generally followed by suppuration in the cavity of the tympanum, a discharge from the ear lasting a long time, and more or less permanent deafness. After such an accident, if there be much swelling of the meatus and pain, a few leeches in front of the tragus, followed by fomentations, will be advisable. Anything more energetic than the treatment suggested in the case of foreign bodies in the meatus is thought more likely to prove harmful than beneficial, notwithstanding the numerous instruments that have been brought forward, it is said with the object of getting behind the body and removing it.

In addition to the deafness induced by cerumen when it blocks up the meatus, there is frequently tinnitus, and sometimes of the most distressing character. Anything which causes undue pressure of the stapes on the fenestra ovalis will produce it, and in this case it is brought about by a plug of wax directly in apposition with the tympanic membrane, thus transmitting pressure through the malleus and incus on to the stapes. The same exciting cause will often induce attacks of giddiness, and this symptom has occasionally existed for years without eliciting a suspicion of its cause, the patient in the mean time having been submitted to all sorts of treatment, without, of course, receiving any benefit. This arose from the fact that there was no accompanying deafness to point to a cause, for in these cases the plug of cerumen does not entirely cover the membrane, and so there is ample room for vibrations of sound to fall upon it and be transmitted as before. As a rule, however, there is deafness as well. Accumulations of hardened cerumen, from continued pressure on the membrane, will occasionally produce ulceration of this structure, thus becoming the direct cause of perforation; and in the same way they have occasionally induced partial absorption of the walls of the bony meatus. There are some preparations showing this in the museum of the College of Surgeons. The only legitimate way of removing cerumen is by the use of the syringe, and the best form is one where the nozzle is very small and can be removed from the other part by bayonet action. This form of syringe is quickly filled, and so time is saved; and from the small size of the nozzle the meatus is not blocked up by it. In so simple an operation as syringing the ears there is a skilful and an unskilful method of doing it. The auricle should be drawn upwards between two fingers of the left hand, so as to put the whole meatus as far as possible in a straight line, and the nozzle of the syringe should be kept in close contact with the roof of the meatus. If the secretion is very much hardened, it is well it should be softened by a little warm water poured into the ears for a few nights successively. At intervals during the syringing the ear should be examined with the speculum to see if the canal is clear; for if it is so, considerable irritation may be set up by the stream of water falling with force upon the membrane. It is not very rare to meet with patients who, having had their ears syringed vigorously when there was nothing to bring away, have had inflammation of the membrane set up, which has ended in a perforation. The wholesale way, therefore, in which people get their ears syringed as soon as they feel deaf is to be deprecated.

One of the most troublesome affections of the external auditory meatus is the formation of small abscesses, which may occur at any situation in this canal. They do not seem to be confined to persons of any particular age or constitution. The first symptom is pain in the ear, which in a few hours becomes so acute as to put sleep out of the question. After twenty-four hours or so the meatus in its entire extent will be swollen; sometimes the whole external ear will be enlarged, red, and tender to the touch. The movements of the jaw occasion great pain. There is more or less deafness, in proportion to swelling. If the patient be seen within a day or two of the commencement of the attack, the greatest benefit may be afforded by two or three leeches placed in the front of the tragus, just over the articulation of the lower jaw, and followed by fomentations: in this way the affection may be cut short occasionally. When the leeches are applied, the meatus should be filled with cotton wool, to prevent the blood getting in. If not checked in the early stage, the state of things described will increase in severity for several days, and, on examining with a probe, one point will be felt to be far more acutely tender than any other part of the meatus. The whole meatus is so swollen that it is not possible to see the position of the abscess with a speculum. This point having been distinctly localised, the sooner an incision is made into it the better, as by this the tension of the parts, to which the agonising pain is due, is relieved. The best knife to use is a curved sharp-pointed bistoury, and a free incision should be made as it is withdrawn. After this the patient gets well in a few days. Very frequently, however, he is no sooner well of one abscess than another commences to form and all the suffering has to be endured again. It is this recurrence that makes the affection such a troublesome one. Sometimes six or seven abscesses will occur successively in the same patient, obliging him to remain in the house for as many weeks, and causing an amount of suffering altogether disproportionate to the gravity of the illness. In recognising this occasional peculiarity in these cases, caution will be observed in promising a patient that he or she will be well within a certain time.

The diffused form of inflammation of the external auditory meatus differs from the circumscribed, inasmuch as it does not terminate in abscess, and, as its name implies, in being diffused throughout the integument of the canal. Since the outer cuticular layer of the tympanic membrane is continuous with that lining the meatus, there is always a certain amount of risk lest this membrane become implicated, and for this reason this affection is more serious than the one just considered. It is not strictly correct to describe it as being of two kinds—viz., acute and chronic,—because, although the attacks are acute at one time and chronic at another, their gradations so insensibly pass into one another that an artificial division is practically useless. Children are especially liable to it; and as it is generally neglected among the poorer classes, with them it is not unfrequently the origin of a perforation of the tympanitic membrane. It often succeeds the exanthemata, although not nearly so commonly as purulent catarrh of the middle ear spreading along the mucous membrane from the throat. The diffused form of inflammation of the external meatus commences with a feeling of uneasiness and itching just within the orifice, which very soon becomes red, tender, and swollen; and there is pain during mastication. These symptoms increase in severity, until with the apperance of a discharge, the pain ceases, and the swelling gradually subsides.

Such is briefly an account of its acute stage, which usually occupies a week or ten days. The treatment is palliative—viz., leeches and fomentations; but as this affection does not occur to persons in a good state of health, the diet should be attended to, and a change of air, if possible, be resorted to, for it is most desirable not to let this condition become a chronic one, as it is most apt to do.

Eczema is a common affection of the outer ear, and may be acute or chronic. After repeated attacks (for a patient who has once suffered from it will be very liable to it again) the auricle becomes very much thickened, and the entire meatus shares in the same condition. If there is any deafness, it is only so far as may be accounted for by the narrowing of the canal, which takes place when the disease has lasted for a long time. Like eczema in any other part, it is very obstinate; but if the patients will consent to keep to rules of diet, including an absence of stimulants, they will get well. This is more important than any external applications. Of these very mild mercurial ointments are, perhaps, the most useful; and before they are applied the parts must be carefully dried.

Exostoses are not unfrequently met with in the external auditory meatus, and are not confined to any situation in its course. They are solid bony growths, covered with periosteum. Sometimes there are two or three at one time, and they scarcely ever have a pedicle. On this account, and from their position, they are not easily removed by an operation. Fortunately, this is not often necessary, as they seldom entirely close the meatus, and so do not interfere very materially by their presence with the hearing, but do so indirectly by secretion of cerumen and epidermis collecting behind them; and in this way may be very troublesome. They have been considered to be the result of a gouty diathesis; and the long-continued use of iodine applied locally has been said to induce a diminution of their size. No evidence of either of these propositions can be found. These osseous tumours, or enlargements of the bony meatus, as the case may be, are of very slow growth, and acquire a considerable size without the patient discovering them.

Original Papers.

ON THE NATURE OF THE CONDITION CALLED EPILEPSY.

By J. Thompson Dickson, M.A., M.B. Cantab.,

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In June, 1870, I published a *résumé* of the results of a series of experiments I undertook some time previously, inquiring into the "Nature of the Condition called Epilepsy," and I am indebted to numerous friends for kind expressions of approval of the views I there set forth. Some of the letters I received on the subject confirmed many points I sought to establish in that paper. Other correspondents catechised me, and discussed the details of my facts. The most, however, put to me the practical question, "How do you treat epilepsy?" My original intention was to complete a second series of experiments, and publish the conclusions I might draw from them before proceeding to the record of my results of treatment; but in order that I may fairly and sufficiently reply to all who have done me the honor to address me on the subject, I have determined upon publishing my views and experiences at once, and I propose at the same time offering a few remarks in answer to those correspondents whose letters require special notice.

In premising, I must again set forth clearly and distinctly the fact that convulsion is no essential part of epilepsy, and that the idea of the former must be dissociated from the idea of the latter before the condition we call epilepsy can be clearly understood. In my paper I demonstrated that the manifestation of epilepsy was a concatenation of phenomena dependent upon various abnormal states of the surface grey matter of the cerebral lobes, and that the only constant among the phenomena was loss of consciousness resulting from the proximate cause of the attack—viz., cerebral anæmia produced by contraction of the small arterial vessels and capillaries—the first effect of any irritation of the brain's surface. It will not therefore be difficult to grasp the fact, that if the spot on the surface which becomes the seat of the affection should be a centre presiding over a ganglion controlling muscular movements, convulsions or movements in the muscles so deprived of control will occur; but if, on the other hand, the spot on the surface should not be associated with ganglia controlling muscles, muscular manifestations cannot occur. And what is true of the muscular manifestations is true of all the other manifestations in epilepsy, and so definite and perfect are the relations of control to action that they can be brought into algebraical formulæ.*

Almost immediately after the publication of my paper in June, 1870, Dr. B. W. Richardson wrote to me, stating that, in an independent inquiry, he found that in cases of death by chloroform the stage of chloroform narcotism presented conditions similar to those I had described; my propositions as enunciated being—

1. Epilepsy is a contraction of the cerebral capillaries and small arterial vessels; the order of the stages in an epileptic attack being—1st, irritation of brain, either direct or secondary to exhaustion; 2nd, contraction of cerebral capillaries and small arterial vessels; 3rd, cerebral anæmia and consequent loss of consciousness.
2. The muscular contraction and spasm, together with all the varying phenomena associated with epilepsy, are altogether secondary, and not at all essential or constant, but they are all manifestations of imperfect nervous (cerebral) control, or a loss of balance between the nervous and other systems.
3. The constant and therefore the pathognomonic sign of epilepsy is loss of consciousness; it is the first subjective phenomenon, and it resulted in Dr. Richardson's experiments, the details of which were published in July, 1870.

Dr. Moxon, with whom I have frequently discussed the subject, wrote me as follows:—"All your facts show is, that there is arterial anæmia as the result of the cause of epilepsy; while many facts go to show that mere anæmia cannot cause a fit. Is it, then, that there must be a sudden anæmia produced in a brain nourished by adequate supplies in ordinary times?" In answer to this question, a reference to the paper will prove that I nowhere asserted that mere anæmia was the cause of the fit. My point was, epilepsy is a contraction of the cerebral capillaries and small arteries. Cerebral anæmia is the third phenomenon of the stages of attack, and therefore is not the cause of the fit. I stated that an animal bled to death will always have a convulsion before it dies. This is true, and the convulsion is epileptic; and it seems to disprove Dr. Moxon's assertion that mere anæmia cannot cause the fit; and it would appear to re-attack my own position of the defensive. The truth of this, however, is, that there is a specious ambiguity in the term anæmia, as used by Dr. Moxon; and it is an expression unequal in its force to that of "cerebral anæmia," which I have employed. When an animal is bled to death Dr. Moxon's mere anæmia occurs; but it is a very imperfect state of anæmia, and by no means corresponds with that blanched bloodlessness which is the condition of the brain when it is anæmic as a result of epilepsy. Dr. Moxon's mere anæmia does not produce a fit, but it may irritate the cerebral vessels, and if it does we shall have all the phenomena of epilepsy, and among them, in its proper place, that perfect cerebral anæmia which is one of the results of epilepsy, and the objective to which the subjective phenomena of loss of consciousness is due. Dr. Moxon has, however, in part apprehended my meaning in his question of suddenness. The cerebral anæmia of epilepsy is sudden; it is the effect, not the cause, of the epilepsy, though it is the cause of the suspension of consciousness. The brain in which it occurs may be or may not be nourished by adequate supplies at ordinary times, but there is no living human brain, I believe, in which epilepsy may not be produced. In support of this view I will merely mention the numerous recorded cases of epilepsy which have resulted from fracture of the skull with depression.

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* These formulæ were exhibited and explained to the members of the Hunterian Society, before whom the paper was read, but they are rather too long for publication here.

Dr. Moxon again asks: If mere anæmia were the cause of epilepsy, it would be common in chlorosis; and why in common fainting does not a fit come on? The first of these queries is replied to in the answer I have already given to the question of mere anæmia. In precise terms, however, unless the mere anæmia of chlorosis produces irritation of the brain, there will be no contraction of the cerebral capillaries and small arterial vessels, and therefore no epilepsy. With regard to common fainting, I think most observers, and I am sure Dr. Moxon, will agree with me that it is impossible to draw a line between it and epilepsy. So perfectly does epilepsy run into syncope, and so frequently is that which is called a common faint *le petit mal*, that there is no demonstrable line of demarcation between the two; in fact, I am strongly inclined to believe that the phenomenon of fainting is more often the manifestation of epilepsy than is generally supposed.

Dr. Moxon again says:—"It seems to me that exaggerated irritability of the direct centres of epilepsy is a simpler idea, though not so mechanically graspable. You have, however, only transferred the same exaggerated sensibility to the vascular nerves, and not explained its origin. Why is it easier to think of excitants of the vessel-nerves to spasm than excitants of the motor centres to the same?" A glance at my paper, however, must answer this, for I never spoke of exaggerated sensibility of vessel-nerves; I do not credit these nerves with more than ordinary sensibility; neither is the contraction of the vessels primary, but secondary to irritation. It also seems that, in the first place, Dr. Moxon has passed by one point that I was at some pains to show—namely, that nerve currents only pass in one direction—i.e., in the direction towards the surface of the brain, a fact established by Galvani and confirmed by Niobe; otherwise he could hardly have spoken of exaggerated irritability of the direct centres of epilepsy. He has not, however, defined what the direct centres of epilepsy are, while the idea of exaggerated irritability seems to be wrapped up in the mystifying complications of convulsion and muscular spasm. I may here, for the sake of clearness, illustrate the simple idea of epilepsy by the detail of an experiment. A rabbit was trephined, the dura mater was divided, and the brain, covered with pia mater and arachnoid, was exposed; a knife was then inserted into the brain, which instantly became blanched and bloodless, and the animal at the same moment became insensible. The knife here represents the irritator, and was the cause; the vessels contracted in obedience to their natural and ordinary sensibility, and these circumstances, together with the subjective phenomenon of unconsciousness which resulted, was the whole of the epilepsy. Neither is any epilepsy more under any circumstances, whatever be the cause of the irritation; but according to the character and the seat of the cause, secondary phenomena will be produced, which often complicate and so distort with hideous contortions the simple idea illustrated, that it has been lost to observation, whilst the violent manifestations have attracted and absorbed all attention.

A reference once more to my paper will show that, though I implied that vessels might be channels of irritation, I spoke of the vessels as one only of many sets of channels through which the brain's surface might become irritated, and I mentioned many other sources, such as direct irritation of the surface of the cerebrum, and irritation of the peripheral extremity of nerves; secondly, I must again insist that irritability of the vessel-nerves, when it exists, is secondary to, and the consequence of, irritability of the brain's surface; and thirdly, I think I clearly explained in my paper that the causes of surface irritation might be classed under two heads—the one "direct," the other "secondary to exhaustion." Whilst I stated, both at the commencement and the conclusion, that surface irritation was the cause of the contraction of the cerebral small vessels and capillaries, nowhere did I assert the converse, which would be reasoning by a sort of back logic. A crow is a black bird, but a black bird is not a crow. Irritation of the surface of the brain will cause contraction of the cerebral vessels, and consequently cerebral anæmia; but mere anæmia will not by any means conversely produce contraction of the vessels, and only under some circumstances will it produce irritation of the surface of the brain.

However reluctant we may be to admit the fact, we cannot deny to ourselves the truth that cerebral phenomena are mechanical, and manifestations of motion; and that in epilepsy, as in all diseases of the nervous system, we must look for material causes, and when we do so we shall probably find our object untrammeled and unfettered by the obscurity which hitherto has surrounded it.

Some of the same questions as Dr. Moxon put to me were written at the foot of a copy of my paper by my friend Dr. Elphinstone, so that to this gentleman I make the same reply as I have done to Dr. Moxon. Dr. Elphinstone, however, asks the additional question—how hereditary epilepsy is to be accounted for if my position is to be maintained? The answer to this is not wanting, and goes to make another link in the chain of the evidence upon which my argument is based. There is as much a material cause for transmitted epilepsy as for the manifestation that can be developed in a guinea-pig by section of its spinal cord. The son is not so unlike the father or so different in his nature as he appears or may seem; there is a variation in their education which produces variation in their mental and general development; but still, speaking in general terms, the son bears the nature of his father as the twig does the stock tree. However inexplicable, the inexorable logic of fact shows that impressions of many and various kinds are transmitted, and not the least important of these are the transmitted impressions of the nervous system. Some authors have gone so far as to assert that memories are transmitted, but the evidence of this is imperfect; the impressions of habit, however, cannot be doubted. Father and son will pursue the same idea of life, though they may never have seen one another. Many cases are on record, but I have one within my own observation. Father and son have never seen one another since the son's infancy. The son is now abroad, the father in England; but so alike are these two individuals that there cannot be a mistake as to their having been struck, as it were, in the same die. The peculiarities of the one are the counterpart of the peculiarities of the other: they both walk oddly; they are both excellent shots, but they both raise the gun to the shoulder with an eccentric motion; their habits of life correspond; in personal appearance, the only difference that is perceptible in them is age—a difference of about thirty years; and not the least marked agreement is that of handwriting—so much do the letters of the son resemble those of the father that it is hardly possible to distinguish them; whilst the temperament and temper of the father are reproduced in the son as perfectly as the seed-corn is reproduced in our harvest-fields.

Dr. Elphinstone, and Dr. Moxon too, will probably answer that in this my case is against myself—that I have only shown in regard to a healthy condition what is undisputed, but that I have proved nothing in respect to the unhealthy condition. I think, however, I may at least claim for all the organs of the body, whether healthy or otherwise, the principle of reproduction in the next generation, and the perfection or imperfection of the reproduction will vary with the healthiness or unhealthiness of the organ as possessed by one or both parents, so that a father or a mother with an unhealthy brain will progenerate a son or a daughter whose brain will also be unhealthy. This is abundantly proved by very demonstrable facts. Again, there are instances on record in which whole families have been born with a nævus on the face, and in a corresponding position in each individual. And generally in the skin it is by no means uncommon to see vagaries reproduced in the second generation. If I mention phthisis I may be met with the objection that I am speaking of a constitutional disorder, and am therefore beside my point. But I think no one will doubt the fact of the inherited delicacy of the lungs and the susceptibility of these organs to slight influences observable in the children of phthisics. I think also that no one will question the fact of transmitted susceptibility to disease in the case of the heart. Surely, then, I am not asking for more in the case of the brain; I am only pleading that the same potentiality may be allowed to be possible in the brain as we accord with much less evidence in the cases of other organs; and, once grant that the brain, or any part of it, is in any way imperfect, should the imperfection only be *atony*, as the inherited impression, we have all that is necessary for the production of the whole train of the phenomena of epilepsy. I incline, however, to the opinion that more perfect impressions still are transmitted, and that the impression of the habit of the patient may be reproduced in the child. I know that some of my friends will argue that my own experiments directly disprove this hypothesis since I have not succeeded in breeding epileptic animals from parents in which I have induced epilepsy. But although I have not obtained such results, I accept those of Dr. Brown-Séquard as conclusive on the point it has been done, and I anticipate success in my future experiments; in fact, I anticipate the same result in the course of two generations of epileptic guinea-pigs as we observe among human races and families who are the subjects of brain disease—viz., sterility and eventual dying-out of the family. This, however, is a subject I must not enter upon here.

I pass on now to answer the practical question put to me—namely, how I treat epilepsy. As a general reply I would state that I bear out in practice the principles I indicated in my first paper. I endeavor in each individual case to discover and remove the cause of the irritation. Most of my observations were made in Guy's Hospital, the earliest upon patients in the wards. A large number of cases were out-patients, some of them under my own care whilst I filled the appointment of assistant medical officer; others were patients of Dr. Moxon, who courteously allowed me to pursue my ideas of treatment in respect of them; the remainder of my subjects have occurred in private.

It would be wearisome to detail *seriatim* a number of cases. I shall therefore merely sketch a few illustrative of the uselessness of the tentative method.

In Guy's Hospital, among the patients of the late Dr. Barlow, was a girl aged about twenty. She used to become insensible with very slight convulsions, and remain unconscious for hours. On regaining control she used to tell me that she thought she had been in heaven, and that before each attack she felt as though she had three balls of fire in her head. Her hair was cut short, her head was shaved, ice was applied to her head, and every anodyne that could be thought of was administered to her without effect, and she died. The post-mortem examination revealed three tumours of the dura mater pressing upon the surface of the cerebrum. The existence of the tumours was surmised during her life, so that the employment of any severe remedies would not have been justifiable.

Another patient, the subject of aura, said she felt a numbness in both her little fingers, which gradually ran up her arms and at length met in her head; she then became insensible. In this case it was attempted to check the progress of the aura by tying a cord tightly round the wrist the moment the tingling commenced in the fingers; but this was to no purpose, and the patient died, exhibiting at the post-mortem a cerebral tumour.

I have never attempted section of nerves. I am convinced that such a proceeding would never be successful in relieving the sufferer. I believe on every occasion on which it has been tried it has failed, and the reason, which is the same as that which accounts for the failure of the string experiment in the above-mentioned case, is detailed in my first paper—the cause of the epilepsy in cases where aura exists is not in the periphery but in the centre. I have seen many cases in which, as a tentative remedy, a seton has been introduced into the neck, but with no effect. On one occasion I saw a patient in whom this treatment had been applied, and in whom the recurrence of the fits was suspended. The most, however, that could be said was, that the suspension was *post* and not *propter hoc*. I could trace no association between the cause of the epilepsy and the issue in the neck, and

I doubt very much whether the seton had anything to do with the disappearance of the fits. I had not the opportunity of following this patient; I have, however, very little doubt in my own mind that his relief was but temporary. I have seen patients blistered on the neck and along the whole length of the spine till the exhaustion produced by the raw surfaces was far greater than that resulting from the epilepsy. One instance made a very strong impression on me: the patient was suffering from albuminuria, and the blisters not only aggravated her epilepsy, but produced strangury. I have seen calomel given in doses of from five to thirty grains, in all stages of the attack—at the commencement, during the *status epilepticus*, and on recovery; but I can apply to such practice no terms but those of the strongest condemnation. Bleeding, I believe, has been attempted; I trust it never may be again, as it is certainly fatal.

It is perhaps unnecessary for me further to detail the steps which led me to the conclusion that it was useless to attempt to treat epilepsy *per se;* and that the only rational system of treatment was that which aimed at the removal of the cause. The cause varies in every subject; therefore its discovery in each individual case becomes the first duty of the physician towards his epileptic patient. Acting on this idea, and associating with it the conclusions drawn from experiment and observation, I found that, in respect to treatment, epilepsy divided itself into two distinct classes corresponding with its cause; and that induced or primary epilepsy was often amenable to remedies, whilst hereditary or secondary epilepsy was radically incurable, but often susceptible of modification and such alteration as to afford the patient much relief.

In the primary or first class of case ranks epilepsy from depression of bone after a fracture of the skull. In such a case I should not hesitate to recommend trephining, and I should persist in this no matter at what date after the injury the epilepsy occurred. In cases also of epilepsy occurring after blows on the head without visible or tangible fracture I should still recommend trephining, for the purpose of relieving thereby the pressure produced by a possible fracture or thickening of the tabula vitrea, or, what is more likely to have resulted, a thickening of the dura mater. Before resorting, however, to trephining in cases wanting in tangible or visible depression, notwithstanding the history of a blow or injury, I should first exhaust the treatment by absorbents, using the iodides and bromides, either separately or together, or chloride of ammonium; and, failing to produce result with these medicines, I should employ bichloride of mercury in very small doses, but unless a good effect was very speedily produced I should not push the drug, but resort to the trephine. The same medicinal agents—namely, the chlorides, iodides, and bromides—may be used with happy results in the cases in which cause can be traced to either alcohol or syphilis. Under the prolonged use of alcohol the cerebral membranes will thicken and produce epilepsy, which will disappear with the prolonged use of absorbents; and syphiloma of the cerebral membranes no doubt is as obedient to the influence of absorbents as syphiloma of other structures, though, from the nature of the case, absolute evidence in this is wanting. The cases in which I have suspected syphilis as the cause of the epilepsy, and which I have treated accordingly, have for the most part recovered. In the cases in which history and symptoms lead to the conclusion that cancer is the cause, it may be taken that the case is hopeless; and the same observation applies to cases caused by bone tumours, or bone nodes growing in the skull, of which I have seen several instances.

The next set of cases are those in which the urine is albuminous. The blood of persons passing albumen with their urine is watery and poor, and irritable to the brain; and will often produce epilepsy. If the flux of albumen can be arrested, the epilepsy will disappear. The epilepsy of parturient women is of this kind, and must be dealt with accordingly.

The epilepsy of toxæmia is usually, though not necessarily, fatal; that of uræmia, for instance, which always kills before long, even though the patient may rally after the earlier attacks.

The epilepsy of exhaustion and insufficient supply of blood to the brain is of a more hopeful character. Organic heart disease may thus be the cause of epilepsy. One case I saw in which the patient, unconscious of any ailment, attempted to climb Mont Blanc, but was seized with epilepsy during his ascent. On examination, it was found that he had a regurgitant mitral valve; and the irregularity of his circulation on reaching a moderate altitude sufficed to bring about the epileptic phenomena. Treatment in such cases must of course be adapted to circumstances, and efforts must be directed towards the heart, not towards the brain. In the case of menorrhagia, a not uncommon cause of epilepsy, the indication is plain. I have in such cases found Indian hemp a most valuable remedial agent; but cod-liver oil, iron, and nourishment in every form, must be had recourse to.

Another set of cases are those in which the excitement has its origin in the peripheral extremity of a nerve, in which case the plain indication is to remove the cause, if it can be discovered. If a nerve is involved in a cicatrix, and a touch of the cicatrix produces a fit, one stroke of the knife will in all probability supply a radical and permanent remedy. If carious teeth are the excitants, they ought to be removed; or surface irritation of any form must be treated locally. If the irritation arises from intestinal worms, their evacuation will in most cases be speedily followed by a disappearance of the epileptic phenomena. And this brings me to the point at which special and general treatment overlap one another. Epileptics are frequently the subjects of constipation; and, conversely, constipation, especially in children, is one exciting cause of epilepsy. Therefore it is essential to watch the bowels of epileptics closely, and secure their regular action. For this purpose drugs of severity must be used, but they must be always such as will produce a good action without depression. A saline, combined with a stomachic as sulphate of magnesia and ginger, will be found most serviceable. Such a simple remedy will often keep in check an epilepsy of hereditary origin. Aloein and jalapin are also valuable remedies. In hereditary epilepsy, however, the treatment is much more general than special, and the patient must be closely watched and studied, particularly as regards the time of the recurrence of the attacks, which often exhibit a marked periodicity, especially when the neuralgic form is assumed. The first indication is to nourish the brain. Many of these cases are attacked in the morning on rising. The moment of getting out of bed is often the time of attack. In such cases my practice is to order the patient to take a cup of coffee and a biscuit before attempting to get out of bed. In cases where the attacks are nocturnal much benefit will be derived from a light but digestible meal before retiring to rest. It should be remembered that animal substances are more digestible than vegetable. A little meat is a better supper for an epileptic than gruel or bread, and a little stimulant in addition to the meat is often of value. I knew one patient, a lady, the subject of epileptiform neuralgia occurring in nocturnal attacks, who was always relieved on the assumption of a meal, which not unfrequently consisted of a rump-steak and a glass of stout. This treatment, of course, requires caution and watching.

As regards remedial agents, so far as this set of cases are concerned, very few are of any use, except perhaps belladonna—a drug that should be persevered in, as, by keeping the vessels of the medulla oblongata congested, it reduces to the minimum the hyper-excitability of the cerebral vessels, which are so ready to discharge themselves into the medullary veins, and thus the attacks may be warded off. The quantity administered must be gradually increased, as the patient soon becomes tolerant of the drug. It is well to begin with one-eighth of a grain of extract, which I usually order to be made into a pill with one-eighth of a grain of the powdered leaf: this should be taken an hour before the attack is expected, if the fits are periodical; if, however, the intervals are uncertain, the pill should be taken night and morning, and again in the middle of the day if necessary. The patient will soon be able to take two or three pills for a dose; but he must be carefully watched, and the drug suspended from time to time, and small doses again resorted to on recommencing its use. In a fair percentage this treatment will arrest the recurrence of the attacks—not a small boon to the sufferer; and so successful has it proved in my hands that I am inclined to attach considerable importance to it.

It is right that I should, in conclusion, mention the fact of the power of mental impressions on epileptics. The idea of a new doctor will often ward off the attacks for two or three weeks; for which reason my invariable rule with new patients is to order at first a bottle of water, or of camphor-water or mint-water, and persevere in this until the charm of novelty has worn off; until then it is unwise to push any drug with specific object, whilst the time gained by the mint-water treatment enables the physician to study all the phases of his patient's case more accurately.

Queen Anne-street, July, 1872.

ON SYPHILITIC CONSTRICTION OF THE ŒSOPHAGUS AND PHARYNX.*

By James F. West, F.R.C.S.,

Senior Surgeon to the Queen's Hospital, and Late Professor of Anatomy in Queen's College, Birmingham.

I have already drawn the attention of the medical profession to this subject on two occasions in the *Dublin Quarterly Journal of Medical Science*. My first paper on this matter was published in February, 1860, and the second in August, 1860.

In the first paper I examined the literature of this question. I showed that frequent allusion had been made by some of the older writers on syphilis, as Astruc, Falck, Daniel Turner, and Carmichael, to the fact of dysphagia coming on in the course of confirmed cases of constitutional syphilis, and producing by its continuance great emaciation and debility, and I quoted numerous cases from those authors in support of my belief that such dysphagia was due to syphilitic constriction of the œsophagus. Modern writers on syphilis had thrown doubts on the existence of syphilitic stricture of the œsophageal tube, and their opinions I gave also at some length. The proof that such stricture might exist was given by the *experimentum crucis*, a post-mortem examination of a patient of mine, Jane M——, whose case I had watched with great interest, in whom all the symptoms were well marked during life, and in whom there was found at the necropsy the following state of things:—"The upper portion of the œsophagus for about four inches was much dilated, its mucous membrane thickened and marked by spots having the appearance of recent cicatrices. At this distance from the upper end it was suddenly constricted, and terminated in a narrow canal which would barely admit a No. 4 catheter. This constricted portion, which was about two inches and a half in length, was formed by the thickening of the mucous membrane, and by fibrous deposit in the form of bands and bridles, having very much the appearance of an old stricture of the urethra." This case solved the doubt. Another case of a similar nature, though less severe, I published in the same paper. The girl, H. P——, in whom Mr. Acton took some interest, was under my notice for seven or eight years. Large doses of iodide of potassium were taken with great advantage, and although great difficulty of swallowing occasionally came on, her condition greatly improved, and the last I heard of her was that she was leading a virtuous life, and gaining a respectable livelihood as a laundress at the west-end of London. The paper containing the report of these two cases was republished in the *Archives Générales de Médecine* for June, 1860, without note or comment from the surgical editor; but as M. Follin was then engaged on the first volume of his important work, "Traité Elémentaire de Pathologie Externe," we are able to know his opinions on the subject. After quoting three cases which he had cited in his thesis on Constrictions of the Œsophagus (Paris, 1853), derived from the works of Ruysch, Haller, and Paletta, and mentioning a case of which Mr. Langston Parker was the reporter, and to which I shall again allude, he finishes by saying:—"Enfin, West a publié sur cette question deux observations qui lui sont personelles; la première, dans laquelle l'autopsie du malade a été faite, est seule probante, et *elle suffit pour faire admettre l'existence du rétrécissement syphilitique de l'œsophage.*" M. Follin states that he himself has seen two cases of œsophageal dysphagia which might have been connected with syphilis. In one of the cases there existed a psoriasis in the palms of the hands, and the patient recovered without the use of the sound; in the other the lesions were more deep-seated, and the cure was not complete. These were cases doubtless of cicatricial lesions which may be ameliorated, but which cannot be cured.

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* Read before the British Medical Association at their late meeting.

In August, 1860, I published my third case, in which again the necessary evidence was supplied by a post-mortem examination of ulceration and contraction existing in the lower part of the œsophagus in a patient who had suffered during life from well-marked symptoms of constitutional syphilis, and in whom, in fact, there was no other cause for death, beyond the debility and anæmia which are always present as marked features of the syphilitic cachexia.

Since that period I have had several cases under treatment where dysphagia existed as a symptom of advanced constitutional syphilis; but in most instances the disease has been due rather to the results of ulceration of the pharynx than of the œsophagus, and has consequently been more amenable to treatment: none of them have, at any rate, proved fatal. Mercurial vapor baths, iodide of potassium, in doses of ten to twenty grains three times a day, with the local use of Adams's vaporiser, by means of which the vapor of carbolic acid, tannin, and other astringents and detergents has been conveyed direct to the affected parts, have enabled me to cure all the cases I have had under treatment at a comparatively early stage.

Recently, however, I have had a case under notice where the mischief arising from the contraction caused by ulcers of the fauces and pharynx had existed a long time, and was so great that the patient was in danger of death from starvation, if not from suffocation; for the opening into the pharynx was so small that hardly a goosequill would pass. The opening into the posterior nares was also nearly lost, an aperture of the same size as that passing down to the pharynx alone existing. Asphyxia was imminent in this case, when Mr. Gilbert Smith, house-surgeon of the Queen's Hospital, performed laryngotomy, and saved life. Still, however, the contraction of the pharynx had to be got over, and a consultation was held as to whether œsophagotomy should not be tried as the best means of relieving the patient from imminent death by starvation. The intense constriction of the upper part of the pharynx was, however, at last overcome by the persistent use of bougies, at first by small urethral, but afterwards by larger prostatic, and eventually by œsophageal, bougies. The largest sized œsophageal bougie can now be passed without much difficulty, except the involuntary straining to which such an operation always gives rise, and the patient may now be said to be cured.

This case I venture to bring before the Association, not only because of its great intrinsic interest, but also because it may elicit the opinion of the many well-known syphilographers—M. Ricord, Mr. Acton, and others—who are present, as to whether the circumstance of syphilis producing constriction of the œsophagus and pharynx, to which I have called attention, is worthy of belief: also whether it has occurred to them in their practice to witness such constriction, and if so, in what way they have been able to overcome them.

Very little notice seems to have been taken by English surgeons of the three cases published by me in 1860, but in France and America they have attracted observation.

Mr. Henry Lee has not called attention to the fact of the œsophagus or pharynx ever being constricted as the result of syphilis, in his article on that subject in Holmes's System of Surgery, vol. i., published in 1870.

Mr. Langston Parker, who at our last meeting in this town was president of the Surgical section, and whose loss the medical men of this district greatly lament, mentions my first case in his work on Syphilitic Diseases, published in 1860; and he himself reports the case of a gentleman who had secondary syphilitic ulceration of the pharynx and great difficulty of swallowing, the result of which was fatal, the patient dying exhausted while on a voyage to the Cape of Good Hope for the benefit of his health.

Yvarew records, in his "Métamorphoses de la Syphilis," two cases where difficulty of swallowing was caused by ulcers in the pharynx. (p. 388.)

Virchow gives a case where the superior portion of the œsophagus was contracted by a syphilitic cicatrix. ("Syphilis Constitutionelle," p. 88.)

Culler's valuable work ("Précis Iconographique des Maladies Vénériennes," 1866), though abounding in illustrations of almost every form of syphilitic affection, does not refer to syphilis as affecting the œsophagus.

Lancereaux, in his admirable treatise on Syphilis, published by the New Sydenham Society in 1868, gives an historical *résumé* of the cases which have been reported both by ancient and modern surgeons, quotes *in extenso* my first case, and expresses his belief in the syphilitic origin of the contraction of the œsophagus. He calls attention to the change in the liver noticed in that instance as further proof of the existence of syphilis, and concludes by saying, "The action of syphilis upon the œsophagus cannot be contested, and there is reason to believe that the cicatricial lesions pointed out in the foregoing cases are the result of morbid modifications analogous to those observed in the pharynx after ulcerative gummy deposits." Dr. Lancereaux does not appear to have met with any cases in his own practice.

Dr. Bumstead, of New York, in his essay on the Pathology and Treatment of Venereal Diseases, gives an abstract of my papers in the *Dublin Quarterly*, and then says:—"In reviewing this subject, it appears extremely probable that Mr. West is right in his conjecture as to the cause of the stricture in the cases which have come under his observation, since we may readily admit that syphilitic ulceration of the fauces may extend to the œsophagus or attack the latter as a primary affection."

Dr. Melchior Robert,* of Marseilles, does not give special attention to constriction of the œsophagus, but speaks frequently of difficulty of deglutition as associated with syphilitic ulcerations of the pharynx and larynx; he is, however, evidently familiar with syphilitic mischief as affecting the œsophagus, because, in speaking of the post-mortem appearances of those who have died from the syphilitic cachexia, he says:—"L'autopsie montre différentes lésions dans la gorge, le larynx, l'œsophage, et les intestins."

The most recent contribution to this branch of medical literature is an article by my friend Dr. Clapton, in the last volume of "St. Thomas's Hospital Reports" (1871). He comments on the rarity of stricture of the œsophagus, and illustrates it by the fact that only five cases have presented themselves at St. Thomas's Hospital during the last six years, and says that, although cancerous infiltration is by far the more common of the two causes of stricture, it may depend on fibrous thickening. He then quotes the case of a man, aged fifty-five, now under his treatment, but nearly cured. The patient does not admit having had syphilis, but there is no other adequate cause for the disease, although undoubtedly a stricture had existed at the lower part of the œsophagus, which had very nearly proved fatal. Dr. Clapton believes that it was "due to thickening of the mucous membrane and submucous fibrous deposit. There are," he says, "several instances on record of such an affection occurring idiopathically, and leading to transformation of a portion of the parietes into a fibrous or fibro-cartilaginous substance, independently of precedent ulceration of inflammation. What the diathetic condition which led to it in this man may be it is difficult to determine; probably gouty, as his mother suffered much from gout." I cannot agree with Dr. Clapton in this opinion. The history states that he had never had gout or rheumatism; but although he equally denied having had syphilis, I cannot help thinking the cure of the case by iodide of potassium and iodide of iron, the amendment appearing almost coincident with the use of the former remedy, renders it extremely probable that the disease was due to syphilitic contraction of the œsophagus, occurring, as in my first and third cases, just above the cardiac extremity of that tube.

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* Nouveau Traite des Maladies Veneriennes. 1861.

I now give the history of the case of constriction of the pharynx, to which I wish specially to call attention:—

Isabella E——, aged twenty-six, dressmaker, was admitted into the Queen's Hospital Jan. 23rd, 1872, under my care. She has been married thirteen years. Eight years ago she was under treatment in the out-patient department of this hospital; she then complained of sore-throat, to which she had been subject for two years; she had also had a rash on her back and chest, and had lost her hair. For the last eight years she has been on and off an out-patient, principally on account of the throat, which would heal up and "break out" again. She has had nodes on the frontal bones and tibiæ. Five months ago she left off attending the hospital; the throat was then, as she says, beginning to "grow up." The patient has had the greatest difficulty in swallowing and breathing; she can only swallow liquids, and those in small quantities. Her breathing is so difficult that she is at times almost suffocated. On examination the soft palate is seen to be ulcerated and cicatrised to the base of the tongue, having on one side an opening only the size of a small pea; through this opening the food has to pass and the function of respiration is performed. Patient has an anxious, careworn look, and has lately been getting much thinner, not being able to swallow a sufficient quantity of food for her proper nourishment. She has come into the hospital to have, if possible, this condition relieved. There is a second opening of the same size into the nares, the soft palate being everywhere adherent to the back of the pharynx. She was ordered a mixture containing twenty grains of the iodide of potassium and a drachm of tincture of chinchona three times a day, and a careful examination was made of the parts with reference to an operation, the nature of which was still under consideration, when on Feb. 1st, about midnight, the patient was seized with the utmost difficulty of breathing; this difficulty increased so much that in about half an hour she was nearly asphyxiated, and it was necessary, in order to preserve her from suffocation, to open the windpipe through the crico-thyroid membrane. This operation was skilfully performed by Mr. Gilbert Smith, the house-surgeon, a vertical incision being made through the skin, and a transverse incision through the membrane. There was no hæmorrhage; the operation was perfectly successful, and was attended with immediate relief.

2nd.—Patient has passed a comfortable night, and is becoming accustomed to the tube. The irritation is not more than is usually caused. Pulse 100; temperature 99·4°. To take the following mixture three times a day:—Iodide of potassium, twenty drachms; ipecacuanha wine, fifteen minims; tincture of opium, five minims; water, one ounce.

5th.—She is now able to wear the tube without much inconvenience. It was resolved to attempt to dilate the aperture in the soft palate by bougies. A No. 10 urethral elastic bougie, the largest that could be introduced, was passed twice a day. Pulse 100; temperature 99°.

6th.—The throat has been made rather sore by the passage of the bougie. Nos. 11 and 12 were passed to-day. Pulse 102; temperature 98·8°.

7th.—Bougie, which had been passed daily, caused such great pain that it was obliged to be discontinued. She has slight bronchial irritation. Pulse 98; temperature 99·6°.

10th.—Bronchitis relieved a little; respiration accomplished with great difficulty. The tube causes great pain. Pulse 90; temperature 98·4°.

14th.—Throat very painful, and appetite very bad; patient very desponding. Pulse 88; temperature 98·4°.

19th.—Appetite bad; complains of pain in the left breast, right shoulder, and lumbar region. A bougie is passed almost daily. Mustard plaster to chest.

23rd.—Bronchitis fast disappearing; she looks much better; throat not so painful.

26th.—Still improving, though very weak. She experienced much benefit from the hot poultices. Pulse 98; bowels regular. A larger-sized bougie passed to-day.

29th.—Feels much stronger to-day.

March 4th.—Greatly improved; throat not so painful. Largest bougie passed again to-day.

8th.—Very much better; bougie passed daily. She left the hospital at the end of March; but was ordered to continue taking her medicine regularly, and to come to the hospital twice a week to have a bougie passed.

June 10th.—The patient has come up for the first time since her discharge, complaining of slight difficulty of swallowing and breathing. A full-sized bougie was passed, which gave relief, and a mixture of iron and iodide of potassium was ordered to be repeated.

In contributing this case I may be thought to be departing from the subject of my paper; but originally the title sent to the secretary of the Printing Committee was "On Syphilitic Constriction of the Œsophagus and Pharynx." The subject of the present paper did not suffer from syphilitic laryngitis or œsophagitis, but simply from a constriction of the upper part of the pharynx, caused by the cicatrisation of extensive ulcers in that situation. Repeated examination with the laryngoscope showed that the vocal cords and epiglottis were entirely free from disease; and that the obstruction, therefore, was purely of a mechanical nature, and existed above the summit of the larynx. The case in some respects resembles that recorded by Dr. M. Robert, where M. Ricord, on the 3rd of January, 1848, performed tracheotomy on a patient who was almost moribund with advancing asphyxia, and who would, even during the operation, have died had not M. Ricord, "moved," as M. Robert says, "par un sentiment d'humanité, appliqua ses lèvres sur la plaie de la trachée et aspira à plusieurs reprises le sang et les mucosités purulentes qui s'opposaient à la respiration. Ce dévouement eut pour résultat de ramener le patient à la vie." In this case, however, disease was so far extended that the patient subsequently died. The pharynx, the soft palate, and the epiglottis were found to be lost to a great extent. M. Ricord's case, then, helps to show that syphilitic disease *may begin* in the pharynx. My case shows the result of loss of substance in the situation—namely, such constriction of the pharynx as would have led to death by asphyxia or by starvation had not suitable treatment been adopted.*

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THE TREATMENT OF SMALL-POX BY VACCINATION AND THE HYPODERMIC INJECTION OF LYMPH.

By R. C. Furley, L.R.C.S.

In replying to Dr. Grieve's comments and cases of the above treatment, it is necessary to clear out of the way some matters more personal than relevant which Dr. Grieve has introduced into the controversy. First, he refers to letters sent by me to a daily newspaper in which I lauded this treatment. This is perfectly true, and I am lauding it still. At the time those letters were written a large number of people were afraid to get revaccinated, as it was considered certain death if the "poisons met". Some medical men refused to vaccinate under the same fear, and others turned away patients who came for revaccination because the mark of primary vaccination was visible; and through the above fear I could not get patients to submit to my treatment. No sooner were my letters published than such foolish fears were completely dissipated, and medical men were calling on those whom they had previously refused to vaccinate, and the people flocked to get revaccinated, one gentlemen saying at the Obstetrical Society that since then he had vaccinated at least a hundred every week. The Society generally acknowledged that, so far as this was concerned, the letters had done a great deal of good; I am therefore not ashamed of doing good.

The next subject introduced by Dr. Grieve is a delicate insinuation that I am practising homœopathically. "It is," he says, "by their great similarity, if not virtual identity, that the vaccine acts upon the variolous disease; and, if Mr. Furley is right in his deductions, in this instance at least the dogma of homœpaths is correct, *similia similibus curantur.*" Now, on this point, the poisons may be identical in origin, but how different there effects! Two seeds may be very much alike, but when the plants grow the similarity has vanished. "By their fruits shall ye know them." Will Dr. Grieve take the microscope and point out the identity?

Dr. Grieve does himself an injustice in "supposing" that all my patients were "unvaccinated," or "that they were between the fifth and twentieth years of age, and had four or more good marks of primary vaccination"; because he has evidence to the contrary. He cannot seriously oppose evidence by suppositions; nor should he "imagine" my sixty cases to be mild when I stated I had deaths.

I come now to the point at issue between us. I take it that our relative positions are, that Dr. Grieve is the defender, and I am the assailant, of the views generally held by the profession. I stated the proposition that "vaccination is not only prophylactic, but *curative.*" Now what should guide us in this inquiry? Surely reason and observation. What reasons, then, can be adduced in favor of the proposition? First let us examine Mr. Marson's rule—the correctness of which I presume Dr. Grieve admits,—and see if there is nothing in it in support of my view. "Suppose," he says, "an unvaccinated person to inhale the germ of variola on a Monday. If he be vaccinated as late as the following Wednesday, the vaccination will be in time to prevent small-pox from being developed. If it be put of till Thursday, the small-pox will appear, but will be modified. If the vaccination be delayed till Friday, it will be of no use." Here, taking the first part of the rule, the variolous poison is first in the blood, and lymph is added. What becomes of the variolous poison? It suffers death, or what is equal to it, it is reduced to inactivity. By what means has this effect been produced? Admittedly by the vaccination. Even if done on the Thursday it has a curative effect, though not to the same extent: "the disease is modified." After 12 o'clock on Thursday night, however, Mr. Marson says it is of no use. This, of course, is the real point in dispute. Is it reasonable to suppose that vaccination, which a few hours before was capable of doing so much good, suddenly, as it were, refuses to render any further aid? I think not; and various writers in THE LANCET have shown that secondary fever does not ensue if the vaccination is concurrent with the variolous disease. Now, what does the absence of secondary fever prove? Dr. Bakewell has pointed out that the secondary fever—or fever of maturation, as Trousseau terms it—is a true surgical fever, and arises from the pressure on the true skin of the variolous matter contained in the pocks. The absence of secondary fever, therefore, proves a diminution of the amount of variolous poison developed. It will tend to fortify rather than to weaken my position if I turn one of Dr. Grieve's own arguments against himself (I am always assuming that he is the champion of the present views, none else having come forward to vindicate them), and the argument is this, as stated by Dr. Ballard:—

"Let us now inquire into the *modus operandi* of the virus (vaccine),—what it has been doing to produce these results, and how it brings them about. We have two things to explain—the production of a local eruption, and the production of a general influence, accompanied in the instance of small-pox with a general eruption also: that is, we see constantly in small-pox and vaccinia a local multiplication of virus; and constantly in small-pox, exceptionally in vaccinia, a systemic multiplication of virus, *E nihilo, nihil fit.* If virus is thus multiplied—manufactured, so to speak,—it must have been manufactured out of *something*. A seed may sprout, the germ may develop, but it is at the expense of the 'albumen' about it and the water it imbibes. It will not grow into a plant, and itself produce more seed, unless it be supplied with appropriate nutriment from the soil and atmosphere. So there must be something in the body into which a virus is inserted to nourish it and permit of its reproduction—a pabulum. This pabulum is believed to exist in the blood—the blood which circulates through the frame, whose 'home' where it remains longest is the capillary vessels reticulated through all the tissues of the body. What is it? It is not anything absolutely essential to the blood, in the sense that it is necessary to that fluid as the source of supply of nutritious matter for the tissues and organs; it is nothing essential to their well-being, for when it is absent, as it is sometimes, from the blood, the body thrives just as well without it. Still it is something natural to the blood, since few people exist in whom it is not present. Its actual nature is not understood. All we know of it is, that the growth and multiplication of the virus does away with it, just as the growth and multiplication of the yeast plant does away with the sugar in an infusion of malt."

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* M. Ricord, after hearing this paper and thanking the author for his observations, said that, although very familiar with stricture of the pharynx, he had never met with a case of stricture of the œsophagus.

Now, supposing this theory correct, we know that from four to eight vesicles successively produced will entirely exhaust this pabulum, while it will take thousands of pocks to bring about the same result. Each vaccine vesicle therefore represents some hundreds of pocks, or a corresponding diminution in their volume. Dr. Grieve will kindly point out the fallacy of my reasoning here, or admit that there is some room to doubt the correctness of the theory. The latter course I myself take, as I think it impossible for the pabulum, whatever it might be, not to be speedily restored, taking into consideration that the blood is nourished by substances from without, and that there is ingestion of a variety of such substances. But it is one thing to find fault with a theory, and another to replace it with a better. From my experiments, I am led to the conclusion that the protection against small-pox is the actual presence of lymph in the blood, and that the pabulum of vaccinia and variola is the pabulum of any other cell formation. This theory seems much more intelligible, and explains almost at once the many anomalies connected with the disease—such as taking small-pox immediately after successful vaccinnation—cases that become aborted, mild or severe. It explains how Mr. Marson is correct as to quantity being chiefly concerned in effecting a good protection. It explains the necessity of revaccination at a subsequent age, as the quantity of blood has increased, while there is no proof that the lymph has multiplied in the system in proportion. Suppose the quantity of lymph required to afford full protection to be represented by the number 8, then any less quantity down to 0 will represent cases of corresponding mildness or severity. I put forward this theory because, even if not correct, it is reasonable, and it is the theory on which my practice is based—namely, to supply the supposed deficiency in any given case. Does lymph circulate in the blood? If this be doubted, how can the areolar stage of vaccination be accounted for, unless by the absorption of lymph from the vesicle? But an areolar stage, exactly resembling the one occuring in ordinary vaccination, can be produced by injection in three or four days in a healthy individual, although it is not advisable, owing to its severity, except in persons who are exposed to the disease, and on whom ordinary vaccination has been unsuccessful. In some such cases I have injected lymph without producing any local effect or any symptoms of constitutional disturbance. I have no doubt therefore, that the real protective power is not the destruction of a pabulum—not the production of a disease, else revaccination would, generally speaking, be unnecessary,—but the circulation of lymph, or the products caused by its presence.

Coming to Dr. Grieve's cases, I have to thank him for the minute and painstaking manner in which they are recorded, and for the comments attached to each case. I only wish it had been in my power to have imitated him in this respect, but the exigencies of general practice, as a rule, preclude those so engaged from recording their cases so minutely as they would wish, or as the cases deserve.

There is one point in his cases which has struck me very much, and that is, the number of abscesses he observed. Assuredly, if I had seen any such mishaps in my first seven cases, I would have come to the same conclusion at which he has arrived, nor would the treatment by injection ever have been recommended by me. But it was a matter of remark and surprise all along how little disturbance was produced by the injection. When I wrote of my sixty cases, I had not observed anything of the kind at all. Since then I have observed it once, and that case is seventieth on my list, in which three small abscesses formed in succession, which I attributed to restlessness during the operation; for I invariably lifted up the skin after the needle was inserted so as to avoid injuring any other textures. How to account for the difference in Dr. Grieve's experience and mine as regards this I am quite at a loss to give any reason, unless it be the presence of his patients in a public hospital. It cannot be the operation, or I should have observed it more than once; indeed, I looked upon the absence of these troublesome complications as one of the virtues of the treatment.

Even at the seat of the operation I have not seen any irritation equal to one-third of what usually occurs in an ordinary case of vaccination, and I have injected as much as three tubes into each arm.

It will be remembered that in my paper in THE LANCET I mentioned that "the treatment was much more successful in the youthful than in the adult period of life," and that I thought this was owing to adults "requiring more of the remedy." Futher experience bears me out of this opinion, as I have since then injected six tubes in two cases in which the eruption appeared copious, and with the effect of both cases aborting on the fourth day. Quantity seems to be everything in the matter; but the great difficulty has been to have an intelligent basis from which to proceed. Before summarising my cases, I may quote a case in which the variolous eruption *re*ceded as the vaccination *pro*ceeded. In the latest edition (1871) of Sir Thomas Watson's Practice of Physic (vol. ii., p. 950) is the following passage:—"The following case, related to me by Dr. Johnson, shows that vaccination *may* curb the small-pox even when performed at a later period after the reception of the variolous poison. A boy in a house where a fatal case of small-pox had occurred was vaccinated for the first time. About the fourth day after the operation, when the vesicles were beginning to rise, he sickened with small-pox. The initiatory symptoms were severe, and an abundant eruption appeared. About the fourth day, however, the variolous eruption rapidly subsided, and the child was convalescent. Meanwhile the vaccine pustules pursued their regular course. The vaccination was just in time to arrest the progress of the small-pox." Now, with all due deference to Sir Thomas Watson and Dr. Johnson, I think that they have drawn a wrong inference from this case. They seem to regard it as one of prevention. But it is not so; for the disease was outwardly and manifestly there, and yet it disappeared.

I have seen this complete disappearance of the eruption five times. I have already described two of the cases, and will now detail the other three.

Case 3.—A. B——, aged twelve, was vaccinated in infancy, and revaccinated three days before he sickened with the small-pox. The eruption made its appearance on the fourth day, when I injected the lymph of one tube. Next day the redness surrounding the vesicles was fainter. The following day the vesicles themselves were not so prominent. On the fourth day after the eruption had appeared all trace of it had gone. I know that this boy was exposed to the infection.

Case 4.—A. S——, aged six, unvaccinated, was first seen on a Monday, being the second day of the eruption, which was very abundant on the face and extremities. I injected two tubes of lymph. On Tuesday the redness was disappearing. On Wednesday the eruption was receding. On Thursday it was nearly gone, except on the arms. On Friday there was no trace of it except on the uninjected arm, where bluish marks were visible, similar to those of a person who had passed through the disease. On the eighth day after the injection there was only a black mark at the point where I had injected, which on the thirteenth was covered by a little scab like a seed-pearl. There never was any appearance of irritation at the point of injection.

Case 5.—A. H——, aged four, with one vaccination mark, and whose brother and sister had small-pox. The initiatory symptoms were not severe. On the second day of the eruption the vesicles contained the characteristic fluid with the surrounding areola. I injected the contents of three tubes. On the third day the areola round the vesicles on the injected arm was not so bright. By the fifth day the eruption had totally disappeared, without leaving a mark or casting a scale.

Now these cases show, as distinctly as cases can show, the curative nature of vaccinia. It might be asked, however, if I was really certain that they were cases of variola at all. They were so, for the initiatory symptoms were present, and the eruption appeared; but the greatest proof of all was the small local effect produced by the injection, for in only one was there a little areolar inflammation about the size of a threepenny piece.

Excluding cases that seemed so mild that they required no treatment, and those that I allowed to take the regular course for the purpose of contrast, and such cases as had to be removed to the hospital on account of landladies being unwilling to nurse them, I have treated in this way seventy-four cases in all. In five of these the eruption entirely disappeared. In forty-three the disease aborted on the fourth or fifth day of the eruption. Of mild cases—and by that I mean where they reached the pustular stage, but did not require to keep in bed—there were thirteen. Of severe cases requiring to keep in bed there were seven, and six were fatal. Of the fatal cases, one was first seen on the fifth day of the eruption, and was bleeding from the gums at the time. One tube was injected. The second case was just recovering from an attack of erysipelas which recurred all over the body (scarlatiniform); and there was only a little eruption on the calves of the legs to denote the exact nature of the disease. She was pregnant. One tube was injected. The third case was a woman aged seventy-two, who had an attack of bronchitis at the time, and she really died of that disease, although it was aggravated by the small-pox. One tube was injected. The fourth case, unvaccinated, occurred at the "change of life" and was desiccating very favorably, but suddenly died on the eighteenth day. One tube was injected. The fifth case gradually sunk. There were no other lesion that I could discover. The menses came on during the eighth day. One tube was injected. The sixth case was hæmorrhage, and two tubes were injected. I was anxious to inject more, but was refused. All the fatal cases were female; and all occurred, with the exception of the last, before I deemed it prudent—considering I was only investigating—to inject more than one tube. I have had no fatal case where I injected three or more tubes. Of those in which the eruption disappeared, three had not been vaccinated previously, and two had. Of those in which the disease aborted, all had been vaccinated. Of the thirteen who had the disease mildly, only one had not been previously vaccinated. Of those who took the disease severely, four had been vaccinated, and three had not. Of the fatal cases, three had been vaccinated, and three had not. Of the total number of cases, nine were confluent, and the others were discrete.

In conclusion, from contrasting cases let alone or treated otherwise, and from the statements of others, I am more than ever convinced of the general correctness of my proposition. More thought and more investigation no doubt it will take to establish it, but these will be obtained through time.

Edinburgh, July 31st, 1872.

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THE RELATIVE FREQUENCY OF DISEASE BETWEEN THE RIGHT AND LEFT SIDES OF THE HEART;

DEGENERATION OF THE HEART, ITS CAUSES AND MEANS OF AVOIDANCE.

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If the question were asked, "Which side of the heart is the more frequently affected by disease?" the answer in perhaps nine cases out of ten would be that the left side of the heart is the one which more frequently suffers. This answer would not, however, embrace the whole truth. It would be true of the aggregate of cases of cardiac disease without reference to age; but it would be untrue if the occurrence of cardiac disease were referred to the later periods of life. If a man lives to the age of forty years without having suffered from cardiac disease, and if after that period the heart becomes affected, the mischief will, as a rule, be found to exist on the *right* side. If, on the contrary, cardiac disease should occur before that age, the disease will almost invariably be found to exist on the *left* side. Hence it follows that the right side of the heart is the seat of cardiac disease occurring after middle age—the left side of the heart the seat of cardiac disease occurring before middle age.

As in time, so it is with respect to the nature of the diseases which affect the right and left sides of the heart respectively. Those of the right side are the result of tissue-degeneration, or of mere mechanical influences; those of the left side are almost invariably the product of inflammation. The former are diseases which tend to widen the valvular apertures and to dilate the right side of the heart; the latter are diseases which tend to contract the valvular apertures and to increase the size and bulk of the left side of the heart.

Disease of the right side of the heart is essentially passive and secondary in its character; disease of the left side of the heart is essentially active and primary in its character. I speak now of disease when it occurs, not when it has existed for some time. Active inflammation of the left chambers of the heart arises; it progresses to a certain extent; treatment subdues it; the patient recovers; but a certain amount of damage is left behind. Years pass on; the patient during this time appears none the worse for his previous illness; but at length pulmonary symptoms suddenly manifest themselves, and then it is that the physician discovers that the left side of the heart is permanently damaged, and that the present condition of the lungs is traceable to this cause.

In this instance the mischief in the heart inducing this condition of the lungs is not, strictly speaking, active. The first step of the cardiac disease was active; but the second step was chronic. Bit by bit—increment by increment,—after the patient's apparent recovery from the primary attack, is the valvular lesion left by such attack added to, not perhaps constantly, but intermittingly, until at length the aggregate increments of addition so hamper, oppress, obstruct, and distort the mitral, or the mitral and aortic valves, that secondary consequences begin to follow.

In such a case the cardiac disease producing the first degree of valvular lesion was *active*, or *acute;* whilst that which really induced the secondary consequences—congestion of the lungs, hæmoptysis, hypertrophy of the lower lobes, or hypertrophy of the left ventricle—was essentially chronic.

Why are the affections of the two sides of the heart essentially different in their nature? Why do those of the left side of the heart point to an inflammatory origin; those of the right side of the heart, with but few exceptions, to a non-inflammatory origin? There must be some cause for this difference. What is it? The reason is found in the difference which exists between the constitution of the blood which reaches the left side of the heart from the lungs and that which reaches the right side of the heart from the general system. The blood reaching the left side of the heart from the lungs has been replenished with all the elements necessary for the growth of the tissues; it has been purified, renovated, and vivified by its oxygenation in the lungs, and it is thus rendered in the highest degree stimulating to the left heart. The blood reaching the right side of the heart from the general system has been deprived, by the requirements of growth, of the chief portion of its nutrient materials; it has been fouled by the débris of tissue-waste; it has been further poisoned by its impregnation with carbonic-acid gas: it is therefore a depressant, rather than a healthy excitant, to the right heart. True, it brings with it to the chambers of the right heart the products of the digestion of food; but what are they, either as nutrients or excitants, when they reach that point? They are no more than inert, unusable, passive elements. Not until they have passed to the lungs, and have there received the vivifying influence of oxygen, can they enter into the real composition of the blood, and thus become active, exciting, disposable constituents of it.

"Like begets like" in very many instances. This axiom is true in relation to diseases of the heart. The rich, stimulating blood of the left ventricle urges, feeds, and actively supports any disease which may arise at that point; whilst the poor, impoverished, fouled, tainted, and attenuated blood which flows through the cavities of the right heart favors disease of a correspondingly low and degenerate character.

So long as the body is rapidly built up and as rapidly pulled down, disease of the left heart maintains an active character; but when the balance between nutrition and waste is destroyed—when nutrition becomes less active, whilst waste remains the same, or is more active than before,—disease of the left heart loses more and more of its active character, and approximates more and more in its nature to disease of the right heart. In many this change begins at the age of forty; in others, not until five or ten years after that period. Thenceforward the tendency to inflammatory disease of the left heart declines—the tendency to degeneration increases. With the gradual declination of the one tendency and the gradual increase of the other, a period is at length reached when active inflammatory disease ceases, as a rule, to affect the left heart. At this juncture the left and right sides of the heart, hitherto dissimilar in their tendencies, are in this respect almost as one. The active tendency of early life has given place to the passive tendency of advancing years—inflammation to degeneration.

Acute rheumatism—a fruitful cause of cardiac disease in the earlier periods of life—is seldom seen beyond the age of fifty. I have, however, attended a case of acute articular rheumatism at the age of seventy-five; but such an instance was an exception to the rule. After fifty, acute rheumatism gives place to a form of rheumatism which slowly produces rigidity of the coats of the blood-vessels, hardens and contracts the tendons, thickens and renders stiff and rigid the ligaments of the joints, hardens and lessens the articular cartilages.

Thus, then, according to a law of nature, the *ultima linea* of life ends in—degeneration.

Apart from the influence of this law, can any accidental, casual, or avoidable circumstance favor this immutable tendency to degeneration, speaking more particularly in reference to the heart? Yes; many circumstances are daily, hourly, momentarily doing this. Thousands annually perish from heart disease, whose lives might and would have been prolonged had but proper attention been given to the simple laws of nature. These laws demand attention to the three great vital functions—the action of the brain and nervous system, respiration, and circulation.

None of these functions must be overworked, as none of them must fall short of their proper duty. Healthy, regular, daily action is their law of life. If the brain and nervous system are overworked, vitality is lowered, the resisting power of the body is diminished, disease is easily produced. If the brain and nervous system are underworked, the generation of nervous power is low and deficient, the vitality of the tissues becomes low in proportion, and disease is easily excited. Overwork exhausts, ruins, kills the body, just as the continued generation of the galvanic current exhausts the acid and wears out the zinc plate. The weakest point of the body has to bear the result of this violation of nature's laws. If the heart is that point, disease falls upon it, and death before the legitimate term of man's existence is the consequence.

To keep the body in perfect health it must be duly oxygenated. There must be free and ample interchange between the blood in the lungs and the air entering the pulmonary cells. The life-stream must be purified by its elimination of carbonic acid; it must be vivified by the absorption of oxygen. The fulfilment of these conditions demands a full, free, and constant admission of pure air into the lungs. This full, free, and constant admission of pure air cannot be obtained in badly-ventilated houses, crowded buildings, schools as at present constructed, theatres, manufactories, pits, underground railways, and the like.

When the body has reached that age at which natural decay or degeneration has begun, the absence of pure air hastens and increases the degenerative tendency. Where the heart is more prone than other organs to disease, the want of pure air falls with powerful effect upon the tissues of the right heart. Their nutrition is defective by reason of the impurity of the blood with which they are fed, their vital force is lowered, their muscular fibre loses its tonicity, degeneration and debility take the place of active nutrition and power. If in this condition any stress is thrown upon the heart by hurried walking, by lifting, climbing, violent declamation, passional expression, singing, laughing, or by any unusual exercise of the voice, the tricuspid valve gives way, it henceforth fails to close its aperture, and the results of a back-flooding of blood upon the venous system of the body begin to follow. If none of these exciting causes occur, the continued breathing of impure air is followed by constantly progressing degeneration of the tissues of the valves and muscular structure of the right heart; they become soft and feeble, their atoms shrink; the segments of the tricuspid are at length unable to meet in their attempt to close their aperture; a small chink or slit is left between them; through this the blood finds its way into the auricle above at every contraction of the heart; and soon regurgitation is followed by the secondary consequences produced in the general system—congestion of the liver, stomach, spleen, kidneys, bowels —by hæmorrhoids, general dropsy, and occasionally by cerebral mischief.

I hold that the breathing of impure air is a fruitful source of disease of the right heart occurring after middle age. How many people ignorantly favor its occurrence by confining themselves to closely-shut, non-ventilated, hot, stifling rooms, in which the carbonic acid has accumulated to 2 or 3 per cent. of the air they respire! How many are thus destroyed by being compelled, through the exigencies of life, to pass the greater part of their time in pits and manufactories where ventilation is defective, or in which the air respired is poisoned by noxious fumes and offensive emanations from the materials undergoing the process of manufacture! How many are falling victims to the poisonous influence upon the heart of the atmosphere of an underground railway! What do these facts suggest? How are these evil results to be prevented? The simple answer is—Let the rooms in which you live be effectually ventilated by an incoming current of air filtered from all adventitious impurities, and so divided that no draught shall be felt; and by an outgoing current which shall remove from the apartments the carbonic acid, carbonic oxide, sulphurous-acid gas, sulphuretted hydrogen, and other noxious compounds, as rapidly as they are generated. Apply the same principle to public buildings, theatres, schools, manufactories, pits, and to all places in which people are accustomed to congregate.

As to undergound railways, the best plan is to avoid them. True, the time passed in their polluted atmosphere is usually very short; but it is, nevertheless, sufficiently long to paralyse occasionally the heart's action, and always, by its pollution of the blood and by its direct effect upon the nervous system, to favor degeneration of the structures of the heart.

It often occurs to a medical man to visit a patient for the first time, and to find him suffering from a dilated right heart, tricuspid incompetency, some degree of hepatic enlargement, and œdema of the lower extremities. There is no asthma, no emphysema of the lungs, no affection whatever of those organs to impede the onward flow of blood from the right ventricle, no history of cardiac affection in earlier life. The patient may for some short time have been sensible of a change in his breathing on walking rather quickly, or in mounting the stairs, or he may never have felt, or at least recognised, any such sensations. His attention was first arrested by observing that his feet and ankles were swollen, especially at night on going to bed. This sign it is which gives him the first alarm, and which causes him to seek the aid of his physician. An examination of his case detects a dilated right heart, with incompetency of the tricuspid valve. How has this condition of the heart been brought about? There is no history of previous cardiac disease; there has been no illness ushering in the present condition of things; there has never been, nor is there now, any affection of the lungs, and yet the right heart has suffered a lesion fatal to life! The answer is, that every such case has passed the age of forty, that the tissues of the right heart have entered upon the period of degeneration, and that this degeneration has, with very few exceptions, been hastened by the breathing of an impure air, either during the pursuit of the ordinary occupations of life, or in the patient's own dwelling.

When the degeneration of the right heart has progressed to a certain extent, incompetency of the tricuspid valve follows either with or without the aid of an exciting cause. Hence it is easy to understand why dilatation of the right heart and tricuspid incompetency are often found to exist apart from any previous history of cardiac disease.

The third great vital function which influences the degenerative tendency of the heart is that of the circulation of the blood. To preserve the health of the tissues, the blood must not only be pure and rich in the materials of growth, but it must flow with a certain speed through all the bloodvessels. If the speed with which the blood moves is on the side of either *plus* or *minus* of the standard of health, disease will shortly arise. If it is on the side of *plus*, active disease of the heart, where that organ is the one to suffer, will follow. If on the side of *minus*, tissue degeneration will ensue. Active disease will be the consequence before middle age; degeneration after that period.

These facts teach that all violent and long-continued efforts of the body should be avoided. They hurry the heart's action to an inordinate degree, they cause it to throw the blood with great force into the extreme vessels, and, as there is almost always one organ of the body weaker than the others, the vessels of this organ become distended, and remaining distended, the organ itself becomes diseased. Running, rowing, lifting, jumping, wrestling, severe horse-exercise, cricket, football, are fruitful causes of heart disease. Those which require the breath to be suspended during their accomplishment are more fruitful causes in this respect than those which require no such suspension of the breathing. Rowing, lifting heavy weights, wrestling, and jumping do this; and of these, rowing is the most powerful for evil. At every effort made with the hands and feet, the muscles are strained to their utmost; the chest is violently fixed; no air is admitted into the lungs; blood is thrown by the goaded heart with great force into the pulmonary vessels; they become distended; they at length cannot find space for more blood; the onward current is now driven back upon the right heart; its cavities and the bloodvessels of its walls become in like manner distended; the foundation of disease is laid. Hypertrophy, hæmoptysis, inflammatory affections of the heart and lungs, are the consequences in the young; valvular incompetency, rupture of the valves or of the muscular fibres of the heart, pulmonary apoplexy, and cerebral hæmorrhage, are too frequently the immediate consequences in those of more mature years.

If the flow of blood is *minus* the standard of health, the heart's walls are imperfectly nourished by reason of a deficient supply of food within a given time; the blood itself, receiving less aëration, is in consequence more impure; degeneration of the heart's walls is thus induced, if it does not already exist—hastened, if it is present.

(*To be continued.*)

OBSERVATIONS ON THE

FOUR CHIEF ORIFICES OF THE HEART.

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In a paper which I had the honor of reading before the Royal Society in the early part of 1870, I attempted to establish the existence of a law which determines the relative magnitude of the areas of the four chief openings of the heart, and I also sought to show the reasons why the orifices differed from each other in size. I took as the basis of my calculations the facts given by Drs. Peacock, Reid, and others, and I assumed—1st, that the four openings during the time the blood traverses them are circular in form; and 2nd, that the area of each orifice is unchanged—constant—during systole and diastole. I propose in this communication to show that these assumptions were correct.

1st. All anatomical writers agree in describing the orifices of the aorta and pulmonary artery to be circular in circumference. I have, therefore, only to prove that the tricuspid and mitral apertures are also circular, although stated in anatomical works to be elliptic in form. And in the first place, I may recall the fact that all the canals of the body and all the outlets through which fluid has to pass are (with a few exceptions) circular in form, for the obvious reason that the *largest quantity* of fluid can pass with the *least amount* of friction through a circular opening, and therefore with the least expenditure of power on the part of the organ propelling or expelling the fluid. If, for instance, we compare the perimeters of a square, ellipse, and circle, each of which contains an area of one square inch, we shall find that they are—4, 3·85, and 3·54 inches respectively; the ellipse being taken in this case for the sake of example, as having its long axis double the length of the short axis, or as 1·6 to ·8 inch. An elliptic mitral opening (on the above supposition of the length of the axes) containing one square inch would have a perimeter of ·31, or nearly one-third of an inch longer than the perimeter of a mitral opening containing exactly the same area, but of a circular form. The openings of the two different forms would be of exactly the same size in area, but the circumferences bounding these equal areas would be very different in length. The elliptic would evidently expose a much larger frictional surface, and be therefore less favorable for the transit of blood than an opening of an exactly circular form enclosing the same area within its circumference. It would be interesting to discover why the longitudinal and straight sinuses in the cranium have a triangular form. It is clear that the arrangement is not merely for the purpose of packing, for we find that the small veins which empty themselves into the longitudinal sinus enter that channel at an angle, and in a direction *opposed* to the current which flows through it, showing, as it were, that great velocity is not required in the stream of the sinus. The aortic and pulmonic apertures are admitted, as I have already stated, on all hands to be circular in form. It may be, therefore, fairly asked why Nature, which economises force in all parts of the body, should adopt *oval* apertures for the admission, and *circular* openings for the exit, of the blood of the ventricles, especially as the incoming tricuspid and mitral currents possess small velocities and momenta, and require every favorable condition possible to secure their reception into their respective ventricles during the period of diastole.

Again, whatever may be the forms of these orifices as observed post mortem, when the heart is lax and empty, there must be, I believe, every probability that volumes of blood which transverse them *during life*, and exert centrifugal pressure on their circumferences, will inevitably tend to throw the *effective* openings into a circular shape. That such is the case is well seen in Dr. Pettigrew's specimen, marked 929 B, in the museum of the Royal College of Surgeons. The chambers of the heart in this specimen have been filled with fluid plaster-of-Paris, which, in hardening, has preserved the circular form of the orifices. Specimen 928 C in the same museum clearly exhibits the naturally circular openings of the tricuspid and mitral orifices in the heart of the Bison Europæus. The walls of the apertures are so thick and firm that they preserve their circular shape post mortem.

If we stuff a sheep's or bullock's heart with cotton and boil it for many hours, and detach—as we can most readily—the auricles from the ventricles, we shall find in this rude experiment the apertures to be very nearly circular in shape.

Lastly, in employing the spherical balls devised by Dr. Peacock for the measurement of the orifices, the apertures are found to be well fitted by them.

These considerations will, I think, be sufficient to establish the truth of my statement that the tricuspid and mitral orifices when in function are *circular* in form.

2nd. I will now proceed to the reasons which show that the areas of the orifices are constant—that they do not alter in superficies during the systole or diastole of the ventricles.

(1) The rings which surround the auriculo-ventricular openings are formed of white fibro-tendinous tissue. Being naturally inelastic, they are consequently unaltered in their perimeter by any normal eccentric pressure to which they are exposed by the passage through them of the auriculo-ventricular volumes of blood. In my former paper on this subject I have shown that the force with which these in-coming volumes enter the ventricles is small; and it is evident, therefore, why the tendinous boundaries of the auriculo-ventricular openings are feebly developed.

(2) The muscular fibres around these orifices are in no way found to be arranged in a circular form, and cannot possibly, therefore, act in the manner of a sphincter to close the openings. The fibres which constitute the walls of the ventricles are formed of seven strata, three of which are external and oblique, one central and transverse, and three internal and oblique. The first and seventh strata are alone inserted into the auriculo-ventricular rings, and converge in a direction almost at right angles to their planes, while the inner and subjacent strata have no connexion with or insertion into them, but are continuous beneath them. The masterly dissections of Dr. Pettigrew establish the truth of this description. It is clear, therefore, from this arrangement of the fibres, that the systolic contraction of the ventricles can have neither power nor influence to diminish the superficial areas of the tricuspid and mitral apertures.

(3) The muscular walls of the auricles, according to an able paper by Mr. Savory, of St. Bartholomew's Hospital, are found to terminate by two attachments. The larger and outer portion (the looped fibres of Quain and Sharpey) is closely inserted into the fibrous auriculo-ventricular ring, while the inner and thinner portion can be traced between the surfaces of the valves, terminating more or less abruptly by attachment to the tendinous tissue. The former offers no arrangement of fibres capable of contracting or in any way altering the capacity of the inlets, and the latter fibres are probably of use, as suggested by Valentin, to assist in raising the tricuspid and mitral curtains in ventricular diastole, in order that the blood may freely pass behind them and close them by recoil pressure.

It follows, therefore, that neither on the ventricular nor the auricular side is there any muscular arrangement existing capable of affecting the areas of the auriculo-ventricular apertures. In fact, the existence of valvular curtains would, I think, lead one, *a priori*, to expect that they alone are requisite and sufficient to close the apertures. *Where sphincter muscles are found, valves are absent.* We find no valves at the pylorus, anus, and orifice of the bladder in man, and the right auriculo-ventricular orifice in birds, which is *entirely* muscular in structure, exhibits no trace of a curtain.

I propose at some future time to enter into the consideration of the probable reasons why the heart of the bird differs in the shape of its right auriculo-ventricular orifice from that of the mammal. If the tricuspid and mitral orifices could be contracted by muscular action and closed, as the neck of the bladder is, by a sphincter, no valvular curtains would be required. The two forms of arrangement for closure are incompatible with one another.

Again, looking at the result, it is manifest that auricular action, if capable of diminishing the area of the orifice, would only tend to impede the flow of the blood from the auricle to the ventricle, and thereby oppose the attainment of the very object which it has to accomplish. And it is equally clear that any contraction of the area of the orifice by ventricular systole would not assist in the expulsion of the ventricular contents, inasmuch as the curtains are *inelastic*, and cannot diminish in superficial area under any effort of the papillary muscles, which regulate their position and tension. The obliteration of the ventricular chamber is effected only by the contraction of its muscular walls, during which process the auriculo-ventricular curtains are drawn down by the papillary muscles in the shape of a cone, which in its gradual descent becomes filled with blood from the auricle. The pressure *on* the ventricular surface of the cone continues, of course, much greater than the pressure of the blood contained *in* the cone, until the end of the systole, when it becomes zero, and allows the constituent portions of the cone to separate from each other under the pressure of its fluid contents; and thus is the onward current of blood favored from one chamber to the other, and a *sudden shock* to the flow into the auricle prevented during the contraction of the ventricle. The *persistent* opening of the auriculo-ventricular orifice during the descent of the valve, so well described by Pettigrew, forms evidently an important part of the arrangement.

There is therefore, I think, great reason for believing that the areas of the tricuspid and mitral openings are as unaffected in systole and diastole as are the areas of the aortic and pulmonic apertures. The rings which surround the latter are very strong, and capable of resisting the pressure of the volumes of blood which pass through them in systole; and unless the orifices maintained their constant areas, their valves, which are *inextensible*, would be unable at all times to adapt themselves to the openings which they have rigorously to close. The same line of argument is applicable to the auriculo-ventricular apertures and their curtains. The respective areas of these openings must be constant, in order that the inextensible valves belonging to them may be able to adapt themselves at all times to them.

As an additional illustration in proof of the constancy of the areas I may refer to some recent experiments made by Drs. Sibson and Broadbent on the living dog. In fasc. vii. of the "Medical Anatomy" Dr. Sibson says:—"When I observed the heart acting vigorously under water, after being cut out, it seemed to me that the circumference of the shut valve (mitral) did not lessen with the diminution of the ventricle towards the end of systole. It would appear that the pressure of the blood, by filling the sacculi on the under-surface, unfurls, flattens out, and enlarges the valve, so as to maintain it almost of full size to the end of systole."

If, then, it be true that all the chief orifices of the heart are circular in form when in action, and that their respective areas are constant during systole and diastole, certain objections which have been raised to the truth of the statements made in my former paper are removed, and the correctness of the formula which I have discovered—namely,

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}} = \frac{\text{Area of pulmonic}}{\text{Area of aortic}}$$

becomes more fully established.

And if it be also true, as it undoubtedly is, that equal volumes of blood must pass synchronously through the pulmonic and aortic openings, and that the two ventricles exert very unequal pressures during systole upon these equal volumes of blood, it must follow, if the velocities are unequal, that equal *times* of efflux can be only obtained by the areas of the aortic and pulmonic orifices being inversely as the velocities of the streams which traverse them. During the period of each systole the left ventricle has not only to sustain and neutralise the pressure of the aortic column, but also to send forth nearly five cubic inches of blood into that vessel. The latter is by far the smallest portion of its task, as shown in an able paper written by Dr. Andrew Buchanan, of the University of Glasgow, upon the "Effective Force of the Heart"; but the principle—the formula—for the truth of which I am contending (the result of the correlation of the forces of the two hearts), does not involve the objects for which the forces of the two ventricles are put forth. It is sufficient to know that the velocities are unequal—that they result from the operation of unequal forces which act in equal and the same times upon equal masses of blood traversing openings of confessedly unequal areas—to be convinced that these areas must be inversely as the velocities of the streams which pass through them. An American reviewer of my paper clearly puts it:—"Dr. Davies's observations relate only to the relation between the *size of the orifices* and to the average velocity of the blood through these orifices, and he deals with *vis viva* only on attempting, as it were, to confirm the latter."*

M. Onimus, in the *Journal de l'Anatomie*, 1865, has attempted to show that the auriculo-ventricular openings are completely closed in systole by muscular (sphincter) action, but the investigations of more recent anatomists completely negative the existence of such an arrangement of fibres; and with respect to one of these openings he himself says:—"Si pour l'orifice auriculo-ventriculaire on peut admettre des fibres circulaires formant des sphincters, pour l'orifice gauche cette expression n'est pas très juste, car cette dénomination suppose un muscle annulaire, et nous avons vu qu'une partie de cet orifice était complètement fibreux." In fact, the dissection of a heart which has been boiled for some hours will readily show that the fibres at the auriculo-ventricular orifice have an almost vertical direction, and that their action would tend rather to keep the openings dilated than closed during the ventricular systole. I cannot believe also that any trust can be placed upon the very rough experiment described by M. Onimus of passing a finger through a wound made in the wall of the auricle of a living animal so that it may reach and be pushed into the ventricle through the auriculo-ventricular orifice. Fatal and sudden syncope would undoubtedly result and interfere with the experiment.

Finsbury-square, June, 1872.

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NOTES

ON THE

GENERAL PRINCIPLES OF CUTANEOUS THERAPEUTICS.

By Tilbury Fox, M.D., F.R.C.P.,

Physician to the Skin Department of University College Hospital.

(Continued from August Number—p. 417.)

I now turn to the general principles of local treatment. The diseases to which I am now referring are, it will be remembered, the hyperæmias, the simple inflammations in the debilitated, eczema, ecthyma, psoriasis, acne, pemphigus, &c. I am the advocate for a much more soothing system of treatment than that usually adopted for these diseases, of which hyperæmia is so frequently a part. And this leads to the question, What should be the object of our local measures? It is threefold; (1) at the outset to *moderate* diseased, especially inflammatory, action; (2) to *protect* the diseased, and therefore weakened, parts; and at length (3) to *stimulate*, with the view of rousing the dormant tissues into due activity, and causing the removal of morbid depositions and formations. My own conviction is that, in the early stages of hyperæmic skin diseases, much of our current treatment is mischievous by reason of its activity. A soothing plan of treatment is wanted in all cases of early cutaneous congestions. By soothing treatment I mean one which diminishes congestion and secures an exclusion of air—one which, in fact, puts the skin in a state of rest. *Heroic* measures, designed to cut short an early congestive stage of a skin disease, often render the course of that disease chronic and persistent. In the early stages, before the deep vessels are involved, much can be done to check congestion by mild applications; whilst active measures do harm. For instance, in acute general psoriasis I have often seen aggravation of the congestion follow the use of tarry applications; while great relief has been produced by alkaline and bran baths, and subsequent oiling of the surface. I am not by any means an advocate of an expectant plan of treatment. I advise potent remedies to be employed after the congestive stages have passed. The dangers of over-stimulating are not imaginative; and they are plainly made apparent, in cases accompanied by active hyperæmia, by much irritability, or by a tendency to degenerative change in the skin, in the spread of the disease, and its undue chronicity.

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* The American Journal of the Medical Sciences, Oct. 1870, p. 503.

I may refer to the local treatment of certain instances of lupus, *quoad* hyperæmia, in illustration of what I mean. Sometimes we have a patch of lupus on the face, which is very tender, very hyperæmic, and which exhibits a tendency to spread —that is to say, there is a tendency in the apparently healthy tissues to become the seat of the cell-growth which is the characteristic of lupus. In these cases the exclusion of air and the use of mild non-irritating astringents will do good by diminishing the hyperæmia; whilst the use of caustics will be sure to cause the disease to spread, because it will greatly increase the hyperæmia of the parts around, and so favor the development of the peculiar cell-growth. In fact, if one still further disturbs the already weakened disease-tending tissue, one is, of course, likely to favor disease. This is also true of simple acne, pityriasis rubra, &c.

Now for the details of local treatment.

First, as regards *moderating* inflammatory action. It will naturally occur to anyone first of all to remove special causes of local irritation in the case of hyperæmic skin affections. There are two sources of mischief I may specially refer to; they are (*a*) the wearing of flannel next the irritable skin, and (*b*) scratching. I think flannel should never come in contact with an irritable skin; it is a great source of irritation. It may be worn outside a linen garment, and the patient will not, under such circumstances, catch cold. The disuse of flannel is important in nettlerash, pruritus, eczema, scabies, the erythemata, &c. Scratching does an infinity of mischief. The usual plan for preventing it is to use some sedative lotion. I think it of great importance to employ emollient and alkaline baths freely, to allay irritation in the early stages of local inflammatory cutaneous diseases, and to subsequently protect the parts by appropriate coverings and applications; and by internal remedies remove all cause of pyrexial disturbance, or alter such blood impurifications as lead to an intensification of the hyperæmia of the parts through which the blood passes. The baths which are best adapted to moderate inflammatory or irritative action in the skin are bicarbonate of soda, 2 to 4 ounces, size from 4 to 6 or 8 lb., poppy, and bran. The patient may remain in for ten minutes or so, and the skin should not be rubbed dry, but patted with hot towels. It is a good plan to oil the skin subsequently, or to powder it with oxide of zinc, or to apply a simple calamine lotion. But we must take care to use such means in due conjunction with general remedies. If bile products or ureal compounds are in abundance in the blood, free purgation, or diuretics employed with a liberal hand, must not be neglected. Pyrexia must be met with appropriate drugs and dieting. In like manner the pain and hyperæmia of an eczema in a gouty subject may be greatly moderated by an alkaline bath, but a good dose of colchicum in addition will bring the greatest relief. The exclusion of air from inflamed and hyperæmic irritable parts is a matter of great consequence, and the more so if the irritable part is denuded of cuticle. The air is very stimulating to such parts; absorbent powders and neutral unguents are here called for. Then it is also very desirable to prevent evaporation taking place from scratched surfaces, or surfaces denuded of their natural protecting layer of healthy cuticle, for thereby they become harsh, hard, and tend to crack. I know nothing so good for the purpose as Kirkland's neutral cerate.

I don't know that I can find a better place to make one reference to the use of water-dressing to indurated and irritable parts, made all the more troublesome by scratching. There can be no doubt that the use of a water pad, which permits the diseased surface to absorb moisture and so become more or less soft and supple, does give great relief; but this is oftentimes succeeded by the recurrence of itching, pain, and, it may be, increased cracking and stiffness or tension. The explanation is not difficult to give in regard to some cases at least. The part has become denuded of its cuticular covering, and, whilst it absorbs moisture freely, it also parts with its moisture equally freely unless prevented. Whenever I use water-dressing to an excoriated part, I invariably direct that a layer of greasy matter shall be applied, and kept applied, on the removal of the compress. In this way the absorbed moisture is retained, and the parts keep more or less soft.

But I am touching upon the second point—viz., the *protection* of hyperæmic and irritable parts; and, indeed, to *protect* is, under such circumstances, to *moderate* diseased action. To protect is to prevent scratching and the stimulation by the external air that gains access to the part and to secure rest, as it were, to the diseased parts; and to negative the operation of special external irritants. In addition to the use of absorbent powders and neutral unguents, one may employ cotton-wool, bandages, and strappings, which I need not now discuss at any length. The use of bandaging and strapping is particularly to be commended where the effects of gravitation are noticeable—in the legs, for example. The bandage prevents undue distension of the vessels and the escape of serosity into the textures, thereby giving the parts a better chance of recovering their tonicity. This simple matter is much neglected in regard to cutaneous diseases. Not only in the leg, however, but in other parts bandaging and strapping act similarly, where the tendency to engorgement of the tissues and vessels is shown.

The third object of local treatment in the diseases with which we are dealing is *stimulation*. One of the chief things the dermatologist should set himself to determine is the exact time at which his soothing kind of treatment should cease and his stimulating remedies be employed. It is at the moment that the irritability of the bloodvessels and the nerves is subsiding that the use of powerful astringents and stimulants is so efficacious, and that it is possible to do the most towards a speedy cure of disease. What happens in the inflammatory eczema, the acne, or the psoriasis? After a while the vessels lose their tonicity and become dilated, favoring the "effusion of inflammatory products," giving rise to œdema, "chronic inflammatory thickening," &c. Now, it is at the moment that the parts are becoming less inflamed and irritable that astringents, lead, zinc, nitrate of silver, mild mercurial preparations, and the like, are of use to restore to the vessels their tone; and if at the same time our general remedies assist nature by removing impurities from the blood, by restoring the balance between absorption and excretion especially of the watery matter of the system, and giving general tone to the body, the patient will probably get rapidly well. The application of sulphur at the nick of time indicated speedily cures an acne that had been irritated before by the same remedy. The same may be said of the use of nitrate of silver to an eczematous patch. The use of tar to a psoriasis which is markedly hyperæmic at a very early stage will often spread the disease, or bring out fresh places. If matters still do not mend, but the disease holds on, one needs to stimulate, or, as we say, "rouse the torpid tissues to activity," but as I should put it, to quicken the activity of the absorbents in order that effused and formed products may be removed. It is really not so much in chronic inflammatory skin diseases that we need fresh remedies, as to use those we have, reasonably, as regards time and circumstances. This is emphatically so as regards eczema and general psoriasis. But what are the indications that local soothing remedies are still needed in a disease, and the time has not come for stimulants? I think the main ones are as follows:—The tendency of the hyperæmia or the disease to spread; the development of new spots of disease, showing the disposition to the implication of healthy parts, which would be favored in its development by anything which irritated such parts; the presence of much pain or heat in the parts; and manifestly the marked hyperæmia. But I lay most stress on the two former features. I like to let hyperæmic conditions in skin disease "quiesce," as it were, before I leave off my soothing treatment. But a disease having "quiesced," its hyperæmic condition having diminished, and the disease showing no tendency to spread or to spring up in new places—e.g., acne, eczema, psoriasis, and pemphigus,—then stimulation can be appropriately adopted. And for what reason are stimulants used? First, to restore the tonicity of vessels; and one must not forget here that artificial pressure by bandages, &c., is a great assistance where gravitation comes into play to distend the vessels, as about the legs. Secondly, to alter the character of a discharging surface. Such is the action of a weak mercurial ointment in a case of chronic eczema. Thirdly, to check the formation of scales, as in the use, against psoriasis, of tarry preparations; and, fourthly, to cause the absorption of inflammatory or heterologous formations; the iodides, mercurials, the soap treatment, blistering, and the like are specially referred to under this last head. But in regard to the use of stimulants and revulsives, I hold equally, as concerns their application, that their efficacy will depend greatly upon the judicious conjunction of internal remedies. I mean that, supposing that the blood of a patient in whom we use these external means is charged with uric acid, he will have a much smaller chance of gaining benefit from the application of revulsives than one who has a pure blood-supply passing through the diseased part. In the former the blood-state may take occasion, by the unbalancing of the nutrition induced by the use of the local remedies, to increase the hyperæmia, or to set agoing an inflammatory action again. The success of stimulant applications, in fact, will be in direct proportion to the degree in which the individual's condition approximates that of health. One very potent method of stimulating not only the skin but the system generally, and emunctory organs in particular, is the use of sulphur or sulphide of potassium baths. They should not be employed in the acute stages, but in the chronic forms of psoriasis, eczema, lichen, and acne. Where there is not much irritation or hyperæmia, and where the latter does not seem to be readily increased, they are most useful.

(To be concluded.)

DESCRIPTION

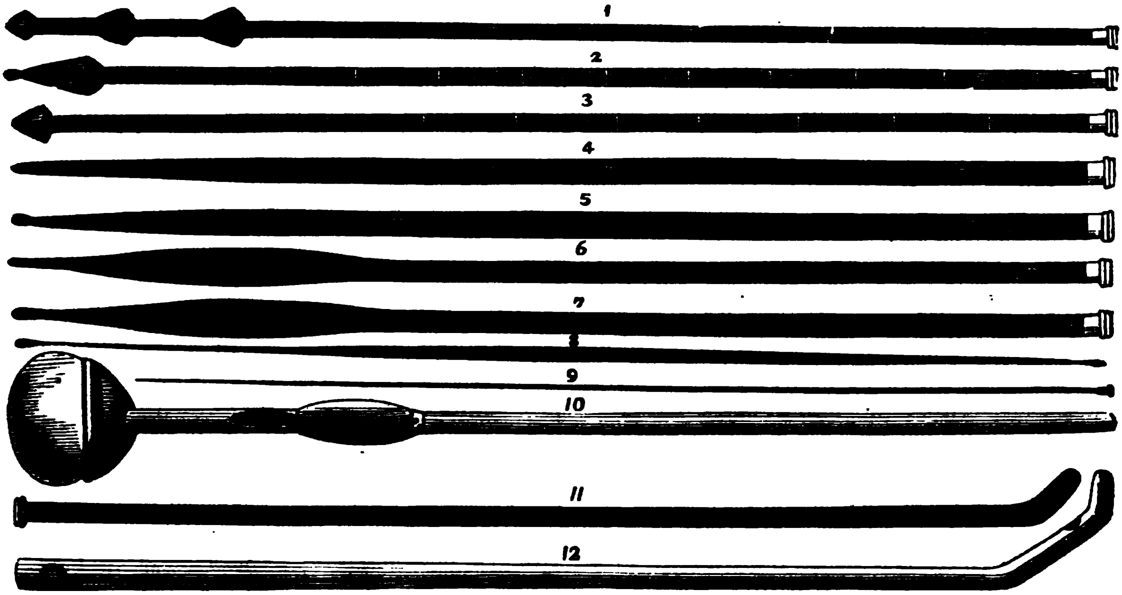
OF SOME

SURGICAL INSTRUMENTS NOT COMMONLY KNOWN IN ENGLAND.

By W. F. Teevan, B.A., F.R.C.S.,

Surgeon to the West London and St. Peter's Hospitals, etc.

Without some of the instruments I am about to describe no surgeon can diagnose and successfully treat many of the affections of the genito-urinary organs. Two of the instruments depicted have within the past few years found their way into English practice, and I have no doubt the others will before long become as familiar. I would premise my observations by saying that, except in certain particular cases, the employment of metal instruments is to be deprecated as causing more annoyance to the patient and far greater local irritation than soft ones. The saying of M. Mercier ought always to be remembered, "La sonde rigide ne cede pas; c'est l'obstacle qui cede devant elle." The use of a stiff, unyielding instrument in a passage often tortuous and rugged can only be accounted for by an ignorance of those pathological alterations which take place in stricture of the urethra. It is hardly fair to a patient for a surgeon to commence the practice of catheterism in the most sensitive tract in the body without previously ascertaining by a trial on his own person the comparative merits of soft and metal instruments, and I hope that before long the latter will be rarely seen. There are, however, some surgeons who will not be baffled in the passage of an instrument, and hence these gentleman will always remain the staunch advocates of an order of things which, happily for humanity, is passing away. Armed with a pointed silver catheter no surgeon need be foiled in attempting to get into the bladder; it is merely a question of force, more or less—no flesh can withstand its bloody onslaught. In one of the museums of London there is a very painful reminiscence of a celebrated surgeon, in the shape of a preparation exhibiting a metal instrument *in situ*, which he had managed to pass into the bladder *without traversing the prostatic urethra.* But I have heard it stated that some patients actually prefer metal instruments. Unfortunately, however, such a statement only shows that in a particular case the surgeon had, through an imperfect diagnosis, used an inappropriate instrument; for if a patient with enlarged prostate and stricture—a common complication—be treated for the latter complaint with an olivary soft bougie he will certainly be subjected to considerable annoyance, for the end of the instrument will impinge against the enlarged lobe of the prostate, and can only be carried over the obstacle by sheer force, whereas a curved metallic bougie will only cause such inconvenience as is inseparable from the material of its construction. The progress of soft French instruments in England has been much retarded by the inferiority of those made here, for most of the catheters soon crack across the eye, and as a rule the olive is much too large, and the neck not being proportionately thick, the bougie gets bent at right angles at its neck, much to the patient's annoyance and the surgeon's fear, for I find there is a wholesome dread of the separation of the olive from its shaft. However with the Parisian instruments such an accident is impossible, and not only are they cheaper, but they are infinitely more durable. I consider a French bougie lives ten times longer than its English representative, which generally ends its short career by breaking its neck. For the introduction of elastic instruments into surgical practice we are indebted to a French goldsmith, named Bernard, who brought them before the French Academy in 1779.



With the exception of Nos. 10 and 12 all the instruments I am about to describe are elastic.

Fig. 1 illustrates the "bougie à trois nœuds." The cone at the end is succeeded by others, each larger than its predecessor. The bougie may be used for dilating strictures or applying ointment to any part of the urethra. It is well known to those who used the bougie à boule that although at first the cone may only be able to get through a stricture, yet, if the instrument be moved to and fro a few times, the obstruction will no longer be felt, owing to the dilatation exercised; and it is the knowledge of this fact which enables us to dilate a stricture considerably at one sitting, if it be thought desirable, by the use of the "bougie à trois nœuds," for each cone, being larger than its predecessor, more and more dilates the contracted part. The instrument is peculiarly adapted to ring strictures, and I would say to them only.

Fig. 2 is the "bougie à boule" as altered by Dr. Mallez; and Fig. 3 is Dr. Henry Dick's modified "bougie à boule." The latter was described by me at p. 389 in The Lancet for 1870, and I simply introduce it here for comparison. Dr. Mallez claims for his instruments an easier introduction, on account of the tapering olive he has added to the bougie; but inasmuch as the ball-staff is for diagnosis only, I prefer Dr. Henry Dick's, as the greater sharpness of the base of its cone permits an increased perception in diagnosis of pathological alterations. These bougies are made of various sizes, but the most useful are those varying in size from a No. 6 to No. 10, English measurement, which enable us to detect incipient contractions long before they can be ascertained by any other instrument. For a knowledge of the value of these bougies I would refer the reader to an article by me in the *British and Foreign Medico-Chirurgical Review* for July, 1867.

Fig. 4 represents the "bougie conique." At p. 455, vol. v., of the Dict., de Méd., M. Desormeaux in the following words states the value of these conical bougies: "Ces bougies sont sans contredit, les plus utiles dans le traitement des rétrécissements dans lesquels elles s'engagent et qu'elles dilatent mieux qu'aucune autre." I find from my own experience that these bougies are most useful in boring through, as it were, tight, narrow strictures, and enabling the case to be completed with the olivary bougies.

Fig. 5 illustrates the "bougie olivaire." This bougie is applicable to the treatment of the majority of cases of stricture which will admit a No. 4 or 5 English. Its great value is dependent on its olive, which prevents the point from lodging in the lacunæ of the urethra. It is also the only instrument which ought to be placed in a patient's hands; he cannot do any harm with it, the formation of a false passage being an impossibility even if the patient tried. As will be seen by the illustration, this bougie is the conical bougie *plus* the olive placed at the extremity. It was invented by Lioutt at the commencement of the present century, and obtained a prize at the Academy in Paris.

Figs. 6 and 7 represent respectively the "bougie conique à ventre" and "bougie olivaire à ventre." When we have dilated a stricture up to a large size, say No. 12 English, the patient often complains of the unpleasant distention of the meatus externus, which has once more become, as it ought to be, the narrowest part of the entire canal. Now, inasmuch as the shaft of these bougies is much smaller than the other part, we shall, if we use them, only subject the patient to an annoyance lasting for one second, instead of the prolonged inconvenience resulting from having a full-sized ordinary bougie in the passage for several minutes. The instrument dilates the stricture only.

Fig. 8 is a representation of the olivary whalebone bougie. Each end is similar to the other, consequently it will last as long as two bougies not similarly constructed. All bougies might be made double, as it were, by uniting them at the end of the shafts. These bougies can be obtained with ends smaller than a No. ½ silver catheter, and they are of inestimable value, for they combine all the advantages of elastic instruments *plus* the special merit of stiff ones in those particular cases where the perineum is one solid mass of induration; the olive at the end prevents the formation of a false passage, which is quite possible if a tapering-pointed whalebone bougie be used. In cases of traumatic stricture these instruments are invaluable, as they are usually the only ones that can be got through.

Fig. 9 is the elastic filiform bougie, and is the smallest known; its point is necessarily conical, as its extreme tenuity will not admit an olive to be placed on it. Some idea of its size may be gained when I state that a No. ½ silver catheter is equal to about six of these bougies put together. Not only is it most useful for treating all those cases of stricture which are absolutely impassable to any English instrument, but it will also obviate recourse to an operation in cases of retention of urine from very bad strictures, for if the bougie be passed and left in for a few minutes, urine will be found to flow on its withdrawal. The surgeon who has had no experience of this instrument will very naturally suppose that it will double up on meeting with an obstacle; but such will not happen if he takes his fingers off the bougie at every half inch of progress made, for if it cannot proceed further, it will spring back, thus giving the surgeon notice of its arrest. It must be wriggled in, rather than introduced in the ordinary manner.

Fig. 10 is Dr. Mallez's instrument for injecting powders into the urethra, as modified by me. I have made it a breech-loader, and so simplified its action. The instrument is useful for injecting various astringent powders into the anterior part of the urethra in cases of chronic urethritis.

I now come to two instruments which no surgeon ought to be without—M. Auguste Mercier's "sonde coudée" and "sonde bicoudée." The beaked catheter No. 11 resembles a lithotrite so far as its beak is concerned, which is placed at much the same angle to its shaft. This instrument is destined entirely to supplant the English metal prostatic catheter, and also the elastic catheter within a stylet; it never draws blood, and can be used either by the surgeon or patient in cases of retention of urine from enlargement of the third lobe of the prostate. But cases of difficulty often arise either from the extreme obesity of the patient or from the great extension of the floor of the prostatic urethra, and no ordinary prostatic instrument can be passed; for such cases M. Mercier's "sonde bicoudée" (Fig. 12) is invaluable. I believe it is practically unknown in this country.

Portman-square, June 29th, 1872.

REMARKS ON

VERSION AND UTERINE TETANUS.

WITH CASES.

By Alexander Milne, M.D.,

Corresponding Member of the Gynæcological Society of Boston.

In a communication of mine to The Lancet, January, 1871, I alluded to some points which I deemed of essential importance in the operation of turning—in fact, indispensable pre requisites to success in the more difficult cases; and as my subsequent experience, which has been considerable, has served to strengthen and confirm the views I then advanced, I am desirous of again referring to the subject, while reporting a few illustrative cases.

The operation of podalic version, where the pelvis is of average size, the os uteri dilated, and the membranes entire, is one of easy performance; but where the liquor amnii is absent or has long escaped, where the parts have become swollen and dry, and the uterus has contracted firmly around the fœtus, we have the conditions of difficulty and even of danger. The difficulty is greatly enhanced if the contraction of the uterus happens to be tonic or tetanic, a condition that not very rarely obtains. I have seen not a few such cases, and where the contained body was grasped so firmly that extrication appeared a hopeless task, the idea of version almost utopian, and extraction by craniotomy or spondylotomy the only hope—a sorrowful one indeed! This tetanus of the uterus I have observed ocasionally to mark the entire organ; at other times, and oftener, it appeared to be partial, a common place for it being the internal os, less frequently the external os. Sometimes it consists of narrow bands in some part of the cervix, having all the rigidity of a ring of iron. Whether partial or general, it grasps the fœtus with immense power, and, uncombated, renders turning utterly impracticable. I ought also to remark that, though more common in the absence of the liquor amnii, it yet occasionally exists prior to the breaking of the membranes, and indeed is a cause of their premature rupture.

Now, when we meet with a case of this kind, where the uterine contraction is continuous or tetanic, intense, extreme, and grasping the child as in a vice, what ought to be done? Chloroform, of course, must be administed, and very freely too; and often it relaxes the spasm in a wonderful manner, but I have seen it fail frequently in uterine tetanus—nay, it usually fails in such cases. There was an impressive case, in illustration of this, reported to the Edinburgh Obstetrical Society in

November, 1865, by Dr. Inglis, and where the late Sir James Simpson had to perform spondylotomy. "The patient," says Dr. Inglis in his report, "had been kept deeply under the influence of chloroform for two hours, yet the uterus remained nearly as firmly moulded to the body of the fœtus as before." Chloroform then being inadequate to the relaxation of this spasm, have we any other means or appliances of a more powerful kind? We have, and they are tartarated antimony and lobelia inflata. When the chloroform is ineffectual, recourse should be had to these. Give a grain of the antimony, and twenty-five minims of the tincture of lobelia inflata; afterwards, on the lapse of twenty minutes, place the patient under the influence of chloroform. The triad form a potent antispasmodic whose conjoint action is almost irresistible, and the spasm is overcome. The uterus relaxed, the hand now finds entrance, and version becomes a possible thing, I am convinced that reducing instruments may often by these means be stayed.

Hand to be employed.—Now, what hand ought we to employ in these extreme cases? This is a deeply important question, and one whose gravity we can scarcely overrate. A somewhat uncertain sound has been given forth on this question; some recommending one hand, some another, while others have ignored the thing altogether. The majority recommend the right hand, which (I say it advisedly) is the wrong one. (I must except Meigs, Lee, and Ramsbotham, who are orthodox on this matter, and have risen to the height and gravity of it.) The left hand, in the great run of cases, is the proper one to employ; and I hereby deliberately impeach the right one with many failures; with deplorable craniotomies and spondylotomies, not to speak of maternal injury. Almost any hand will succeed where it can float in abundant waters, and where there is a roomy pelvis; but in the more difficult cases, where space is scant and the parts are tumefied, commend me to the left one. It is the proper one in all dorso-anterior cases; and we shall succeed better with it even in dorso-posterior ones where there is fluid and space. The grounds for preferring the left hand I stated in THE LANCET, for January, 1871, and have noticed them also in my work on Midwifery; I need not, therefore, take up space by reiterating them. I would only again repeat as forcibly as I can, Cultivate the left hand in version if you wish to avoid failure—nay, if you wish to conserve life. I have been baffled with the right hand; but I am happy and proud to say that I have *never* failed to turn with the left.

Shall we seize one leg or both?—There is only another point in connexion with this subject to which I would shortly refer, and that is in regard to the leg or legs to be grasped. In those difficult cases, to speak paradoxically, we can neither afford to seize both nor to want both—that is to say, there is little room to enable us to get hold of both, but it is better to have them if at all possible. The fœtus will then revolve more readily, and come down more speedily—nay, neither evolution nor descent may be accomplished if only one extremity be seized, as was seen in Case 6. Of course I allude to the extreme cases. More especially is it of importance to attend to this where the head is the presenting part. M. La Chapelle called attention prominently to this, and my experience of version warrants me in placing considerable emphasis upon it.

I now subjoin some brief notes of cases selected from many in which I have been engaged.

CASE 1.—June, 1869. Mrs. S—— (multipara). In labour for about twenty-four hours when Dr. Millar was called. Dr. Millar sought my aid; and on my arrival I found an arm hanging out of the vagina. The limb was swollen and discolored; and I was informed that a woman had "made a pull or two" at it before the arrival of Dr. Millar. The os uteri was strongly contracted around the shoulder, and I ascertained by external manipulation that the entire uterus was in a state of persistent contraction. The liquor amnii had escaped the day before. I gave a grain of tartrate of antimony; and afterwards Dr. Millar placed the patient well under chloroform. I then introduced my left hand, and reached the feet in about four minutes. They were brought down without much difficulty, the uterus having relaxed greatly. Delivery was soon accomplished. The child cried in about forty minutes, and survived, the arm remaining sore for about a fortnight. The mother recovered well.

CASE 2.—July, 1869. Mrs. L—— (multipara). Under the care of a student. Had a note from Dr. Cairns (to whom the student applied) to attend as he was ill. I found the woman had been in labour for about fourteen hours. The liquor amnii several hours away; arm and loop of cord protruding externally; feeble pulsation in funis; the os rather rigid, and the whole uterus contracting moderately. Placed the woman under the influence of chloroform, turned with the left hand, and delivered pretty easily, the uterus having relaxed well. The child cried speedily, and survived. The mother got on well.

Case 3.—Dec., 1870. Mrs. M——, (multipara). Had a message from Dr. Cairns to see this patient. Found an arm, swollen and livid, projecting from the external parts. It had been down for some hours; uterus contracting with immense power around the child; woman somewhat exhausted. Dr. Cairns placed her under the influence of the anæsthetic, and I introduced my left hand gradually until I hooked a leg, which I brought down; found the fœtus did not revolve. Passed hand up again and caught the other foot; turned readily and delivered, the head passing the upper strait with difficulty. Child dead; mother recovered.

CASE 4.—Nov., 1870. Mrs. ——, (patient of Dr. Furley, who asked my help). Shoulder and loop of cord presenting. Bag entire, but firm broad band of strongly contracting fibres about internal os; and external os rather rigid. Dr. Furley chloroformed patient, and I introduced my left hand and turned easily. Delivery was speedy, and child and mother got on well.

CASE 5.—Feb., 1871. Mrs. R—— (primipara). Patient had been in strong labour for twenty-four hours, attended by a female. At length Mr. Moir was sent for. I was called and found an arm presenting immensely swollen and dark colored. It had been down for upwards of twelve hours. Uterus contracting strongly and persistently, the regular pains having almost ceased. Woman complained of some tenderness over abdomen, but her strength and pulse were fair. Gave tartrate of antimony and Indian tobacco, and afterwards chloroformed patient. Introduced left hand and seized both feet in about seven minutes; turned with little difficulty. Child died; mother recovered in three weeks. There was no peritonitis, but she remained weak from the profuseness of the after-discharge.

Case 6.—May, 1872. Mrs. —— (multipara). This was an arm-presentation, under the care of Dr. Miller. The doctor brought down a foot skilfully, but the fœtus would not revolve, owing partly to the strong contraction of the uterus, but mostly to part of the head having come below the brim, and partly also, in my estimation, to only one leg having been seized. The doctor sought my assistance, and on arrival I found the following to be the state of matters. Leg and head jammed in upper strait; uterus contracting powerfully and continuously. Patient's strength fair. Dr. Miller chloroformed thoroughly, and I seized the leg presenting with four fingers of the left hand, pushing the head up with the thumb of the same. Failed to evolve. Proceeded with the same hand in quest of other foot; brought it down, and pushed up the head with thumb first, and externally with the right hand. Revolution speedily effected. Delivery was soon over, but the head passed the brim with difficulty. The child was quite dead, but the mother got on well.

I shall not comment on the foregoing cases beyond remarking that the unvarying success, so far as maternal safety and recovery are concerned, may be greatly credited to chloroform, and partly to the use of the *left hand*. In every case the shock of the operation was abolished, and thus an element inimical to recovery excluded. It is doubtful if in pre-anæsthetic days some of these cases could have been undertaken at all; or, if so, whether rupture of the uterus might not have been occasioned in one or more of them. There was antimony, opium, tobacco, and the lancet, it is true; but these relaxed with less efficiency and speed, and abrogated suffering and shock with less power than the great anæsthetic of modern times. Happy, then, is the obstetrician of the latter half of the nineteenth century; and ungrateful must he be viewed if he does not often think of Simpson, while heaving a sigh that he went home so soon.

Edinburgh, June, 1872.

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TWO NEW FORMS OF ENEMA APPARATUS.

By W. L. Shepard, M.R.C.S., &c.,

Late Tutor to St. Bartholomew's Hospital.

Most of the enema apparatus hitherto invented present some disadvantages which militate against their usefulness. The ordinary brass enema apparatus is cumbersome and extremely inconvenient to use alone, as it needs both hands to work it, so that it is difficult to keep the canula *in situ*. Again, if not often used, the packing on the piston is sure to get very dry, and, in hot climates especially, the instrument is in this way often rendered useless, it being no easy matter to pack a piston properly.

The irrigateur of Dr. Eguisier is easy to manipulate, but it is very bulky, very liable to get out of order, and could only be repaired by an instrument-maker. Perhaps the best form of apparatus yet made is either the Higgenson or Kennedy. They are both of them portable, and can be used with one hand, but they have also both of them one radical defect—namely, the vulcanised india-rubber ball. However good this may be when first manufactured, after a time it undergoes some molecular change and becomes worthless. Again, if the ball be made thin it fills but slowly, and if made thick it is very tiring to the hand; but more than all, oil or turpentine (so often used in enemata) soon destroys the india-rubber.

Fig. 1.—The Simplex.

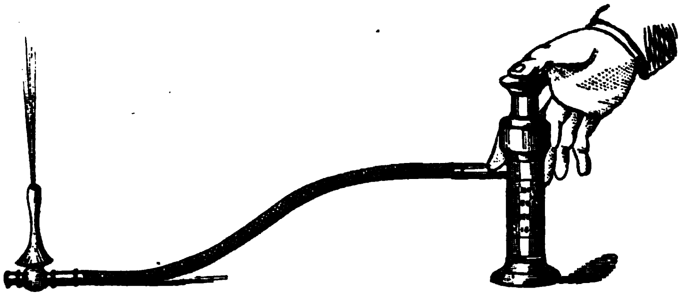
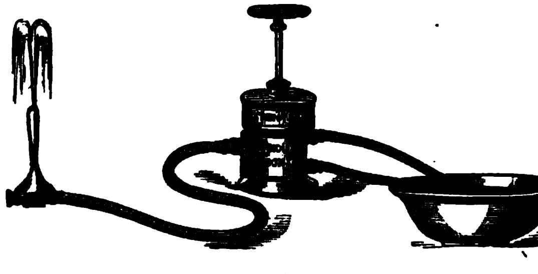


Fig. 2.—The Facilis.



I have endeavored to remedy all these various defects in the two forms of enema apparatus I have had made for me by Messrs. Arnold, of West Smithfield, E.C. I propose to call that depicted in Fig. 1 the Simplex, and the other, Fig. 2, the Facilis.

The Simplex consists of two cylinders, sliding easily the one within the other. The upper and smaller of the two is closed at the top, as shown in the engraving, and is kept raised by means of a spiral spring which rests against the bottom of the lower cylinder. This latter is closed at its lower end by the solid base on which the instrument stands, and its upper end is made of rather larger diameter than the rest so as to form a circular chamber, which has holes pierced at its under surface. An inlet valve is fitted into the base of the lower cylinder, and an outlet valve to an opening leading to a space between the inner and outer surface of the lower cylinder, which space is continuous with the discharge-pipe.

To use the apparatus, it is placed in a basin or other vessel containing the liquid; the upper cylinder is pressed down by the hand, and then allowed to rise again by the action of the spring. it thus acts as a piston, and the fluid is drawn through the inlet valve into the lower cylinder. On again depressing, the greater part is forced through the outlet valve, and the little that escapes between the cylinders is forced into the circular chamber, whence it runs through the holes on its under surface back into the vessel.

The Facilis is made essentially on the same principle, but it is constructed to feed itself from any vessel, and is intended to be used in cases where it would be inconvenient to stand the apparatus in the fluid: for instance, when it is required to be used in bed, or with delirious or unruly patients. It consists of a cylinder having the upper part of larger diameter than the lower, and fitted with a water-tight cover, which is pierced with an opening fitted with a stuffing box. Inside the cylinder is a loosely fitting metallic piston without packing, kept pressed against the cover by means of a spiral spring. In the centre of the upper surface of the piston is a depression corresponding with the opening in the cover. The piston-rod, which is loose, drops into this. An inlet valve is fitted to an opening in the lower part of the cylinder, to which the feed-tube (the lower of the two shown in the engraving) is attached. The upper tube is fitted to an opening on the under surface of the enlarged part of the cylinder, and acts as a waste-pipe. An outlet valve is fitted to the opposite side of the cylinder, and is connected with the discharge-pipe. To use this apparatus, the feed-pipe and waste-pipe are both allowed to dip into the vessel containing the fluid. Then, on alternately depressing the piston and allowing the spring to raise it, the liquid is drawn into the cylinder through the inlet valve, and forced through the outlet valve. What little makes its way beside the loosely-fitting piston goes into the large upper part, and runs through the waste-pipe back into the vessel.

The advantages which I think these apparatus possess are—1. They are very easy to use, needing only one hand to work them. 2. They are eminently portable, the Simplex being only four inches high and an inch and three-quarters in diameter at its widest part. The Facilis is three inches high and two inches in diameter. 3. There is no packing on the piston; and they are so simple in construction that it is next to impossible they should get out of order. 4. They can be taken to pieces to clean, and put together again in a few seconds, and without trouble. 5. They are made entirely of metal, and the springs are of plated steel, so they are not likely either to rust or become weakened.

Burton-crescent, May, 1872.

BATTLE-FIELD AND ACCIDENT SPLINTS.

By A. Moffitt,

Staff Assistant-Surgeon, Royal Victoria Hospital, Netley.

Whenever patients with fractured limbs, whether occurring in warfare or by accident, have to be transported from the places where the injuries have happened, it is most essential that splints of some kind should be applied before removal is attempted. Splints, however, to answer such emergent purposes, must be different in many respects from the permanent splints usually employed in the treatment of fractures; they should be light and portable, complete within themselves, capable of being rapidly applied, and so simple that bearers of sick and wounded as well as the surgeon can be taught to use them.

Keeping in view these properties—namely, lightness and portability, completeness, rapidity of application, and simplicity,—I have devised two sets of splints with which all fractures of the limbs may be temporarily put up. These splints, after a series of trials, I have found to surpass my most sanguine expectations; and, in addition, I have had the satisfaction of knowing that they have been approved and recommended for use in the army by the Committee appointed by the Secretary of State for War to report on the Hospital Transport of the British Army.

Though designed more especially for military purposes, yet I am led to believe they are well suited also for use in civil life. In mining and manufacturing districts, or wherever accidents occur, medical men, when suddenly called upon to attend to fractured limbs, feel the want of some ready method of putting them up. A surgeon, furnished with these splints, when called to an accident, could, on the spot, in a few minutes, put up any fracture, and thus leave his patient in comparative comfort and safety, and give himself time to decide and get ready at his leisure any other apparatus which the peculiar nature of the injury might demand.

The splints are in two sets: one set is for fractures of the upper extremity—the arm and forearm; and the other set for fractures of the lower extremity—the leg and thigh.

The set for the upper extremity consists of two pads, two straps, two buckles, two splints, and an arm-sling. The pads are made of strong ticking, each made in two pockets, one of which (the larger) is for containing the stuffing, and the other (the smaller) for sliding the splint into. One end of the pockets is shut and the other open, but capable of being closed by a flap fastened with two tapes, when the stuffing and splint have been introduced. On the outside of each pad, running from one end to the other, stitched at intervals, is a piece of narrow webbing. This is for attaching the straps to on one pad, and for loops to pass the straps through on the other. The straps are of strong webbing, each furnished with a brass buckle. They are fastened to the narrow strip of webbing on the outside of one of the pads in such a way that their position can be shifted, but that they cannot be altogether removed; thus obviating the likelihood of their being lost. The splints are Duncan's ratan splints, altered in dimensions and other respects to suit the purpose. Each is three inches and a half in breadth, one nine and the other twelve inches in length. The arm-sling is made of half a piece of calico, a yard square, cut diagonally.

The set for the lower extremity consists of two pads, four straps, four buckles, and two splints. The pads are made similarly to those of the upper extremity, but larger to suit the splints. The straps and buckles are also arranged in the same way, but are longer, to go round the greater thickness of limb. The splints are Duncan's ratan splints, each four inches in breadth, one two feet in length, and the other one foot eight inches.

In time of war it is intended that a complete set of these splints be carried on each stretcher, and for this purpose the committee before-mentioned have provided pockets in the canvas of the new pattern Government stretcher.

The weight of the complete set is two pounds and a quarter. The different parts being attached to each other there is no fear of any of them getting lost. When the pads become dirty the stuffing and splints can be removed, and the covering and straps washed. Hair, wool, or even hay or grass, will, on an emergency, do for re-stuffing them.

The following are the directions for use:—

Fracture of the arm.—Rip the sleeves of the coat and shirt up their inner seams as far as the arm-pit, apply a suitable first dressing to the wound should such exist, make extension of the limb, and replace the broken fragments in their natural position. Now, having bent the elbow to a right angle, place the short splint on the inside of the arm, the long one on the outside, and buckle the straps sufficiently tight to prevent movement. If time and circumstances permit, apply a roller bandage from the fingers up to the elbow. That done, carry the forearm across the chest and support the hand and wrist in an arm-sling.

Fracture of the forearm.—Rip the coat and shirt sleeves as high as the elbow; apply a suitable first dressing to the wound, should such exist; make extension of the limb, and replace the broken fragments in their natural position. Now bend the elbow to a right angle, place the short splint on the inside, and the long on the back of the fore-arm, and buckle the straps sufficiently tight to prevent movement. This done, carry the hand across the chest, and support the whole of the fore-arm in an arm-sling.

Fracture of the thigh.—Lay the patient on his back; rip the trowsers up the outer seam as high as the hip; apply a suitable first dressing to the wound, should such exist; and make extension until the limb is the same length as the sound one, and the broken fragments are replaced in their natural position. Now place the short splint—one end close up into the perineum on the inside, the long one on the outside, and buckle the straps, the lower one immediately below the knee, and the upper round the pelvis. If time and circumstances permit, apply a roller bandage from the toes up to the knee.

Fracture of the leg.—Place the patient on his back; rip the trowsers up above the knee; apply a first dressing to the wound, should such exist; make extension, and replace the broken fragments in their natural position. Now place the short splint on the inside, and the long one on the outside, and buckle the straps, the lower one in a figure of 8, round the foot and lower ends of the splints.

The splints have been made by Messrs. Evans and Wormull, of Dowgate-hill, London, E.C., to whom belongs the patent of Duncan's ratan splints.

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CASE OF MALIGNANT CHOLERA TREATED WITH CALOMEL; RECOVERY.

By William Bloxam, M.D.

W. P——, aged fifty, previously quite well, was seized on July 31st ult. with diarrhœa and vomiting, which continued all day. On the same night the vomiting and diarrhœa became incessant, the alvine discharge being colorless; and cramp occurred in every muscle, even in those of the face.

I first saw him at 9 A.M. on Aug. 2nd. The face was dusky and pinched; eyes hollow; voice whispering; tongue cold, with white fur; vomiting and purging constant, the latter of the characteristic rice-water matter. The skin was cold and clammy; respiration hurried; cramp universal and intense, so severe in the hands that he had grasped a large shell in either palm, fearing, he said, that should the hands once close he could not again open them. The pulse was barely perceptible at the wrist, very small, and could not be counted. The veins on the back of the hands were thread-like in size. No urine had passed since the night of Aug. 1st. Two grains of calomel were ordered to be put upon the tongue every hour from 9 A.M. to noon. He expressed much relief after the second dose; but the rice-water purging, vomiting, and cramp still continued very severe. The surface was warmer, and the veins of the hands had slightly increased in size. The dose was then given every two hours from noon of Aug. 2nd to 8 A.M. on Aug. 3rd.

Five visits were paid to the case on Aug. 2nd, and at 10 P.M. the alvine discharge became green, with white flakes floating in it. The vomit was also occasionally green. Equal parts of milk and water, cold, was the only diet; stimulants were strictly forbidden; warmth was applied to the epigastrium and extremities.

Aug. 3rd.—10 A.M.: Cramp much abated; vomiting and purging less; stools brownish liquid. To take two grains of calomel every three hours; to suck pieces of ice.

4th.—10 A.M.: Still better; motions semi-solid; slight retching; headache; no fever; pulse quiet; passed about six ounces of urine, dark-colored, specific gravity 1010, very acid, slightly albuminous. Concentrated sulphuric acid gradually added produced first a bottle-green, then a deep-red, and ultimately a blackish color. The calomel was now stopped, the patient having taken in all thirty-six grains. One or two doses, however, had been lost by vomiting, and no dose was given when the patient slept at the hour.

Remarks.—This, it is to be hoped, will prove an isolated case only; but it is one of such typical intensity in all but entire pulselessness that it serves to illustrate the plan of treatment which I have pursued in like cases, with like success, since the cholera epidemic of 1854. Having in the epidemics of 1849, 1854, and 1867 held public appointments, in which cholera and diarrhœa came particularly under my notice, I was induced to watch carefully the success of the various plans of treatment. Unhappily the immense pressure of work rendered it impossible to keep account of cases; but the general result was largely in favor of the treatment by calomel. In some cases the arrest of symptoms was immediate, particularly in those of very recent occurrence. Very rarely did ptyalism occur, and then only slightly. The consecutive fever was greatly lessened. I regarded the milk diet as absolutely essential, and have more than once seen a relapse from convalescence caused by simply eating bread. The increased fulness of the veins was always hailed as a good omen, and no doubt indicated that free endosmosis had occurred, and with it a better chance of recovery.

I do not desire to appropriate to myself the merit of first proposing the use of calomel in cholera, which chiefly belongs to the late Dr. Ayre of Hull; but I would earnestly urge upon the profession a fair trial of calomel, in doses varying from a quarter of a grain to two grains, at intervals of from ten minutes to four hours, according to the severity of the case, and age and constitution of the patient; always with milk diet only, and never by any means giving either opiates or stimulants. This method has succeeded in infants at the breast and in the aged, and is applicable not only to confirmed cholera, but to those suspicious cases of violent diarrhœa and vomiting occurring at epidemic periods.

I have abstained from theorising upon a disease of which so little is yet known, but would observe that among the very large number of cases which have passed through my hands, those almost always did best where the diarrhœa and vomiting were urgent, and that many suddenly collapsed in which those symptoms were very slight, the intestines being found, on post-mortem examination, full of rice-water matter.

I strongly incline to the view that the success of the calomel treatment favors the "elimination theory."

Mount-street, W., August, 1872.

Cantharides Poisoning.—Dr. Pallé, in a recent number of the *Journal de Phar. et de Chimie*, relates an interesting case of some soldiers who had in mistake drunk large quantities of tincture of cantharides. Although the physical suffering and agony were very great, there were no cerebral symptoms apart from the excitement caused by pain and terror. Vesical and rectal tenesmus were observed in all the patients. The penis was swollen and painful, but not rigid; no priapism and no aphrodisia. In every case the urine was albuminous, while some suffered from paraplegia. The treatment pursued, and under which all recovered, was the ordinarily received one—viz., emetics, warm baths, camphor, and opium, with oleaginous drinks and injections.

A Mirror
OF
HOSPITAL PRACTICE,
BRITISH AND FOREIGN.

> Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—Morgagni *De Sed. et Caus. Morb.*, lib. iv. Prooemium.

KING'S COLLEGE HOSPITAL.

The following operations were performed at this hospital on the 27th July:—

REMOVAL OF SCIRRHOUS MAMMA.

(By Sir William Fergusson.)

The patient was a married woman, aged forty-three. She first noticed a swelling in the right mamma twelve months ago; but now the whole gland was converted into a firm dense mass, and there was also extensive implication of the axillary glands. Otherwise the woman looked healthy and well nourished.

The breast was removed in the following manner:—A long elliptical-shaped incision, extending from nearly the middle line of the chest to the axilla, was made through the skin, and then the tissues were cut wide of the tumour, the pectoral muscle being freely exposed, although the cancer did not appear to have implicated it. The mass of diseased axillary glands was then removed, being for the most part torn away, the knife being used very sparingly. A few vessels were tied, and the wound brought together by silk sutures supported by strapping and pads of dry lint.

Sir William thought that the duration of the disease was probably longer than twelve months, as it is very rare to find scirrhus spread so extensively in so short a time, nearly all the axillary lymphatic glands being diseased. It was true that some of the diseased glands were only small, but these were just as dangerous as the large ones, and it was therefore of the first importance to get them all away. This is best done by the finger and thumb, partly because there is considerable danger in using the knife in this region, but chiefly that the diseased structure can be more effectually removed by this process of enucleation than by dissection with the knife. The great advantage of enucleation by manipulation was well shown in this case; for, after the diseased structures had been removed, the large axillary vein was soon exposed for a considerable extent, and had the knife been used the vein might have been seriously injured. Sir William further remarked that it would have been bad surgery to have removed the diseased mamma and left the glands; for, from experience, it is well known that cancer in the axillary glands is much more rapid and destructive than when it is confined to the breast.

DISEASE OF LOWER END OF FEMUR AND KNEE-JOINT; AMPUTATION.

(By Mr. Henry Smith.)

The patient, a thin, delicate-looking man, between thirty and forty years of age, had suffered from disease about the knee-joint for over twenty-five years, during which time he had been more or less crippled. Latterly, the disease had extended, and the patient was recently admitted into the hospital with the right knee-joint full of pus. Incisions were made into the joint, and a large quantity of pus let out, with temporary relief to the patient. There was, however, extensive disease of the femur at the lower part, and, as the man's health was suffering considerably from the excessive suppuration, amputation through the thigh was advised, and to this the patient consented. Accordingly, the diseased limb was removed by antero-posterior flaps about the middle third of the thigh, the posterior flap being a little longer than the anterior. The femoral artery was compressed against the pubes by means of the thumb. On transfixion a quantity of thick curdy pus escaped. Several vessels were ligatured, and the flaps then brought together by silk sutures, supported by strips of wet lint, and a bandage over the end of the stump.

Mr. Smith remarked that excision of the joint could not have been done in this case, as, on account of the disease of the lower end of the femur, at least four inches of that bone would have to be removed.

TUMOUR OF TIBIA; AMPUTATION THROUGH THE KNEE-JOINT.

(Under the care of Sir WILLIAM FERGUSSON.)

A. B——, a delicate-looking man, aged thirty, had been under observation with a tumour of the right tibia for the last seven months. The growth commenced between eight and nine months ago as a swelling from the front of the tibia; it gradually increased in size for several months, but then took on more rapid growth, the limb increasing an inch and a half in circumference during the last three months, until it formed a large mass of irregular consistence, extending downwards from about two inches below the head of the tibia, and taking in about seven inches of that bone. It appeared also to embrace the fibula, which at first was quite free. The greatest circumferential measurement was twenty-two inches. The skin was distended, but of normal color, the veins being only slightly increased in number and size. The operation was performed in the following manner:—

Chloroform being administered, the artery was compressed against the pubis by means of the thumb. An oblique incision was then made through the skin from above downwards and backwards for about two inches on each side of the limb opposite the upper part of the tumour; the lower ends of the incisions were then united by a transverse cut above and below. The anterior skin flap thus made was then dissected back, the limb flexed, and the joint opened, the ligaments divided, and the knife carried behind the joint, cutting outwards and downwards to form the short posterior muscular flap. A few vessels were ligatured with whipcord, and the flaps brought together by silk sutures, and supported by means of pads of wet lint covered with oiled silk and a bandage.

Sir William remarked that when the tumour was first seen it was so small that no opinion could be given as to its nature, except the opinion founded on the experience that tumours of the tibia, not venereal, will probably increase in size. It would be impossible to say whether it is benign or malignant; the first impression is that the tumour is malignant. From an early period it was evident that nothing short of amputation would be of any value. If the tumour were in the fibula, the whole or a portion of that bone might be removed; but not so in the tibia, and especially in a case like this, where the growth affected the whole thickness of the shaft, and probably began from its interior. Amputation through the knee-joint seemed to be a justifiable operation in this case. This operation, which was formerly rare, has now become more frequent, owing largely to the advocacy of Mr. Carden, of Worcester. Whether it is a good operation remains to be seen; but Sir William thinks it is. Further, this would seem to be a good case in which to employ it; for the disease is entirely limited to the tibia, and if the growth be not positively malignant it will not recur. But it does not necessarily follow that if there be a tumour of a bone, the whole of the bone must be removed, as some surgeons think, and Sir William has again and again protested against such extreme measures. The anterior flap was sufficiently long to give a good covering to the end of the femur, and did not necessitate the removal of the patella, which remained *in situ*, so that the part of the joint opposite and above that bone was not exposed. It is a moot point whether the patella should remain or not. Should the cartilage be removed? In this case it was not, and this is perhaps the best practice when it is healthy; for then there is little fear of absorption of septic matters from the wound, as may happen when the cancellous tissue is exposed. The stump, as far as could be judged, was a good one. But the operating-table is not the place from which to judge a stump; it is when the limb is used, three, four, or six months after, that we must form our opinion.

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GUY'S HOSPITAL.

CANCER OF THE TIBIA; AMPUTATION THROUGH THE KNEE-JOINT; RECOVERY.

(Under the care of Mr. BRYANT.)

S. H——, aged fifty, a shoemaker, was admitted on the 22nd of April, 1872, with cancer of the left tibia. He stated that none of his relatives have ever had cancer in any form. His parents, brothers, and sisters have always been remarkably strong and healthy. Thirty-six years ago (the patient being then fourteen years old) he had a violent fit of shivering after bathing in a stream; he went home, feeling ill, and the following morning had a severe pain in the left leg. In a few days the right leg also pained him, and both legs swelled, especially around and just above the ankles. Abscesses then formed in both legs from the ankles to the knees; and the left leg, which was the worst, increased to an enormous size, becoming nearly as large as his body. They were opened by his medical attendant in several places, and discharged large quantities of pus. About two years after he was first taken ill a piece of bone, two inches long, came away from his right leg. After this numerous pieces came away from both legs, the largest being five inches long and one inch broad. Small pieces of bone have continued to come away from both legs until two years ago, when the last piece came from the left leg, leaving a wound over the front of the tibia. The wound did not heal as previous ones had done, but slowly increased in size. Sixteen months ago, the wound being very painful and bleeding a great deal, he went to the London Hospital, and was there six weeks.

On admission at Guy's he appeared to be in tolerably good health, but was rather thin. Over the front and inner side of the left leg was a large fungoid growth, measuring 5¼ in. by 4 in., involving the bone and the superficial parts; it was considerably raised above the general surface, but had a very irregular appearance, presenting a number of soft papillary projections of a bright-red color; it was covered with a thick layer of pus, which had a very offensive smell. Its margins were everted, and the skin was very red and inflamed for some distance around. The glands in the left groin were enlarged and indurated; those on the right side slightly so. Pulse 100; temperature 98·4°. The urine contained a large amount of phosphates, but no albumen.

On May 7th, the patient being under the influence of chloroform, Mr. Bryant proceeded to amputate the leg at the knee-joint. He first made an anterior rectangular skin flap, and then a posterior one of about the same length, but including all the soft parts; the joint was then opened from the front, but the patella was not taken away. Considerable difficulty was experienced in securing the vessels by torsion, one good-sized vessel causing a long delay; this was finally secured by a carbolised catgut ligature, which was cut off short and left in. The flaps were then brought together with silk sutures, the limb firmly strapped to a short back-splint, and the whole enveloped in a fold of cotton wool and secured by a bandage.

On the following morning the pulse was 120; temperature 103·8°; tongue slightly furred. He had slept for several hours during the night, and felt comfortable; had taken four ounces of wine and a considerable quantity of milk. On the 9th the pulse was 136; temperature 102·8°. On the 10th the pulse was 134; temperature 103·2°. He complained of a good deal of pain in the stump, which was also swollen. The dresser cut a few of the upper bandages, which relieved him. In the evening he was in so much pain that the house-surgeon removed the dressings, and found that there was a considerable swelling on the outer side of the stump, and the skin was red and inflamed. On the 12th two or three of the sutures were removed, and some pus escaped. Pulse 130; temperature 103°. On the 13th the remaining sutures were removed. The anterior flap looked healthy; it completely covered the condyles. The posterior flap had retracted about two inches, leaving a suppurating surface. The thigh was considerably swollen, especially on the outer side; when it was pressed, pus poured out from the stump. On the 16th the posterior flap had retracted still more, leaving an interval of about four inches between it and the anterior flap, exposing the posterior part of the condyle and the intercondyloid notch. Small granulations now appeared in places. There was a considerable discharge of very offensive pus, which appeared to have burrowed almost up to the hip; when pressed downwards, the pus ran from the stump. The anterior flap had sloughed through in one place, making a hole about one inch in diameter, exposing the inner condyle. On the 30th, an incision was made into the swelling about half-way up the outer side of the thigh, and about half a pint of thick pus escaped. On June 5th the stump was healing rapidly by granulation; but there was still a considerable discharge of pus both from the stump and the incision. On the 17th the stump was doing well; there was still a great deal of discharge from the incisions made in the thigh.

July 1st.—Patient is doing well; his general health has improved the last week or two: takes his food well, and sleeps better; he has also gained flesh.

Aug. 8th.—The patient has greatly improved in health since the operation. The stump is almost entirely healed. To leave the hospital in a day or two.

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ST. GEORGE'S HOSPITAL.

A CASE OF VICARIOUS MENSTRUATION; DEATH.

(Under the care of Dr. CLARKE.)

FOR the following notes we are indebted to Mr. E. R. Rowland, resident obstetric assistant.

J. C——, aged eighteen, was admitted into the hospital on May 30th, 1872. The history of the case is as follows:—Her family were healthy. She was single, and had never seen any catamenial discharge; but for three months before admission she had from time to time suffered pain at the lower part of the back and between the shoulders. During these attacks of pain she had bleeding from the nose and gums, which lasted about a week, and then ceased, returning again after the interval of one month. For two or three weeks before she came into the hospital she had had great irritability of her skin; to relieve which she had recourse to scratching, but this gave rise to immediate bruising of the parts. For four months past she had complained of pain in the left side, accompanied with difficulty of breathing, cough, and spitting of blood. She had never had rheumatic fever; but about five years ago she suffered from chorea.

On admission she was very anæmic, the lips and conjunctivæ being almost bloodless. She suffered from shortness of breath, and had frequent bleedings from the nose, mouth, and skin. She said she had never menstruated. There were hæmorrhagic spots on the tongue, inside the lips, and on the gums. Some of the spots on the tongue were as large as half a split pea, and the tip was so covered with ecchymoses that it had the resemblance of a strawberry. The lips were cracked, and on the inner side were numerous ecchymosed spots. The surface of the chest was more or less marked with these hæmorrhages, but here some of the spots could be picked off. At the places where scratching had been practised there were distinct bruises. On the legs and thighs the spots had more the character of the hæmorrhages seen in purpura. In many places the blood seemed to have actually exuded from the skin, as they could readily be lifted off; but there was no evidence that mechanical means had been employed to produce them. For four or five days she had suffered from epistaxis. On examining the chest, a loud mitral murmur, most marked at the apex, was heard, the heart's action being very irregular and rapid. The lungs were resonant, and air freely entered; but the breathing was rapid and labored even after slight exertion. There was a troublesome cough, and occasionally the patient spat blood. There was no vaginal orifice; the small cavity representing the canal of the vagina ended in a cul-de-sac, and was not deep enough to hold a teaspoonful of fluid. The urethra was in the middle of this cavity. The labia majora were well formed, but small, and there was an ordinary amount of pubic hair. The space between the rectum and the urethra measured about half an inch. On passing the finger into the rectum, no uterus could be discovered; and, when a catheter was introduced into the bladder, it could be distinctly felt through the anterior wall of the rectum. Numerous ecchymoses were present on the inner side of the labia majora. The patient was ordered beef-tea, milk, and eggs.

May 31st.—Breathing easier; ecchymoses still come out; bleeding from nose and gums about the same as above. Ordered two drachms of infusion of digitalis, and twenty minims of tincture of perchloride of iron, to be taken every four hours with some spirits of nitrous ether. At night to have a purge composed of ten grains of calomel and colocynth pill.

June 1st.—Medicine to be discontinued and purgatives to be administered. The patient complained in the evening of feeling faint.

2nd.—Bowels have acted freely; epistaxis much diminished; complains of feeling sick; has vomited. To have some hydrocyanic acid and soda mixture every four hours.

3rd.—Spots gradually fading; bleeding from nose and gums nearly stopped. Purgative medicine to be discontinued.

8th.—Gradually improving.

The patient continued to improve till June 11th, when the breathing became much embarrassed, and accompanied with severe palpitation of the heart, cough, and spitting of blood, death taking place at 3 P.M., consciousness remaining till the last.

Autopsy.—Body well nourished; limbs and trunk covered with ecchymoses. Mammæ fairly well developed, but nipples small. Color of the hair light brown. On opening the thorax the pleuræ were found to be spotted with ecchymoses. The lungs were œdematous, and gorged with blood. The pericardial cavity contained a small quantity of light-red fluid, but the walls were dotted with hæmorrhagic spots, especially the visceral wall. The endocardium at the upper part of the left ventricle was thickened and opaque. The aortic valves were thick, puckered, and inefficient; the mitral valve thickened, and so contracted that the orifice would only admit the tip of the little finger. The muscular walls of the right ventricle and left auricle much hypertrophied. The liver, spleen, and kidneys did not present any abnormal appearance. The ovaries were very well developed and congested, and contained a recent false corpus leutem. The uterus was absent (evidently congenitally), only a small nodule of fibrous tissue being found in the folds of peritoneum between the rectum and the bladder.

ST. MARY'S HOSPITAL.

ACUTE YELLOW ATROPHY OF THE LIVER; DEATH.

(Under the care of Dr. SIEVEKING.)

THE following notes are communicated by Mr. Cole, house-surgeon.

Henry F——, aged twenty-four, a coachman, was admitted into the Albert ward on the 25th of June. He was single, and of sober habits. No history of syphilis, nor any fever. He was quite well until a week before admission, when he was taken ill with pains in the back and abdomen, and sickness. On the 26th the following notes were taken:—The patient complains of pain in the back and loins, slight headache, pain in the epigastrium, and in the legs if he tries to walk. There is general jaundice of the trunk and extremities; conjunctivæ yellow; tongue moist and furred; bowels regular; pulse 44; temperature normal; urine scanty, sp. gr. 1020, contains a large quantity of bile, but no albumen. Hepatic dulness extends from the level of the right nipple to three inches below the margins of the ribs. Ordered three grains of calomel at bedtime, with an ounce and a half of compound senna mixture in the morning; and to have infusion of broom, with some spirits of nitrous ether, three times a day. Simple diet.

July 1st.—Patient restless and somewhat strange in his manner.

2nd.—Jaundice not diminished; hepatic dulness extends only as low as the eighth rib. Ordered a hot bath in the evening; after which the patient became violently delirious, and apparently in pain from inability to pass his urine, which had to be drawn off with a catheter.

3rd.—Answers questions with difficulty. Some difficulty in swallowing; bowels relaxed. To have one drachm of taraxacum added to his mixture.

4th.—Patient violently delirious at intervals of about twenty minutes during the night. The delirium is followed by a heavy comatose condition, from which he can be roused. Urine is passed involuntarily, and under the microscope shows crystals of leucine. Bowels relaxed; pulse much quickened (120) during delirium; temperature 97·8° F.

5th.—Area of hepatic dulness extends from the lower margin of the fifth rib to the seventh rib. Jaundice continues. Ordered to have spongiopiline soaked in a lotion containing one drachm of nitro-muriatic acid to the ounce applied to the region of the liver. To have ten minims of ipecacuana wine and one fluid ounce of taraxacum every four hours.—10 P.M.: Temperature in axilla 106·5° F.

6th.—5 A.M.: Death.

Autopsy, nine hours after death.—Liver small, and in parts softer than natural. On section and on the surface were numerous yellow patches, where the acini could not be distinguished, giving the liver the appearance of agate. Under the microscope, the liver-cells were found to be in an advanced state of fatty degeneration; but no leucine or tyrosine could be seen for several days, and then only the former. The hepatic vein was distended with blood. Weight of liver, forty-four ounces. The gall-bladder contained about an ounce of pale greenish bile. The kidneys appeared healthy, but were larger than usual. The spleen was enlarged and congested. Heart healthy; but beneath the visceral pericardium was a considerable amount of ecchymosis, especially on the left side. Lungs congested; the right in a state of red hepatisation. Brain natural, and not colored like the other tissues of the body; dura mater tinged yellow.

Dr. Sieveking remarked that the above name fully expressed the more evident conditions of the disease, without involving any theory as to its intimate nature, which is not at present known. The symptoms do not accord with the views ordinarily taken as to what constitutes *inflammation* of the liver. The case is further interesting, inasmuch as it occurred in the person of a young man in whom none of the ordinary causes of the disease could be found to exist. The prognosis is very unfavorable and the treatment unsatisfactory, as we are ignorant of the true nature of the disease; and, further, the disease is rarely recognised with certainty till the symptoms of blood-poisoning show themselves, which in turn indicate an advanced degeneration of liver-tissue.

ST. BARTHOLOMEW'S HOSPITAL.

ACUTE PULMONARY TUBERCULOSIS; TUBERCULAR PERITONITIS; DEATH.

(Under the care of Dr. BLACK.)

C. J. A——, aged thirty-six, was admitted into the Matthew ward Feb. 13th. The patient had been ailing some months, and in December last he was confined to his bed with ascites, which was said to be due to an enlargement of the liver, but under treatment the ascites subsided without tapping. The patient said that, when a youth, he was much subject to headaches, especially after reading. Lately he had had many domestic troubles, and had been in the habit of taking a large quantity of opium and spirituous liquors.

At the time of admission the patient had a clear complexion, but was emaciated. The eyes had a vacant expression; the speech clear; tongue clean; pulse 116, regular, but rather weak; temperature normal. The sounds of the heart and lungs normal; the abdomen much retracted; a few nodular bodies, supposed to be enlarged mesenteric glands, could be felt through the abdominal walls; no diarrhœa. Ordered one drachm of syrup of iodide of iron three times a day, and to have four ounces of wine.

Feb. 17th.—In the morning the left side of the face and temple red and congested. Pulse 120, feeble, but regular; temperature 100·6° F. Bowels costive. Has no pain nor feeling of heat in the face. In the evening there was well-marked erysipelas of the face, which had spread. Ordered to take four grains of carbonate of ammonia with some decoction of chinchona; the wine to be increased to eight ounces.

18th.—Erysipelas spread over greater part of face. Pulse at 10 A.M. 126; temperature 100·2°, being about the same in the evening. Answers questions coherently.

19th.—Not quite sensible; does not answer coherently. The erysipelas covers the whole of the face and forehead. Tongue moist; bowels costive. Pulse about 110; temperature 98° in the morning, and 99·8° in the evening.

21st.—The erysipelas dying off; patient looks brighter; temperature 99·8°.

March 2nd.—Patient is weaker, and complains of pain in the abdomen. Mind quite clear.

11th.—Died, having had great pain in the abdomen for two or three days before death, but no diarrhœa.

Autopsy, thirty-six hours after death.—Brain atrophied; membranes full of fluid; no tubercle. Pericardium and heart normal. Recent adhesions of both pleuræ, and on the left side were collections of concrete pus. On stripping off the false membranes, the pleuræ were seen to be studded with granulations, having all the appearance of tubercle. A few tubercles could be detected in the lungs themselves. The bronchial glands were caseous. The periosteum was everywhere firmly adherent, the serous membrane being loaded with tubercles. On the capsule of the liver many small tubercles were seen, and a few could be detected deeper in the tissue, some of which had softened into little greenish cysts. The spleen also contained a few yellow tubercles. In one kidney was a cheesy mass, of about the size of a horsebean, situated in the cortex. The enlarged mesenteric glands were extensively tubercular; and in the intestines many tubercles could be seen through the mucous membrane, but there was no ulceration present. The stomach was healthy.

The interesting points in this case are—1st. The obscurity of the diagnosis, the only symptom pointing to the peritonitis being the retraction of the abdomen and the enlargement of the mesenteric glands. There was no diarrhœa and no pain, excepting a few days before death. 2nd. That there was no evidence during life of the acute pulmonary tuberculosis, there being no cough, no expectoration, no dyspnœa, and no pain in the chest; nor was the temperature elevated to any degree. 3rd. The peculiar character of the erysipelas, which consisted simply of a deep red staining of the skin, there being no swelling; and lastly, the erysipelas seemed to do very little harm, and cause very little disturbance.

LONDON HOSPITAL.

INCARCERATED SCROTAL HERNIA; OPERATION; ARTIFICIAL ANUS; RECOVERY.

(Under the care of Mr. JONATHAN HUTCHINSON.)

THIS case presents many points of considerable interest. In the first place, it is a typical case of *incarcerated* hernia—that is, a hernia that is rendered irreducible by impaction of fæces. Another point is the fact that there was a total absence of strangulation, and yet the bowel was sloughy. Further, there was no vomiting and no constipation, but the prostration was extreme—so great, in fact, that at first the patient's life was despaired of. Lastly, we have the great relief following the operation, and the gradual uninterrupted progress towards recovery.

John G——, aged sixty-three, was admitted into the hospital on July 18th, with a large scrotal hernia on the right side, which could not be reduced. The patient has had a rupture on the left side for thirty-six years, and for many years he has had an occasional hernia, which he has always easily "got back," on the right side. Three weeks before admission, he became ill, and was confined to his bed; the bowels were costive, nodules of hardened fæces passing at every motion. About a fortnight before admission, the patient noticed that his right rupture had come down, but this time he was unable to replace it. This remained, but there was no constipation, the bowels being opened forty-eight hours before operation. On admission, and for several days before, the patient suffered intense constitutional depression, and became feverish and very exhausted, but there was no vomiting, nor did he complain of much pain. When taken into the hospital, he was in a state of extreme prostration and collapse. The right scrotum was enlarged, swollen, tender, and of a dusky red hue.

Chloroform was administered and the sac cut down upon and opened. The tissues of the scrotum were found thickened and infiltrated; the hernial sac was distended with about half a pint of turbid, fetid serum, and large masses of recent lymph. The neck of the sac was quite free, and no constriction could be felt. At the upper part of the sac was a knuckle of intestine, which contained a mass of fæces of almost stony hardness, but which could be indented by firm pressure with the finger-nail. At the lower part of the knuckle was a small sloughy patch of about the size of a sixpenny piece, and from this point the intestine was incised for about an inch and a half, and then the hard fæcal mass (of about the size of two walnuts end to end) was turned out. After this about a pint of soft liquid fæces escaped. The bowel was then stitched to the margins of the scrotal wound, and an artificial anus thus established, all the fæces escaping through it.

On the day after the operation the patient complained of slight pain in the abdomen, but this was soon relieved by the application of poultices to the abdominal walls, and by the administration of opium in suppositories. He was ordered liquid diet and some stimulants. On the second night after the operation his temperature was 101·5° Fahr.; pulse about 100. Fæces passed through the artificial anus without pain, only flatus passing by the rectum.

Since then he has gradually improved, never having had a bad symptom, and is now convalescent, the greater part of the fæces passing through the natural anus.

Medical Societies.

OCTOBER—NOVEMBER.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

MR. T. B. CURLING, F.R.S., President, in the Chair.

ON THE PATHOLOGY OF THE MORBID STATE COMMONLY CALLED CHRONIC BRIGHT'S DISEASE WITH CONTRACTED KIDNEY (ARTERIO-CAPILLARY FIBROSIS).

BY SIR WILLIAM GULL, BART., M.D.,

AND

HENRY G. SUTTON, M.B.

THE authors of this paper commence by stating that Dr. Bright and subsequent pathologists have fully recognised that the granular contracted kidney is usually associated with morbid changes in other organs of the body; and the disease in the kidney and the other coexistent morbid changes are commonly grouped and collectively termed "Chronic Bright's disease." It is generally assumed that, in chronic Bright's disease, the kidneys themselves are the organs primarily affected; and, in consequence, a cachexia is induced through which other organs subsequently suffer and undergo chronic changes. The authors consider that the history of the disease does not support this opinion. The morbid changes in the kidneys themselves are first considered, the coarser anatomy being omitted. The microscopical appearances observed in granular contracted kidney are given in detail, and their conclusions on this point are as follows:—The visible morbid changes in granular contracted kidneys are due to the primary formation of a fibroid or hyalin-fibroid substance in the intertubular parts, including the vessels, and to atrophy of the tubular and intratubular structures of the kidney. On this point the authors confirm the observations of Dr. Dickinson and other observers. And they further state that this formation commences in different parts of the kidney, commonly near the surface; but it also seems to commence in the outer coats of the arterioles, and in the walls of the capillary vessels. From these parts it extends round the convoluted tubes and Malpighian bodies. This fibroid or hyalin-fibroid substance subsequently contracts and draws the Malpighian bodies together, compresses the urinary tubules and vessels, and may entirely obliterate them. This thickening of the capillary walls, and the diminished calibre of some of the arterioles, must naturally interfere with the nutrition of the tissues and tend to produce atrophy; and the blood-supply to the secreting cells being in this manner reduced probably causes diminished secretory function and atrophy from diminished use. The alteration in the renal epithelium, when great, is the result of the atrophy; and its slighter changes, such as granular appearance and desquamation of some of its cells, are not peculiar to granular disease of the kidney, such slighter changes being even consequent on the process of dying. The morbid changes in the vascular system are next considered. Dr. Bright and subsequent observers have recognised that atheromatous arterial disease is common with granular contracted kidney. In 1852, Dr. George Johnson, in his work on Kidney Disease, stated that the minute renal arteries are much thickened in chronic Bright's disease, owing to hypertrophy of the muscular coats of the vessels. A few years ago the same writer pointed out that the arterioles, not only in the kidney, but in the skin and other parts of the body, are thickened and their muscular coat hypertrophied in chronic Bright's disease. Sir Wm. Gull and Dr. Sutton describe at considerable length the changes that they have found in the minute arteries and capillaries of the kidneys, pia mater, and other parts; and they state that their observations show that the minute arteries and capillaries are thickened in chronic Bright's disease, and they remark, "we gladly acknowledge the debt the science of medicine owes to Dr. George Johnson for so distinctly insisting upon this fact." The microscopical observations of Sir Wm. Gull and Dr. Sutton show that the arterioles and capillaries are more or less altered in chronic Bright's disease. This alteration is due to a hyalin-fibroid formation in the walls of the minute arteries, and hyalin-granular change in the corresponding capillaries; that this formation occurs chiefly outside the muscular layer, but it also occurs, but to a less extent, in the tunica intima of some of the arterioles. Further, that the degree in which the affected vessels are altered, and the extent to which the morbid change is diffused over the vascular system of the different organs, vary very much in different cases. The muscular layer of the affected vessel is often atrophied in a variable degree. The authors recognise that some pathologists may consider that the perivascular canals are the seat of these hyalin-fibroid changes. On the existence of such canals they express no opinion, they assert only that the morbid changes are chiefly outside the muscular layer of the arterioles. The arterio-capillary changes observed in chronic Bright's disease are not seen in the vessels of healthy persons who have been accidentally killed or who have died of phthisis and of other diseases not allied to chronic Bright's disease. The condition which induces the vascular change is next considered. Dr. George Johnson states that the general arterial thickening is due to muscular hypertrophy. He considers the blood is impure in consequence of the kidney disease, and the arterioles resist the passage of this impure, more or less noxious blood, and in doing this they become hypertrophied. The left ventricle of the heart, therefore, makes an increased effort to drive on the impure blood, and the result of this antagonism of forces is, that the muscular walls of the arteries and those of the left ventricle of the heart become in an equal degree hypertrophied. This theory Sir William Gull and Dr. Sutton do not accept, for their observations show that arterial changes are not dependent on muscular hypertrophy but on hyalin-fibroid formation. They have found the heart and vessels healthy in cases of chronic disease of the kidneys, and, further, their inquiries show that the cardiac and vascular changes may occur independently of renal disease. Cases given in the appendix to this paper are next alluded to, which show that there is a general morbid state in which the kidneys may be contracted, the heart hypertrophied, and the minute arteries and capillaries altered by a hyalin-fibroid formation. The kidney changes are most often, but by no means always, part and parcel of this morbid state, and their absence shows that the renal changes are not an essential and indispensable part of the general process; but as the vascular system is at some part affected with this hyalin-fibroid change in all the cases, therefore they conclude that the vascular disease is to be regarded as the constant and essential part of this morbid state. The pathology of this hypertrophy of the left ventricle is next considered. Bright considered that the quality of the blood was altered by the kidney disease, and the heart, in consequence, had to contract with greater power to force the morbid blood through the vascular system, and became hypertrophied in order to accomplish this. Many pathologists have adopted Bright's explanation. Dr. Wells, in 1853, suggested that the hypertrophy might be dependent on atheromatous changes in the vessels. Against the explanation offered by Bright and others, the authors state that the frequent association of cardiac hypertrophy and renal disease does not prove that there is a casual relation between these two morbid states. Moreover, that in many cases where there is chronic disease of the kidneys, and the blood therefore presumably impure, the heart is not hypertrophied—instance, large white, lardaceous and scrofulous kidneys, as well as some cases of granular contracted kidneys. Dr. Johnson has endeavored to account for the absence of the hypertrophy in such cases by assuming that the muscle of the heart is imperfectly nourished. The authors consider this suggestion, and state that this may explain a dilatation disproportionate to the hypertrophy, but it does not explain how a normal-sized heart acquires additional force requisite to overcome the supposed obstruction. Evidence is next brought forward to show that the cardiac hypertrophy is induced by the morbid changes referred to in the vascular system, the heart being found hypertrophied in all the cases in which the vessels were much and generally thickened by hyalin-fibroid change: slightly hypertrophied where the vessels were little thickened; and greatly hypertrophied where the vessels were much thickened, although there might be little or no kidney disease. The hyalin-fibroid change is assumed to impair the elasticity of the vessels, thus imposing upon the left ventricle a necessity to contract with greater force to carry on the circulation. The other conditions which make up the morbid state known as chronic Bright's disease are next noticed—namely, vesicular emphysema, retinitis albuminurica, atrophied brain, contracted spleen, and morbid changes in the intertubular parts of the stomach. In all these conditions it is shown that the arterioles are more or less thickened by fibroid or hyalin-fibroid changes. This general morbid state is shown to belong principally to the period of life at or after forty years of age, and after forty its frequency greatly increases as age advances. It is pointed out that the kidneys, even in children or in other young persons under adult age, are sometimes much contracted and death caused by uræmic poisoning, without the cardio-vascular changes alluded to. But the general hyalin-fibroid change in the vessels may occur in early life; and, in proof, the case of a girl aged nine years is mentioned, where the kidneys were granular and very contracted, the heart hypertrophied, and there were hyalin-fibroid changes in the arterioles of the pia mater.

The authors desire to show that in the state known as chronic Bright's disease with contracted kidney, the morbid changes described do not arise in a constant order. In some cases the changes seem to commence in the kidneys or in the heart, sometimes in the lungs or in the brain, or in other organs; hence the symptom of the disease varies very much in different cases. In all cases, whether many or few organs are affected, the minute arteries and capillaries are altered by hyalin-fibroid formation, attended with atrophy of the adjacent textures. The authors cannot regard the functional disturbances which occur in many organs during chronic Bright's disease with contracted kidney as dependent on blood changes only or chiefly—as, for instance, pain in the head, discomfort after food, palpitation, dry skin, epistaxis, &c. These are probably due not so much to changes in the blood as to changes in the tissues themselves.

The conclusions arrived at in the paper may be briefly summed up as follows.—1. There is a diseased state characterised by hyalin-fibroid formation in the arterioles and capillaries. 2. This morbid change is attended with atrophy of the adjacent tissues. 3. It is probable that this morbid change commonly begins in the kidney, but there is evidence of its beginning; primarily in other organs. 4. The contraction and atrophy of the kidney are but part and parcel of the general morbid change. 5. The kidneys may be but little, if at all, affected, whilst the morbid change is far advanced in other organs. 6. This morbid change in the arterioles and capillaries is the primary and essential condition of the morbid state called chronic Bright's disease with contracted kidney. 7. The clinical history varies according to the organs primarily and chiefly affected. 8. In the present state of our knowledge we cannot refer the vascular changes to an antecedent change in the blood due to defective renal excretion. 9. The kidneys may undergo extreme degenerative changes without being attended by the cardio-vascular and other lesions characteristic of the condition known as chronic Bright's disease. 10. The morbid state under discussion is allied with the conditions of old age, and its area may be said hypothetically to correspond to the "area vasculosa." 11. The changes, though allied with senile alterations, are probably due to distinct causes not yet ascertained.

Dr. Broadbent commenced the discussion by avowing his intention to confine himself to the question as to the nature of the changes in the minute arteries. Sir William Gull, Dr. G. Johnson, and Dr. Sutton were agreed that in contracted granular disease of the kidney there was hypertrophy of the heart and thickening of the arterioles, and that the cardiac hypertrophy was a true muscular hypertrophy due to changes in the arteries. But these physicians differed as to the character of the thickening of the arterial walls, and as to its mode of production. Dr. Johnson believes it to be true hypertrophy of the muscular coat, caused by overwork in resisting the passage of impure blood. Sir Wm. Gull and Dr. Sutton record their belief that it is a degenerative change in the tunica adventitia, the causation of which is unexplained. The ultimate appeal was naturally to the microscope, and Dr. Broadbent did not pretend to have traced the history and progress of the changes as closely as the authors. But he was so firmly convinced as to the existence of hypertrophy of the muscular coats of the walls of the minute arteries that, if compelled to relinquish this belief after what he had seen, he should cease to credit the existence of muscular fibre at all in the walls of normal arteries, for the appearances were indentical in kind, differing only in degree. He proceeded to canvass the arguments advanced in support of Dr. Johnson's theory. That the cardio-vascular hypertrophy is not usual in other forms of diseased kidney, and has therefore some other cause than renal disease, and consequent deterioration of the blood, to be valid, involves the assumption that the effects in the blood are indentical in all affections of the kidney. It is not sufficient that the blood is rendered impure, because one sort of impurity may, and another may not, provoke vascular resistance. But it is obvious that the deterioration of the blood in granulated contracted kidney cannot be the same as in waxy kidney. In one, the urine is abundant, pale, of low specific gravity, and contains little albumen; in the other, it is scanty, with coloring-matter present in the usual proportions, more albumen, and higher specific gravity. The blood cannot, as a mere matter of subtraction, be in the same state, to say nothing of the less tangible but more important distinction which the differential characteristics of the urine indicate. In large kidney, too, the course of the disease is shorter, and hypertrophy requires time. Scrofulous kidney is attended with suppuration, which latter condition is associated with vascular relaxation. And, after all, the forms of renal disease other than contracted granular kidney are not uncommonly attended with arterial tension and thickening and cardiac hypertrophy, and this is only to be explained by Dr. Johnson's theory. The second objection, that the cardio-vascular changes are not constant in contracted granular kidneys, again, is of no great weight, for exceptional cases are very rare and absolute uniformity cannot be expected in conditions so complicated. The general vascular change indicated in the views of Sir Wm. Gull and Dr. Sutton is not consistent with Dr. Johnson's theory, as the former imply arterial resistance and consequent hypertrophy intermediate between the renal disease and the hypertrophy of the heart. The vascular changes, indeed, need not be proportionate to the degree of kidney disease, since a long-continued slight amount of kidney mischief may cause greater vascular hypertrophy than a more rapid and complete degeneration of their organs. And with reference to Dr. Johnson's theory that the change in the arterioles is hypertrophy of their muscular fibre, which latter is induced by long-sustained resistance to the passage of impure blood, manifested clinically by high arterial tension, we ought occasionally to have the vascular change and cardiac hypertrophy independent of, or antecedent to, renal degeneration. In the gouty condition, for example, long recognised as a cause of contracted granular kidney, there is frequently, if not always, increased arterial tension, and, therefore, vascular resistance, with no indication in the urine of kidney changes. It must be remembered that the arterial tension comes and goes, so that it cannot be attributed to hyalin-fibroid degeneration. If, as the views of the authors indicate, the thickening of the walls of the arterioles is degenerative in character, the latter must be converted into unyielding tubes, and it is indeed in this way that the cardiac hypertrophy is said to be induced, and they cannot therefore be influenced by any of the causes which contract or dilate the vessels, and so there can be no variation in the character of the pulse other than as is due to the greater or less frequency and more or less force of the heart's action. What are the facts? Pyrexia is clinically known to relax the arteries and lower vascular tension. It does so in Bright's disease, and the speaker's attention was particularly called to the fact (frequently indeed since) by the character of the pulse and the interesting modifications in the sounds of the heart shown by Dr. Sibson to be due to high arterial tension in a case which the post-mortem examination showed to be one of contracted kidney. He continually made use of the fact in clinical teaching, and, on one occasion, having arrived at the bedside and described the distinguishing peculiarities of the pulse, he found that this had entirely disappeared: the pulse was soft, the vessels relaxed as in ordinary pyrexia, and, in effect, the temperature was high, and the patient passing through an attack of pyrexia, on subsidence of which the arterial tension returned. It is well known that the effect of nitrite of amyl is to relax the arteries and to lower the vascular tension, and Dr. Broadbent experimented with it in a case of Bright's disease. If these vessels were thickened by a degenerated hyalin-fibroid deposit, it is obvious that no result could follow; but the speaker was enabled to exhibit a sphymographic tracing, taken by Dr. Handfield Jones, showing that, whereas before inhalation the trace presented a convex rise, a round summit, a partial fall, and a vibrating slope to the lowest point (unmistakable indications of high arterial tension), after inhalation the height of the trace was more than doubled, the rise was concave, the apex pointed, the fall precipitate, and there was marked dicrotism, and, in fact, the tension was exchanged for complete dilatation. Dr. Broadbent, therefore, felt it impossible to accept the conclusion arrived at by the authors, and his conviction remained unshaken that Dr. Johnson's theory was one of the most comprehensive and interesting links between physiology and medicine.

Dr. George Johnson said that the interest of the subject obviously centred in the question as to what is the nature of the thickening of the walls of the minute arteries in the kidney and in most of the tissues in chronic Bright's disease. He maintained that the thickening was a genuine hypertrophy of the muscular coats of the artery, while the authors denied the existence of hypertrophy, and declared that a hyalin-fibroid degeneration external to the muscular coat constituted the essential change. An appeal to actual specimens must decide the question. He invited attention to those on the table, which had been declared to be examples of hypertrophy by many men, including Dr. Sharpey, Dr. Carpenter, Sir James Paget, and Dr. Rutherford. He was surprised to find that the authors had not produced their specimens, and he maintained that degeneration of the muscular coat of an artery must impair its contractile power, and therefore lessen its stop-cock action. Hence the high arterial tension and the cardiac hypertrophy could not be explained on the theory of arterial degeneration. With reference to this point, Dr. Broadbent's experiment with nitrite of amyl was of great interest. He had specimens of arteries whose walls had undergone the so-called waxy degeneration, showing the canals irregularly dilated in consequence of their diminished power of resisting the blood-pressure. In opposition to Sir W. Gull and Dr. Sutton, he stated that the renal degeneration commences in the glandular structures, and not in the arteries; and that, with the exception of the Malpighian capillaries, the walls of the capillaries in all the tissues were entirely unaffected, and the walls of the veins also were free from structural change. Hence the statement that the area of the morbid change is that of the area vasculosa is inaccurate. The same hypertrophy of the arterial walls occurs in the large white as in the contracted kidney, and he failed to see how the authors explain this in their theory that atrophy of tissues results from the primary vascular changes. The authors gave no explanation of the assumed primary degeneration of the arteries, whereas the speaker's explanation of the various forms of renal disease, with the resulting cardiac and arterial hypertrophy, was physiologically complete and intelligible. In the absence of specimens, it was difficult to give any positive opinion as to the nature of the so-called hyalin-fibroid degeneration described by the authors; but judging from the description of the change as occurring in a hyalin form external to the muscular coat of the artery, and from the appearance of the drawings, he ventured to suggest that they described as pathological that swollen and translucent appearance of the tunica adventitia so commonly produced by the imbibition of fluid, especially when that fluid is mixed with glycerine or dilute acetic acid. This appearance is readily produced in the normal arteries of the pia mater, and may be seen in the minute arteries of the skin and mucous membranes. If these were not the appearances described and figured in the hyalin-fibroid degeneration, the speaker could not say to what the description applied until he had examined the authors' specimens.

Dr. Silver remarked that undoubtedly there were other modes of origin of contracted kidney, connected with the tubular epithelium and the intertubular connective tissue. But with relation to the atrophy, the symptoms produced were excess of urine of low specific gravity, and containing little albumen, and he thought that the theory, especially as it applied to the heart, did not help to explain the matter. As to the urine, it was peculiar, being neither blood, serum, nor urine, and while it contained little of the elements of urine it also contained little albumen. The filtering apparatus remained tolerably sound, otherwise more albumen would be present; but the amount of fluid filtered through served to show that the process went on under great blood-pressure, although enough urea was not got rid of. So the patient had little risk of dropsy, but great risk of uræmic convulsions, or of inflammation of the serous membranes. These probabilities showed that urea was formed, and it was known that crystals of urea had been seen on the skin of a patient in an advanced stage of the disease. Life, however, could not be maintained unless the urea was carried off. It was necessary, therefore, to increase the pressure, and the hypertrophied left ventricle showed that this was done; and as the speaker did not believe that the hypertrophy was immediately due to the contracted kidney, so also he could not believe that it was due to the presence of any irritant in the blood. This was not the hypertrophy of a damaged organ, but hypertrophy due to blood under higher pressure. To increase tension resistance in the arteries was necessary, and he suggested that that might explain the muscular thickening of the vessels. So he had given ergot in a case of chronic Bright's disease, which, as is well known, would act on the muscular coat of the vessels, cause them to contract, and so increase the pressure of blood in the kidney. The result was that the patient's urine increased from twelve to upwards of one hundred ounces, but in other cases the increase was less marked. He doubted whether, on the whole, this new theory helped much to elucidate the phenomena of the disease.

Mr. Brudenell Carter said the authors of the paper had referred to albuminuric retinitis, and that the history of this condition was calculated to support their general conclusions. The changes in the retina were once supposed to be pathognomonic of albuminuria, but it was now known that they might be present without that symptom. Again, there was no constant relation between the retinal and the renal affections. In many cases of Bright's disease the retina escaped for a long period; in some it escaped altogether. In many others, on the contrary, the troubled retina was the cause of the patient's seeking advice; and the ophthalmoscopic examination first led to the discovery of albumen in the urine. The retinal changes, again, differed greatly in different instances—the white patches sometimes preceding, sometimes following, the characteristic hæmorrhages. On the whole, the weight of evidence was opposed to the supposition that the retinal was an effect of the renal disorder, and was in favor of the supposition that both were the effects of some more general cause, and that each, for a certain time at least, might exist independently of the other.

Dr. Anstie wished to ask Dr. Broadbent whether he was aware of the extent to which the muscular tissue of the arterioles remained sufficiently intact to be sensible of the action of nitrite of amyl, even in very advanced states of degeneration. He (Dr. Anstie) had had occasion to try the effects of amyl in many patients in whom the arterioles must have been extremely degenerated by the effects of old age; nevertheless, the usual sudden dilatation was well marked—indeed, alarmingly so.

Mr. Hulke said that he had found in the retina (associated with albuminuria) true muscular hypertrophy of the arteries, and also true sclerosis—two conditions perfectly distinct, but sometimes commingled—i. e., hypertrophy followed by sclerosis. He asked what relation the authors had found between the arterial changes and the grave alterations in the nervous tissue characterising (in kidney disease) the albuminuric retinitis.

Dr. Rutherford stated his conviction that an examination of the microscopic specimens exhibited by Dr. Johnson was sufficient to satisfy any impartial observer that there is a positive increase in the number of muscular fibres in the walls of the vessel. He regretted the absence of microscopic specimens in connexion with the authors' paper; but observed that it was competent for them to point out in Dr. Johnson's preparations the appearances indicative of hyalin-fibroid as well as the results of the so-called capillary fibrosis.

Dr. Symes Thompson briefly subscribed his belief in Dr. Johnson's views.

Sir William Gull, in replying, remarked that the subject was important as to its bearing upon mortality at a certain period of life. The state called chronic Bright's disease with contracted kidney was a mode of death comprising a large percentage of cases, especially in males, after the middle period of life. When visiting Bath a few years ago the attention of the speaker was directed to the tablets on the walls of the Abbey Church, recording the deaths of persons who had come from many parts of the kingdom for the benefit of the waters. Deaths recorded on the tablets near him ranged from fifty-seven to sixty-three, and from later knowledge he did not doubt that nearly all these patients had gone to Bath for what is commonly termed latent or suppressed gout, or, in other words, for symptoms depending upon the state of cardio-vascular degeneration then under discussion. Hence the Bath waters had been humorously characterised as well calculated to lay the dust. But the object of the paper was important in a therapeutic sense. The somewhat antithetical views set forth in the paper deserved, and must receive, the fullest discussion, whether the affection began, according to former pathologists, locally in the kidney, and so, by extension of morbid influence through the blood, producing a change in the heart and arteries, or whether the disease had, as it was now attempted to show, a more general beginning in the arterioles, the kidneys becoming contracted as a part of the general morbid change. Dr. Broadbent's argument that the cardiac and vascular changes might be limited to one form of renal disease, and not producible by other forms of kidney degeneration, was at present an hypothesis only, and not supported by the fact that extreme contraction and degeneration of the kidneys in young persons did not produce the cardiac and vascular changes which on such supposition should occur with it. Dr. Bastian and Dr. Dickinson gave clinical evidence supporting the views advocated in the paper; the former pointedly stated that dyspnœa was one of the early symptoms, and the latter said that in the early stage of the disease the urine is free both from albumen and casts. The clinical history of chronic Bright's disease, though not included in this paper, seemed distinctly to show that the renal affection may be developed at different stages of the disease. Sir William Gull concluded by saying that it had long been a dream of the authors of this paper that the lines of pathological change in later life, so far as they were essential in contradistinction to merely accidental influences, would be found to correspond with the lines of early development. Chronic Bright's disease with contracted kidney appeared to the writers of the paper to belong to the area vasculosa. Had there been time, it might have been possible to point out how the mucous layer and skin had its senile pathology corresponding to the mucous layer of the embryo, and so probably had the nervous layer. He also wished emphatically to record that his colleague, Dr. Sutton, must properly claim whatever credit was due with reference to the morbid anatomy of the subject.

Dr. Sutton, in reply, said that he should be glad to show the specimens, and would have done so before the meeting had he been apprised of Dr. Johnson's demonstrations. He did not agree with Dr. Johnson's opinion that the changes observed in the vessels were dependent on some incidental and not upon a diseased condition, for a great number of specimens prepared in the same manner had been examined, and the vessels taken from persons accidently killed, or who had died from tuberculosis, fever, or other acute diseases, did not present this hyalin-fibroid appearance, which showed the condition to be a morbid one. Where the vessels had undergone the hyalin-fibroid change, the kidneys were found diseased, or there were cardiac hypertrophy and the other morbid conditions commonly found in Chronic Bright's disease. He thought that the thickening of the arterioles was not dependent on muscular hypertrophy, and was supported in this view by other observers. Bader and Pagenstecker had studied the changes which the retinal arterioles undergo in Chronic Bright's disease, and had concluded that the alteration was due to fibroid formation in the tunica intima and the tunica adventitia of the vessels.

Editorial.

SIR JAMES COXE ON LUNACY.

Seldom is it that any scientific body opens its session with such a masterly survey of the chief problems before it as the Medico-Psychological Association at its last meeting in Edinburgh. In his introductory address Sir James Coxe touched on the four points in the field of lunacy round which controversy has most actively revolved: What are the causes of insanity? How is its growth to be checked? What are the results of asylum treatment? What can be done by moral and physical training?

Commencing with a criticism of asylum treatment, Sir James Coxe proceeded to estimate its results in checking the growth of insanity. The Lunacy Acts were passed under the belief that by multiplying asylums the number of cures would be increased and the propagation of the malady arrested. That belief, however, has not been justified. Partly through the more frequent occurrence of insanity, partly through the preponderance of the admissions over the discharges and deaths, the number of victims to the malady has risen immensely. If the prolongation of life in these institutions explains the phenomenon, that is no reason why they should be constantly multiplied. But how far have they diminished the disease? Granted that many admissions are discharged recovered, is the result due to the treatment or simply to the recuperative powers of nature? Taken early, the patient certainly does get well much oftener than when admitted at a more confirmed stage. But is the asylum to be credited with the recovery? Would not the patient have had an equally good chance if treated outside the asylum, with change of scene and surroundings, with proper regimen and therapeutics? The question is so difficult to answer that the extension of asylums has yet to justify itself; nay, it can be proved that, admirably as they are conducted, they are sometimes put to an unreasonable use. Detention of patients, in Sir James Coxe's opinion, is prolonged far beyond the stage at which any good can be effected. The treatment having hopelessly failed, treatment elsewhere, though possibly efficacious, is rarely thought of. Again, asylums are said to improve and humanise their inmates; but do they not as often degrade and deteriorate? There is much in their routine to confirm the malady; there is much in ordinary life, with its variety of incident, to restore healthy action. Sir James Coxe, indeed, does not shrink from saying that he has seen recovery produced by home treatment where incurability would have followed removal to an asylum. Beyond its power of control and isolation, that institution has no value for Sir James Coxe; while the experience of its superintendent in dealing with the insane has its bad as well as its good side—its tendency to foster belief in exceptional measures and inspire unwarrantable self-confidence. Recovery from insanity is far less due to the genius of the prescriber than to the observance of broad rules of hygiene. Purgatives, hypnotics, anodynes, and tonics are useful auxiliaries; "but a comfortable meal is the best of sedatives, and abundance of exercise the best of hypnotics."

Deterioration of the body rather than an exclusive affection of the nervous system is the main cause of insanity. Insanity from sunstroke, from tumours in the brain, is exceptional and generally incurable. The malady has, *κατ' ἐξοχήν*, been ascribed to the forces of civilised life—to the tear and wear of competition. But this induction Sir James Coxe regards as untenable. It is among the lower and ill-conditioned orders, not among the more refined and better cared for, that insanity swells its numbers. It is the pauper lunatic that creates the demand for asylum-extension. Dissipation in its various forms, overwork, meagre fare, lack of ventilation, neglect of moral culture—the forces adverse, not auxiliary, to civilisation, in a word, are the chief factors in the production of insanity. Emotional excitement among the upper classes is a more frequent cause of the malady than among the lower; but even in this case physical deterioration is at the root of it. "There is no disease less likely to supervene in a healthy constitution than insanity." Even where insanity occurs, as in delusion, when no physical disease exists, yet deviation from healthy action, either in the patient himself or in his ancestors, must have preceded the delusion. For the cure of such a patient the proper course is not to argue with him, but to improve his bodily health. His mind strengthens with his physical state, and the delusion disappears as the power of rightly comparing things and estimating their relations is recovered. In all its phases, in fact, insanity originates in some form of bodily disease; and its main preventive is the preservation of bodily health. How is this to be achieved on that national scale which can alone affect the growth of insanity? By education; but education in a far more catholic sense than it has hitherto borne. Religious dogmas which enter so largely into public teaching may point the way to salvation, but "they do little or nothing to incline our hearts to keep the moral laws." If among the upper classes the non-observance of these, with the neglect of physical training, impairs bodily health and issues in insanity, how much more must not the same evil result prevail among the lower, whose neglect of moral and physical laws, whose non-education, in short, is so notoriously general. Let us adopt other tactics against the multiplying malady. Not by waiting till insanity has been produced and then struggling to cure it, but by stepping in before the mischief has been done, shall we arrest its growth. We have an ever-growing number of lunatics, for whom the nation is subjected to an ever-growing tax. Why should not the money, now consumed by thousands of these useless beings, be spent in maintaining their health and enabling them to mingle with vigor and effect in the affairs of life? Hygiene languishes because of the ignorance of the people. We preach the necessity of pure air to hearers who know nothing of the laws of respiration. Let every member of the common herd be taught in what manner and on what grounds he should so regulate his life as to keep his health and do his duty to his neighbor and himself. His accredited teachers, however, are themselves too often ignorant of the importance of hygienic training; and legislative enactment is resorted to for the counteraction of his evil habits and the rectifying of his conduct. But in spite of all this curtailment of the liberty of the subject, drunkenness, pauperism, lunacy, and crime increase. Drunkenness, indeed, is proposed to be remedied by the forcible detention of the drinker; while the means by which that vice has within half a century disappeared from the upper classes—education, refinement, higher fields of enjoyment,—are withheld from the lower, with what results, we know!

Ignorance of the human organism, with the laws which regulate it, lies at the root of all the distempers of society; and till this is replaced by knowledge, the reports of the Registrar-General will continue to show an undiminished death-rate, and those of the Lunacy Commissioners an increase of insanity. Even our own profession, with too few exceptions, directs its activity more to the restoration of health than to the prevention of disease; while from the teacher and the clergyman up to the legislator little or no intelligent appreciation of the bane and its antidote inspires their work. This, the thesis we have often maintained, is again reinforced by the high authority of Sir James Coxe, who speaks from an official coign of vantage and from the experience of more than forty years. Self-knowledge, moral and physical, in substitution for the excess of intellectual training now prevalent; bodily training, not in the sense of the athlete only, but that modified army discipline which develops the constitution while teaching masculine accomplishments,—such are the remedies in which, if early enforced and diffused through all the people, Sir James Coxe sees our best escape from the disorders of society. So far from being a "disease of civilisation," insanity is a disease of ignorance; and the only way to check its growth is to let every man know the fabric and functions of his own body, and the relations in which he stands to the surrounding world, moral and physical. No class of men appreciate this more than our own profession; but our mode of dealing with the problem—admirable, so far as it is seen, in the regulation of our asylums—is too restricted. It is for our medico-psychologists to impress these facts upon the Legislature, and for the profession at large to prepare society for legislative action.

GRANULAR DEGENERATION OF THE KIDNEY.

The paper by Sir Wm. Gull and Dr. Sutton, reported in our present number, deserves notice as possibly marking a real advance. It has long been known that granular degeneration of the kidney is associated with changes in the vascular system. Bright recognised hypertrophy of the heart as a result of renal disease, and knew also that the arteries of diseased kidneys were sometimes abnormally thickened. Subsequent observations have shown that arterial deterioration of one sort or another is widely associated with renal change. The infiltrating matter of lardaceous disease makes an early appearance in connexion with the coats of the smaller arteries, thence spreading into the neighboring tissues. And granular degeneration, as distinguished from the other types of renal disorder, has a baleful association with vascular decay. Arterial degeneration and consequent hæmorrhagic accidents have been already shown to belong especially to this form of disease.

So far we are treading on undisputed ground. Passing thence to the territory of the recent discussion, we may first limit our glance to the kidney itself, within whose narrow bounds two rival pathologists contend. Dr. George Johnson long ago described the granular kidney as the result of "chronic desquamative nephritis," attended with a crumbling away of the epithelium and consequent falling together of the rest of the gland. The crumbling, however, as a morbid process, eluded the search of other pathologists, and those who walked by sight and not by faith were still in doubt as to the mechanism of the granular kidney. According to the antagonistic view, which was propounded by Dr. Dickinson, the granulation is caused by changes, not in, but between the tubes; a new growth of fibroid tissue in this situation encroaching upon the glandular elements, contracting like a cicatrix, and drawing down the surface at its points of attachment after the manner in which cirrhosis of the liver is produced.

Distant and scattered arteries next send back the echoes of battle. Dr. Johnson drew attention to the fact that in connexion with chronic Bright's disease there is a general thickening of the walls of the smaller arteries. This thickening he regarded as a muscular hypertrophy; and proceeded to elaborate its pathology in ingenious harmony with his views as to the action of the pulmonary blood-vessels in cholera. According to this view the arteries become hypertrophied in their efforts to oppose the action of the heart, while the heart becomes hypertrophied in its attempts to overcome the opposition of the arteries. The kingdom is divided against itself, and an internecine war rages on every stream. The cause of the contest is the impurity of the blood, which the arteries by an inhibitory or stop-cock action strive to keep out of the tissues, for the nutrition of which it is unfit. The heart makes violent and inconsiderate efforts to overcome the obstacle. This view, which presents Nature in a somewhat inconsistent attitude, has been shaken by subsequent observations. Dr. Lionel Beale stated that the thickened vessels, more especially in the kidney, were not simply magnified in their muscular coats, but were changed in other respects. They were roughened and corrugated inside and out. Their walls were increased by an accumulation of fibrous tissue outside the muscular coat, and the muscular coat, instead of being hypertrophied, was truly atrophied, the contractile tissue being largely replaced by uncontractile fibrous growth. The researches of Sir W. Gull and Dr. Sutton confirm this view, and give it a wider application. They maintain with Beale that the thickening of the small arteries is not strictly hypertrophy, but is due to a morbid production of "hyalin-fibroid" tissue which encroaches upon and replaces the normal structures. They proceed to confirm the intertubular view of the origin of the disease, and hold that the new fibroid growth by which the kidney is destroyed is a formation analogous to that which thickens and narrows the vessels. They point out, consistently with our own observations, that the new growth commences about the vascular structures of the gland. Thus, in granular degeneration of the kidney, they recognise only one manifestation of a general disease, whose range is as wide as the course of the arteries. According to this view the change may begin in the kidney or elsewhere; it may progress in other parts of the body while the kidneys are free; it is as much a general disease as atheroma or lardaceous infiltration, and belongs to the whole frame, not to a particular organ.

While we are prepared to concur with many of the opinions expressed by the authors of the paper, we must suspend our judgment as to the origin of the disease in the general arterial system. The discussion has shown that, though the existence of the arterial thickening is undisputed, there is as yet no consent as to its nature. The simple observations needed to decide the question cannot be long wanting. For our own part, we cannot doubt that the coats are altered in structure as well as in bulk. They certainly break with abnormal facility. Whatever be the nature of the change, it acknowledges no relationship with any but the granular kidney. Whether the renal lesion be the progenitor or only the companion of the vascular is a question which future observations must decide.

Cirrhosis of the liver is closely analogous to granular degeneration of the kidney. The new growth of fibroid tissue between the glandular elements is probably, like that which occurs in the kidney, in close relation to the vessels of the organ. If the renal disease be but part of a general vascular change, its hepatic analogue has probably the same relation to the arterial system. If this be so, hypertrophy of the heart should accompany the hepatic as constantly as the renal disorder —an association which, if it exist, has hitherto escaped notice.

Again, it is improbable that the vascular changes —allied, as the authors say, to senile deterioration— should occur in childhood, except as the result of antecedent disease. Sir Wm. Gull, indeed, expressed an opinion that children, however their kidneys may be affected, are generally exempt from such changes. An example to the contrary, however, is mentioned in the paper, and another fell under our own notice. A child twelve years of age died of sanguineous apoplexy, associated with extreme granular contraction of the kidney. The arteries in the kidney and pia mater were marked instances of the change under discussion. No doubt it is open to us to explain the arterial change, however early, as primary—a kind of senility in youth; but it would seem to consort better with our present knowlege to look upon the general as consequent upon the local disease—visceral changes of the fibroid type, though most frequent in advancing years, occurring sometimes at so early a period of life that we are inclined rather to assign them to causes directed solely upon the organ affected than to suppose them coeval with a general arterial change by which the whole body is blasted with antiquity.

Thus, while allowing many of the grounds on which the authors base their view, we are constrained to remit their general conclusion to the verdict of the future.

RICORD ON SYPHILIS.

The emphatic testimony borne by M. Ricord to the value of mercury in syphilis, and his lucid exposition of the principles which should govern its administration, will come to the profession, not only of England but of the whole civilised world, with a weight which no other living man could impart to his utterances upon the subject. A few years ago we had amongst us a small number of eccentric practitioners, or rather, perhaps, of eccentric writers, who professed to be able to treat syphilis successfully without mercury. Of these, some have passed away, others have lived to acknowledge the erroneous character of their earlier teaching, and only a very few remain steadfast to their faith—resting it, for the most part, upon the tendency of some particular local manifestation of the malady to exhaust itself under favorable circumstances, and to terminate in a species of recovery by virtue of the lapse of time. Syphilitic iritis will often subside in this manner, if occlusion of the pupil be prevented by the free use of atropine, and if the patient be well nourished, tended, and cared for. Those, however, who have to do with syphilis as a whole are generally fully conscious of the necessity of administering mercury; but we doubt if the profession generally has quite so clear a view upon the question as that which M. Ricord has striven to impart. The doctrine that mercury should be given so long as primary or secondary symptoms continue, that iodine should be used for tertiary symptoms, and mercury and iodine together when secondary and tertiary symptoms coexist, although very generally recognised and acted upon, has seldom been so well and so concisely stated. But that which gives an altogether exceptional value to M. Ricord's words is his emphatic statement, based upon an experience of unrivalled extent and of forty years' duration, that the treatment of primary syphilis, to be successful, must be continued, without reference to symptoms, for at least a year. Many surgeons of less position and authority would shrink from declaring to a patient, in the first instance, that so long a period would be necessary; and many patients would take alarm at the intelligence, unless it came to them from one who commanded the most implicit confidence. There is one practical point on which M. Ricord was mute, and on which an expression of opinion from him would have been highly valuable. Like all other practitioners, he prefers any other channel for the administration of mercury to the stomach; and he laments over the dirtiness and inconvenience of inunction, while he declares its efficacy. Now, it is well known that Mr. Henry Lee has long advocated the use of mercurial vapor baths as a means of the prompt and convenient introduction of mercury through the skin, and that he claims for these baths all the advantages of inunction, with immunity from nearly all of its unpleasantness. It would have been very interesting to hear whether M. Ricord had employed the baths, and whether he believed them trustworthy. It would have been interesting also to hear his experience of the relative toleration of the different preparations of mercury by different people, and to be informed of the cases in which he had found any form of the drug difficult to administer or to control.

The nominal occasion of M. Ricord's visit was the meeting of the British Medical Association at Birmingham; but the real occasion was the reading of a paper by his friend and former pupil, Mr. Acton, before a medical assembly in this country. M. Ricord has long ceased to teach in public or to write on professional subjects, and it was highly important that his latest and most matured views concerning syphilis should be given to the world. In calling forth these views Mr. Acton has done signal service; and M. Ricord, who, at his advanced period of life, and in the midst of his overwhelming engagements, has found time to make a hurried and fatiguing journey to England, has shown himself now, as ever, true alike to the duties of friendship and to the demands of science.

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ORIGIN OF FAT.

The question of the origin of the fat present in every well-nourished person is not easily answered. Putting aside the debated point of the direct conversion of the carbo-hydrates into fat, there are three theories that have obtained a certain amount of credit. The first—resting on the fact that, with free supply of oleaginous food, an abundant deposit of fat takes place in the subcutaneous connective tissue, in the omentum around the kidneys, and elsewhere—maintains that the fat enters the economy in the ordinary manner, and is directly stored up in the interior of the fat-cells. This view entirely ignores the mechanical and physiological difficulty of the passage of an insoluble substance through the cell-walls. The second view rests on the now well-ascertained fact that albumen and its analogues are capable of undergoing disintegration, in the course of which, besides other substances, fat is certainly formed. Nothing can demonstrate this more clearly than the growth of the eggs of the common bluebottle on meat or in defibrinated blood. It is therefore maintained by those who hold this view, as Toldt and Subbotin, that the cells originally take up soluble albuminous compounds, which, undergoing disintegration within the cells, split into certain compounds that readily escape from them, and fat, which remains behind. These authors hold that all the fat of the food is consumed or burnt off during its passage through the economy, and is only exceptionally applied to increase the natural stores present in the body. A third view, to a certain extent intermediate to the other two, is held by Radziejewski, who maintains that an important proportion of the fat of the food is converted into soap by the pancreatic juice and the alkali of the biliary acids, and in this state easily traverses the intestinal epithelium, as well as the membranes of the proper fat-cells. These cells, again, have the power of converting the soaps into free fat, which remains in their interior. This view is certainly supported by various microchemical and physical researches showing the importance of saponification in facilitating the passage of fats through animal membranes. Radziejewski himself has demonstrated—first, that soda soaps are absorbed in their passage through the intestines; secondly, that soaps made of oils foreign to the economy are deposited but slightly, if at all, in the form in which they are ingested. Whilst the experiments of Matteucci and others, showing the effects of alkalies on the osmosis of fats, are well known.

The whole subject is well reviewed by M. Hofmann in a paper just published in the *Zeitschrift fur Biologie*, giving an account of a series of experiments in which he attempted to ascertain whether any, and if any, how much, fat was stored up from the fat consumed in the food. With this end in view, he starved a dog till it had consumed all, or nearly all, the fat in its body. It was then supplied freely with fat mingled with a small quantity of albumen. After a few days it was killed, and the total amount of fat in the body determined. A certain amount, it is evident, might proceed from the decomposition of the albumen; but if the quantity found exceeded this amount, estimated as highly as possible, it is equally clear that the excess must proceed from the fat consumed as food. This, however, did not prove that the fat was stored up in fat-cells, since it might be contained in the blood; and a special set of researches were instituted to show that the blood, under these circumstances, was not extraordinarily rich in fatty compounds. The general result of M. Hofmann's experiments, as far as they have yet gone, seems to prove definitely that the fat stored up in the body cannot possibly be entirely derived from the albumen consumed as food, but must be in part obtained from the fats. M. Hofmann does not, however, attempt to explain the mode in which the fat gains entrance into the cells of adipose tissue—a point on which we are at present completely in the dark.

Medical Annotations.

"Ne quid nimis."

DR. PETTIGREW ON THE CIRCULATION.

Dr. Pettigrew's seventh lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man" was devoted to the fœtal circulation. The lecturer divided the fœtal circulation into two kinds—1st, the circulation within the body of the fœtus; 2nd, the circulation between the maternal and fœtal portions of the placenta. The fœtal circulation, he remarked, was carried on chiefly by visible forces—viz., the heart and bloodvessels; the placental circulation mainly by invisible forces—viz., absorption, evaporation, osmosis, capillary attraction, chemical affinity, nutrition, &c. He showed that the fœtal circulation was a closed circulation; all the vessels of the fœtus (those of the fœtal portion of the placenta included) being quite distinct from those of the parent; the fœtus in the early stages of pregnancy being nourished by the imbibition of nutritious juices, not necessarily those of blood; and in the later stages, by the more fluid portions of the blood of the mother, which, in the placenta, passes through the walls of the maternal vessels into those of the fœtus by a process of osmosis. The parent gives to the fœtus by endosmose, and takes from it by exosmose, similar remarks holding true of the fœtus. The relation between the mother and fœtus is, therefore, of the most intimate description. The lecturer gave some original views as to the structure of the placenta. He maintained that the constituents of the placenta are essentially those of the external and internal skin. He explained his meaning by stating that the most active parts of plants are the roots and leaves, these drawing nourishment from the ground and air; that animals consist of two parts—an external skin or outer surface, corresponding to the leaves of plants; and an internal skin or mucous lining, which corresponds to the roots of plants. An animal, in fact, is to be regarded as an organism, with an involuted portion corresponding to its alimentary canal. The external and internal skin are consequently continuous without and within the body, and have a community of structure. The lecturer dwelt upon this community of structure, and showed that the outer and inner skin were composed of epithelium of a basement membrane, of capillary vessels and innumerable ductless glands—the elements, in fact, of which the placenta is composed. He demonstrated that the external and internal skin presented little elevations (papillæ); that each eminence, whether of external skin or mucous lining, was covered by a layer of epithelium, had a basement membrane, a capillary tuft of bloodvessels (arteries and veins), and numerous glands. He contrasted the villous eminences of the external skin with those found on the internal skin—e.g., the villosities of the stomach, colon, uterus, &c. They were found to agree in a remarkable manner. He observed that if two such surfaces were placed in apposition, a structure closely resembling, if not identical with, the placenta would be at once produced. This view, he remarked, was favored by development and comparative anatomy. The maternal and fœtal vessels in the cotyledon of the ox, he explained, formed pyramidal-shaped masses, which readily separated. The separation is facilitated by a thin layer of cotyledonous milk, found between the individual vessels, this inaugurating and promoting osmosis between the vessels of the parent and its offspring. The same thing substantially happens in ourselves. The maternal and fœtal villous tufts or capillary vessels interdigitate in the placental area, as the fingers of one hand may be made to pass through those of the other. This brings the ultimate radicles of the vessels of the mother and fœtus into the most accurate apposition possible, the two sets of vessels abutting against each other throughout large spaces, and being only separated from each other by the extremely thin walls of the vessels, their appropriate coverings and a thin layer of utricular substance (the secretion of the utricular glands), which the lecturer believes is absolutely necessary to an effectual exchange of nutrient and effete materials between mother and fœtus. The lecturer dissents from the commonly received opinion that the bloodvessels of the mother expand and cover those of the fœtus with a sac-like investment. He further doubts the existence of venous sinuses in the uterus as receptables for blood, neither of these being found in extra-uterine fœtation. He pointed out that if the relation between the mother and fœtus was such as it is usually described, the injury done to the uterus by the forcible removal of the placenta would be such as to jeopardise the life of the parent by hæmorrhage; whereas, according to his view, the separation would be as natural as the original union. The lecture, which was illustrated with quite a large number of diagrams and specimens, was listened to with marked attention throughout.

In his eighth lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," Dr. Pettigrew explained at considerable length the intimate relation which exists between the lungs and heart—the lungs circulating air and the heart blood. The lungs, he remarked, in their most rudimentary form, are mere sacs, walled off at an early period from the alimentary canal. They retain within certain limits the peculiar rhythmic movements which characterise the intestines. He compared the lungs to the hollow viscera as a class. In the water newt, the lungs consist of a pair of elongated sacs, which have no laminæ or folds in their interior. In the frog internal ridges make their appearance, these increasing the respiratory or aerating surface. In the crocodile and turtle the ridges multiply and divide the interior of the lungs into cells; the differentiation going on in the lungs of birds and of mammals until those structures present a honeycomb appearance. By this means the aerating surface is enormously augmented. The lungs of serpents maintain their orginal tubular form, each terminating in an air sac. The lungs of birds communicate with the interior of the bones, and with as many as fifteen air sacs. These sacs open and close rhythmically, and pass on relays of air at regular intervals. If the humeri or arm-bones of a bird be perforated, its trachea or windpipe may be tied without producing suffocation, respiration going on by the openings in the bones. The air sacs have vessels ramifying on their surface, so that a certain proportion of the blood is aërated in them, as in the lungs themselves. The lecturer drew attention to the arrangement of the great vessels in the reptile, bird, and mammal. He especially directed attention to the large number of valves found in the heart of the bird, several of these being composed of muscular fibres. He believes the complicated and highly differentiated valves occurring in the heart of the bird are necessitated by the very perfect and elaborate respiratory apparatus; capacious lungs and a vigorous circulation being required to maintain the temperature and the muscular energy of birds flying and diving. He described the circulation as it exists in the mammal, and contrasted it with that found in birds. He adverted to the pushing and pulling power exerted by the heart; the ventricles pushing when the auricles are pulling, and *vice versâ*, and gave it as his opinion that, as long as the vessels are full of blood, collapse of their walls is impossible, fluids being very slightly compressible. He contended that, under these circumstances, a sucking power might be exerted either by the auricles or ventricles. He illustrated this by a very simple experiment. He took a large vessel with thin walls and fixed an ordinary syringe at either end of it. This done, he filled the vessel and one of the syringes comfortably with fluid, the other syringe being empty. The one syringe, he observed, represented the right ventricle, the other the right auricle. When the syringe representing the right ventricle was made to discharge its contents into the vessel, that representing the right auricle was made to draw into its interior a corresponding amount; the fluid being transferred from the one syringe to the other without in the slightest degree altering the calibre of the vessel. It is simply a question of supply and demand, and if the supply equals the demand, the vessel may be as thin as tissue-paper and yet have no tendency to collapse. This is precisely what happens in the heart and bloodvessels. The quantity of blood in the vessels of the body is nearly always exactly the same. The cavities of the heart are of the same size, and when the one closes the other opens. The blood ejected by the ventricles makes the circle of the lungs and the body and is received by the auricles, so that the blood is passed on in relays and according to measure. There is therefore no possibility either of confusion or accident. The lecture was illustrated by a large number of diagrams and by specimens.

In his ninth lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," Dr. Pettigrew gave a description of the structure, uses, and movements of the capillaries and smaller vessels of animals; after which he proceeded to a consideration of the larger vessels and the heart, which is to be regarded as an expanded portion of the vascular system. He divided the capillaries into two sets—the lymphatic capillaries and the vascular or sanguineous. He regarded the lymphatic vessels as the feeders of the vascular vessels, in the same sense that the roots of a tree are to be considered as ministering to its general circulation. In this respect the sanguineous circulation is to be looked upon as an evolution or development of the chyliferous. The capillaries of the lymphatic system, so far as known at present, do not form a through route, but begin in the tissues by blind ends resembling the spongioles of the roots of plants. They therefore form a connecting link between the vegetable and animal. The lecturer drew attention to the presence of white corpuscles in the lymphatics, and their subsequent reappearance in the sanguineous capillaries. He also stated that he had distinctly seen the white blood-corpuscles pass through the walls of the sanguineous capillaries, and that they appeared to incorporate themselves with the surrounding tissues. The sanguineous capillaries differ from the smallest arteries and veins in having a simpler structure, being, in fact, composed of a very delicate, structureless membrane, in which oval-shaped nuclei are embedded. As this membrance is freely permeable by liquids, the tissues are by its instrumentality at once irrigated, nourished, and cleansed. The small arteries and veins are composed—first, of a sheath of fibrous tissue, the fibres of which pursue a more or less longitudinal direction; second, of a muscular layer, composed of flattened, spindle-shaped bands, with elongated nuclei, these bands running transversely or across the vessel; and, third, of a layer of very delicate elongated epithelial cells, the oval nuclei of which are arranged longitudinally, or at right angles to those of the muscular fibres. Some of the larger vessels are possessed of longitudinal as well as circular muscular fibres, and to these elastic and fibrous tissue are added in oblique bands. The lecturer attached much importance to the cross arrangement of the elements forming the walls of the arteries and veins, this arrangement enabling the muscular and elastic elements to work harmoniously together when they increase or diminish the calibre of the vessels.

The circular muscular fibres can of themselves increase or diminish the calibre of the vessels. Thus, if heat be applied to a small vessel, it opens or widens, whereas cold causes it to close or narrow. The walls of a vessel thus vibrate on either side of a given line, which correspond to their position of rest. The widening of a vessel is occasioned by an elongation of its muscular fibres, assisted by the pressure of the blood it contains; the narrowing of a vessel is due to a shortening of its muscular fibres. When longitudinal and circular muscular fibres are present in a vessel, and the vessel is to be widened, the circular fibres elongate in the direction of the breadth of the vessel, the longitudinal fibres shortening in the direction of the length of the vessel. The converse happens when the vessel is to be narrowed. When a cavity (say the ventricle of the heart) is to be obliterated, the longitudinal and circular fibres shorten and broaden in every direction; when the cavity is to be increased they lengthen and narrow. This invests muscle with a centripetal and centrifugal action. When a limb is to be flexed the flexor shortens and the extensor elongates. This does away with the necessity of the muscle which shortens forcibly dragging out that which lengthens, which is a mere waste of power. Muscles when not acting enjoy absolute repose, and are not in a state of tonic contraction, as it is termed. This is contrary to the commonly received opinion, but it nevertheless appears to give the true cue to muscular movements, whether voluntary or involuntary. The lecturer stated his belief that the voluntary muscle was a differentiation of the involuntary. When a voluntary muscle shortens (contracts) it swells out in parts, and presents a waved appearance, the sarcous elements or squares of which the muscle is composed changing shape also. Thus in the swelled parts the long axis of the sarcous elements are directed transversely or across the muscle; whereas in the parts between the swellings, the long axis of the sarcous elements are turned in the direction of the length of the muscle. This implies the presence of two forces acting at right angles to each other, and a power inhering in the sarcous elements of muscle of shortening and elongating. The lecturer claimed for the whole muscle and properties which he believes inheres in its sarcous elements. That muscles have the power of elongating is proved, according to him, by the movements of the worm. This animal is invested with a tunic composed of longitudinal and circular muscular fibres. When it moves it pushes out, telescopic fashion, the first inch or so of its body. This part is then shortened, and another inch sent on until the tail is reached and an inch of ground gained. The body of the worm is alternately narrowed and thickened, as in ordinary muscle when in action. The thick portions of the worm may be compared to the bodies of hollow viscera, the narrow portions to their sphincters. When the thick portions become narrow to expel their contents, the thin portions, which represent the sphincters, widen, and allow the contents to escape. The lecture was illustrated by a large number of original diagrams.

In his tenth lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," Dr. Pettigrew drew a parallel between the circulation of air in the chest and lungs and the circulation of blood in the heart and vessels. He gave it as his opinion that the apparatus by which the air and blood are circulated is essentially the same, both as to its mechanism and movements. He stated that if the element of bone is eliminated from the chest, the direction of the muscles composing the walls thereof correspond exactly with the muscles composing the walls of the heart (especially the ventricles), stomach, bladder, and uterus. Thus the muscles composing the walls of the thorax are disposed spirally, in vertical, slightly oblique, oblique, and transverse lines, and arranged to antagonise each other; precisely similar remarks holding true of the muscles of the hollow viscera, the diaphragm, and the muscles of the extremities. He regards the diaphragm as equal to half a heart, or half of any of the other hollow muscles. This is an entirely new view, and opens up a wide field for investigation. He pointed out that if a cast is taken of the left ventricle of the heart, the cast is of a spiral form. By comparing the cast with the bones of the anterior extremity of the elephant, or the wing of the bird, he showed that the same spiral form reappears. From this it follows that the muscles of the left ventricle are disposed around the ventricular cavity in the same manner that the muscles of the fore limb are disposed around its bones, or the muscles of the chest around the thoracic cavity. The lecturer stated that the muscles of the hollow viscera ought to take precedence of the muscles of the thorax, and that the muscles of the thorax ought to take precedence of the muscles of the extremities; for the obvious reason that the hollow muscles exist before there is a chest proper, the trunk being found without the limbs, which are a further differentiation. The analogy between the chest and heart becomes very obvious if the diaphragm be regarded as a movable partition which separates the chest from the abdomen, as the valves of the heart separate the auricles from the ventricles. When the diaphragm shortens in all its diameters, and flattens, it enlarges the floor of the chest; whereas, when it lengthens in all its diameters, and arches in an upward direction, it diminishes the floor of the chest. The diaphragm ascends when the ribs descend, and *vice versâ*, the ascent of the diaphragm and the descent of the ribs diminishing the capacity of the chest, and increasing that of the abdomen; the descent of the diaphragm, and the ascent of the ribs, increasing the capacity of the chest, and diminishing that of the abdomen. This brings out a very interesting point—viz., that the chest and abdomen open and close alternately, precisely in the same manner as the auricles and ventricles of the heart; the chest opening when the abdomen closes, and *vice versâ;* the chest pumping and sucking air, the heart pumping and sucking blood. When the chest expands and the abdomen closes, a wave of motion passes from the symphysis pubis in the direction of the ensiform cartilage, a reverse wave passing from above downwards when the abdomen expands and the chest closes. The lines representing the movements cross each other figure-of-8 fashion. This is quite an original explanation of those movements. Hitherto it has been supposed that in inspiration the anterior wall of the chest and abdomen are both pushed outwards, and that in expiration both are drawn inwards. It appears from the explanation given by Dr. Pettigrew that in inspiration the anterior wall of the chest is pushed out and that of the abdomen drawn in, these movements being reversed in expiration. Another point of great interest in the lecture was the explanation given of the circulation and diffusion of the oxygen of the atmosphere and the carbonic acid within the lungs. The chest and lungs supply air in the same way that the heart and bloodvessels supply blood. Once, however, the air is inside the lungs, the ciliated epithelium which lines the air-passages creates an outward and inward current, the strength of these currents being increased by the tendency which the oxygen of the atmosphere has to mingle with the carbonic acid given off by the lungs. There is therefore what may be regarded as a visible and invisible circulation of air, as there is a visible and invisible circulation of blood and other nutritious juices. The lecture was illustrated by a large number of diagrams and models, and by Dr. Pettigrew's famous dissections of the heart.

The eleventh lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man," was devoted by Dr. Pettigrew to the great vessels and valves concerned in the circulation of animals. The lecturer described the structure and uses of the veins and arteries, and the relation which the valves bear to them and to the heart. He also described the structure and uses of the valves, and showed that in proportion as the circulatory apparatus of an animal becomes complicated, so the valves become more and more differentiated. The object of the valves in every instance is to cause the blood to travel in a circle, and always in the same direction. When the valves are diseased, particularly those of the heart, they become incompetent—i. e., they permit the blood to travel in a wrong direction, which is always a serious matter. The valves, the lecturer observed, may consist of only one segment, as in the smaller veins, or of thirty-six segments, as in the bulbus arteriosus of the American devil-fish (*Cephalopterus giorna*). The valves are formed on a common type. They are all more or less semilunar in shape. In the veins the valves consist of from one to four segments; in the arteries, where they join the heart, usually of three; in the heart (between the auricles and ventricles), of two or three segments, to which as many accessory segments may be added. The segments may or may not be provided with tendinous cords (chordæ tendineæ). When tendinous cords are present, they are to prevent eversion of the segments when pressure is applied to the blood for the purpose of sending it forward. The tendinous cords are only present when the valves are surrounded by, or in some way placed under, the influence of muscles which elongate and shorten or open and close. The tendinous cords first make their appearance in the bulbus arteriosus of the fish, which is a muscular structure which opens and closes like the heart itself. They acquire their fullest development in the mitral and tricuspid valves of the mammalian heart. Between the tendinous cords as they exist in the fish and as finally developed in the mammal there is an infinite variety; but in every instance—and this is the important point—the valves are geared by them to living and moving muscular tissue, which regulates the amount of movement occurring in the valves. Some valves regulate their own movements, and are for this purpose partly or wholly muscular—as, for example, the right auriculo-ventricular valve of the bird. The movements of the venous and arterial valves are essentially mechanical, the segments of the valves being placed obliquely in the vessel, so that they permit the blood to pass through, but prevent its return. The movements of the mitral and tricuspid valves, placed between the auricles and ventricles of the heart, are vito-mechanical. This is necessary, because they are placed between actively contracting and expanding structures (the auricles and ventricles), and because the apertures which they guard are widened when the heart opens, and narrowed when it closes. The lecturer gave a very beautiful and novel explanation of the manner in which the mitral and tricuspid valves act. He stated that the segments of these valves are geared by the tendinous cords referred to above to fleshy columns (musculi papillares) situated in the interior of the heart. These columns, when the heart expands to receive its blood, elongate, and permit the segments of the valves to float upon its surface, as the leaves of water-lilies on water. The moment the heart (ventricles) closes, the valves are shut, regurgitation being rendered impossible. As the closure becomes tighter and tighter, the fleshy columns shorten and drag the segments of the valves in a downward direction, until they form a dependent cone, which has the effect of diminishing the heart at its base, until literally no blood remains in the heart. The mitral and tricuspid valves, therefore, rise and fall (like the diaphragm in the chest) when the heart opens and closes. Another point of peculiar interest was the form of the fleshy columns and ventricular cavities. These, the lecturer observed, were twisted to form conical screws. As the fleshy spiral columns project into the spiral ventricular cavities, it follows that between the columns there are two spiral depressions. These spiral depressions run from the apex of the heart to the bases of the segments of the mitral and tricuspid valves. They are filled with blood when the heart opens; and when it closes, the blood which they contain, and which is arranged in two spiral fluid columns, is forced in an upward direction in such a manner as to cause the segments of the mitral and tricuspid valves to rotate slightly, and screw and wedge into each other. When the valves are screwed home, the aortic and pulmonic valves are unscrewed and the blood projected along the main vessels, very much as a bullet is projected from a Minie rifle. The heart literally wrings out its blood by a screwing movement, and in this movement the valves situated at the base of the heart participate. The heart is screwed home during the systole and unscrewed during the diastole. Dr. Pettigrew bases these statements on actual dissections. The screwing movements of the heart can be seen during life; and the lecturer performed an experiment with a fresh heart, to show the screwing and unscrewing movement of the valves. The lecture was illustrated with a number of original models and by a large number of diagrams.

The concluding lecture on "The Physiology of the Circulation in Plants, in the Lower Animals, and in Man" was devoted by Dr. Pettigrew to a consideration of the movements and sounds produced by the heart and valves when acting, and to the probable causes of those sounds and movements. This afforded the lecturer an opportunity of showing his very exquisite and unique dissections of the ganglia and nerves of the heart, which he described at considerable length. The lecturer began by stating the order in which the apparatus concerned in the visible circulation opened and closed in parts to effect this object. In the cold-blooded animals the large veins (even to the venæ hepaticæ) close first, then the auricle or auricles, as the case may be, then the ventricle, and lastly the bulbus arteriosus. In the warm-blooded animals the terminations of the pulmonary veins and cavæ (superior and inferior) close first, then the auricles, then the ventricles, and then the large vessels in the vicinity of the heart—the large vessels, as he pointed out, being furnished with an elaborate plexus of nerves. The blood is, as it were, manipulated while it is being transmitted. Thus it is drawn into and seized by the large veins leading to the heart, then by the auricles, then by the ventricles, and then by the large vessels leading from the heart. No sooner, however, is it seized than it is dismissed, the seizure and the dismissal being alike necessary to the circulation. The blood is not permitted to wander about at pleasure in the cavities of the circulating apparatus; on the contrary, it is made to flow in a continuous onward stream—nay, more, it is made to open and close the doors which lead to and conduct from the chambers through which it passes. One part or other of the heart is always moving. There is no pause in the heart's action corresponding to the sounds of the heart described in books. The one movement glides into the other as a snake glides into the grass; all that the eye can detect is a quickening of the gliding movements at stated and very short intervals. A careful examination of the sounds of the heart shows that the sounds, like the movements glide into each other. If, however, the heart is acting more or less vigorously as a whole, the question which naturally presents itself is—How is the heart rested? The lecturer gave it as his opinion that it rests as it acts—viz., in parts. The centripetal and centrifugal movements pass through the different parts of the heart very much as the wind passes through leaves; its particles are stirred in rapid succession, but never exactly at the same instant. The heart is moving as a whole, but its particles are only moving at regular and stated intervals; the periods of repose, there is every reason to believe, greatly exceeding the periods of activity. The nourishment, life, and movements of the heart are in this sense synonymous. The various parts of the heart—viz., the muscular fibres, valves, and even the blood which the heart contains—produce sounds when the heart is moving; and these run into each other and echo, or repeat themselves in such a manner as makes it exceedingly difficult to determine what parts or substance produce certain sounds. The susurrus (i.e., the sound produced by the contraction of the muscular fibres) and the rushing of the fluid blood against the valves and into the cavities and vessels of the heart constitute the major factors. The lecturer expressed his belief that the power of opening and closing in its several parts inheres in the heart itself, and is not due to the impinging of the blood against the lining membrane of the heart, the blood acting as a stimulus. He argued against the stimulus theory, on the ground that, if the blood acted as stimulus, it would cause the heart to contract before it had received its full quantity of blood, which it did not. He inferred that if the contents of the heart and the other hollow viscera acted as irritating stimuli, the hollow organs as a class could not consistently be regarded as proper receptacles for either dead or living matter. There is, the lecturer observed, a certain connexion between the brain and the heart, as we all feel when we receive a sudden piece of intelligence, good or bad; but the brain and spinal cord may be crushed in hybernating animals without interfering in the slightest with the heart's action, so that either the heart moves in virtue of a power inhering in muscular substance, or in the nerves and ganglia which are so plentifully distributed thereto. The lecturer drew attention to masses of unipolar and bipolar nerve cells which he found in the ganglia of the heart, these in some instances being arranged in terminal clumps like grapes upon their stems. The lecture was illustrated by a large number of diagrams, by experiments, and by Dr. Pettigrew's elaborate nerve dissections already referred to.

At the close of the lecture the President of the Royal College of Surgeons, in the name of the College, moved a cordial vote of thanks to Dr. Pettigrew "for his very admirable, interesting, and original course." The vote was seconded by the ex-President of the Royal College of Physicians, and enthusiastically passed.

We are glad to find that these lectures have been numerously attended by professors, lecturers, practitioners, and students. The problem which Dr. Pettigrew set himself appeared to us to be not only novel, but one of no ordinary magnitude. To trace the circulation from the lowest to the highest forms of life opened up a wide field of inquiry, and necessarily required for its elucidation an extensive knowledge of anatomy, physiology, and natural science generally. We have reason to know that these lectures, characterised by great vigor, originality, and breadth of thought, have been listened to with no ordinary interest, and we congratulate Dr. Pettigrew on the success he has achieved as a lecturer. As regards the physiology of the circulation they are unique, and by their grouping together isolated and original facts they have materially advanced our knowledge, and imparted to the whole subject a new and lasting interest.

PLASTIC APPARATUS IN SURGERY.

Dr. Samuel St. John, of New York, has contributed a very practical paper on this subject to the current number of *Hay's American Journal of the Medical Sciences*, and the observations he records seem to us deserving of the attention of surgeons in this country. After a short introductory sketch as to the past and present treatment of fractures, the author goes on to substantiate the expression of a conviction with which he sets out—namely, that no other apparatus is so successful for the treatment of this class of surgical injuries as the so-called immovable apparatus, and that no other variety of that apparatus is so good as that made with gypsum or plaster-of-Paris. The author appeals to the records of the Bellevue Hospital, where this treatment of fractures has been most extensively pursued; and he has tabulated the results of 187 cases of simple, and 26 cases of compound fracture. Among the results claimed for this method we may notice freedom of the patient to move about. The routine plan of treatment of simple fracture of the leg at Bellevue Hospital has been, to put it up immediately, and get the patient upon crutches the next day. Malgaigne observed that "for the patient to leave the bed is of no therapeutic utility; by it the surgeon only aims at pleasing the patient, at dazzling the eyes by a kind of *tour de force* which is not wanting in originality." As regards fractures of the femur, in which Malgaigne declares this practice to be specially fraught with danger, Dr. St. John shows, by his record of 50 cases of this very injury, that the average result was superior to that obtained by any other method, and in which it so happened that the two worst results arose in cases confined to bed for other injuries during the consolidation of the fracture. A patient with simple fracture of the femur treated by the gypsum apparatus may be out of bed on crutches two or three days after the fracture. For a description of the various materials to be employed, and the different methods of their application, so as to meet the exigencies of surgical practice, our readers must refer to the details in the paper itself. Our author claims that the results obtained at Bellevue Hospital from 192 cases of simple fracture (50 being of the femur) and 26 cases of compound fracture are equal, and in some fractures better, than those obtained by other methods; that the union is not delayed by it; and that it may be safely applied at once in fractures of all bones. A word in conclusion. We agree with a remark quoted from a prominent American teacher, to the effect that, without distrusting the method itself, special experience is required to apply it properly.

STRUCTURE OF THE GREY SUBSTANCE OF THE CORTEX OF THE CEREBRUM.

Prof. Gerlach publishes in the *Centralblatt* (No. 18) the results of his investigations, continued through the past winter, of the structure of the cortex of the hemispheres of the cerebrum. He applied the gold method as a hardening means, and found—1. That besides the well-known white medullated nerve-fibres running from the white into the grey substance, and which are arranged in fasciculi radiating towards the periphery, there are also numerous horizontally-running, medullated fibres, which communicate both with one another and with the radially disposed fibres, forming a coarse plexus visible even with a power of sixty diameters. 2. That in the meshes of this plexus are contained the ganglion-cells, and a second far finer plexus of extremely delicate non-medullated fibres which can only be brought into view with the highest powers of the immersion system. This second plexus is formed of the finest protoplasmatic processes of the nerve-cells, whilst the medullated fibres appear in part to take their origin in it, the individual fibres becoming surrounded by medulla. 3. Some of the protoplasmatic processes of the ganglion-cells develop directly into medullated fibres without branching, and thus such fibres originate partly from the cells themselves, and partly from a plexus of non-medullated fibres.

A PRACTICAL AMERICAN.

Our contemporary, *Land and Water*, contained last week a very interesting paper by General A. T. Pleasonton, of Philadelphia, U.S., on "The Influence of the Blue Color of the Sky in Developing Animal and Vegetable Life." Remembering what he had been taught and had read concerning the blue and violet rays of the spectrum, their great chemical power and their stimulating influence upon vegetation, General Pleasonton constructed a vine-house, and had every eighth row of glass of a violet color. In other respects his vinery did not differ from other vineries, but the growth of the vines was most extraordinary. Their increase of size was so rapid, and the amount of fruit which they bore *in the second year* was so extraordidary, that he soon became famous among his grape-growing neighbors, who prophesied that such marvellous fecundity would very soon result in complete sterility. This prophecy has not been fulfilled, and the vines still remain the admiration and envy of all beholders; and their proprietor attributes his success entirely to his having used a few panes of violet glass.

CONTAGIOUSNESS OF PURULENT SECRETIONS.

Dr. Hiller, in an inaugural dissertation on this subject, maintains the truth of the old view that all purulent secretions are contagious when applied to healthy mucous membrane. He states that pus taken from the vagina of a bitch in which inflammation had been excited by the injection of ammonia produced pyorrhœa in the urethra of a dog into which it was injected, though not before the latter had been mechanically irritated. If no mechanical irritation were employed no catarrh occurred. The pyorrhœa both in the male and female animal appeared to be closely analogous in its nature to gonorrhœa, since it spread in the former to the posterior part of the urethra, and in the latter to the mucous membrane of the uterus. M. Hiller holds that the same obtains in the case of the eye, and that all purulent discharges applied to the conjunctiva are capable of exciting inflammation in it. It is certain, however, that the matter from an inflamed lachrymal sac, or from an abscess of the lid, may freely enter the eye without producing a purulent discharge.

NEW MICROSCOPIC SLIDE.

At a late meeting of the Biological and Microscopical Section of the American Academy of Natural Sciences, reported in the *Philadelphia Medical Times*, Dr. Hunt called attention to an ingenious slide invented by Mr. D. S. Holman for exhibiting infusoria, especially under the gas microscope. These slides are made by grinding a deep depression in a thick slide, and then making a very shallow circle all round the deeper central cavity. If the central depression be filled with the organisation in water which is desired to be studied, and covered with a thin glass, in a short time the shallow circle all round the margin will be filled with minute bodies, often zoopores or other objects in the life history of the object in the deeper cell. The pressure of the atmosphere retains the cover in position.

A means of using the electric battery during the whole process of shampooing and washing in the Turkish bath has been devised by Dr. Joseph Dow, a well-known American physician in the city of Providence, Rhode Island. Streams of water are charged with the electric current, and the patient, as he lies on the lavatory slab, receives into his body the magnetic fluid without contact with the attendant, and almost imperceptibly over his entire system. All whose nervous cachexia has induced them to give Dr. Dow's device a trial report favorably as to its efficacy.

Correspondence.

"Audi alteram partem."

MICROCYTHÆMIA.

To the Editor of THE LANCET.

SIR,—The current number of the *Quarterly Journal of Microscopical Science* contains an abstract of a memoir published last year by Profs. Vanlair and Masius, of Liege, "On a Morbid Condition of the Blood hitherto undescribed," consisting in the presence of a number of spherical, highly refractile, red-colored bodies, in addition to, and to a great extent replacing, the ordinary red corpuscles. These bodies, from their small size (their diameter being rather more than half that of the red discs), are called "microcytes," and the condition of blood in which they exist is styled "microcythæmia." For fuller details as to the character of these bodies, and for the history of the case in which this condition of the blood existed, I must refer your readers to the journal in question. It is sufficient to state that improvement in health was accompanied by a marked diminution in the number of "microcytes" till finally none remained.

In April of the present year a female patient, the subject of Addison's disease, was readmitted into the Middlesex Hospital, under the care of Dr. Greenhow, suffering from extreme prostration consequent upon a slight injury she had received on the forehead. Examination of her blood while she was still in this condition yielded the following results:—Very few red corpuscles were present, and these not forming rouleaux, but occurring scattered singly over the field; a large number of globular, highly refractive nuclei of a red color, measuring about 1-4500 of an inch on an average, mostly isolated, but here and there occurring in considerable groups; no marked diminution or excess in the number of white corpuscles. As the patient increased in strength the number of these bodies diminished, and at the same time the tendency of the red discs to form rouleaux became restored, so that on her leaving the hospital at the end of about three weeks, her blood presented almost a normal appearance.

At the time no satisfactory explanation of the condition of the blood could be offered. The observations of Dr. Braxton Hicks on pathological changes in the red corpuscles* suggested the possible derivation of these "nuclei" from pre-existing red corpuscles, being the separated "zooid" element of Brücke. But then no corpuscle was seen in the act of breaking up, and the "œcoid" constituents were nowhere visible. That it was a condition not special to Addison's disease was soon proved by its occurrence (although to a far less marked extent) in a case of simple debility and anæmia, and in one of constitutional syphilis. The publication of this memoir seems, however, to offer a satisfactory solution to the question, or at any rate suggests the identity between the bodies found in the blood in these cases with the "microcytes" occurring in "microcythæmia."

My object in writing this has been, not to criticise the hypothesis framed by the Liege professors to account for the production of the "microcythæmic" state, but simply to draw attention to the fact of the existence of such a condition, in the hope that it may lead to some further light being thrown upon the minute pathology of the blood. Apologising for trespassing thus on your valuable space, I remain yours truly,

SIDNEY COUPLAND.

Middlesex Hospital, July 22nd, 1872.

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* Quart. Journ. Micr. Sc., April, 1872.

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ON THE USE OF HYDRATE OF CHLORAL IN THE PREVENTION OF SEA-SICKNESS.

To the Editor of THE LANCET.

SIR,—As during the next few weeks medical men will frequently be requested by holiday pleasure-seekers to prescribe a preventive of sea-sickness, I am desirous of directing attention to the value of chloral hydrate for that purpose. Isolated cases illustrative of the use of chloral in this disorder have been from time to time recorded in the medical journals; but, so far as I have seen, no collection of cases sufficiently large in number to enable us to form a correct estimate of its utility, or the reverse, has yet appeared.

During the spring of the present year an unusually favorable opportunity for experimenting as to the value of chloral in sea-sickness presented itself—viz., an exodus of stone-cutters which took place from Aberdeen to New York, and of this I gladly availed myself. Being appealed to by a considerable number for "a cure for sea-sickness," I ordered in every case hydrate of chloral. Its effects were of the happiest description; for out of upwards of eighteen cases, in one only did the drug fail to fulfil the indications for which it was prescribed. The weather during all the voyage was rough, and sickness among the other passengers was exceedingly prevalent. So satisfied were the men who went out first of the efficacy of the medicine they had employed that they wrote home, directing those who were to follow to supply themselves with similar prescriptions. I may also mention the case of a young lady, a martyr to sea-sickness, who on sailing the other day had no sooner left this harbor than she began to feel sick. She therefore, according to my directions, took a dose of chloral and lay down, the result being that during the whole passage, lasting two days, she was perfectly free from her former enemy. The same result has attended many other cases in which I have given it. The dose I generally employ is thirty grains in slightly sweetened water, directing the patient to take a like quantity if relief does not follow in twenty minutes. I advise a little light, easily digested food to be taken before sailing, and direct the chloral to be taken and the patient to lie down on the first symptoms of sickness presenting themselves.

Yours, &c.,

J. C. OGILVIE WILL, M.D.

Aberdeen, August, 1872.

DR. RICORD ON SYPHILIS.

(*Meeting of the British Medical Association at Birmingham, August*, 1872.)

In the Surgery Section a paper was read by Mr. Acton, M.R.C.S. Lond., on the Treatment of Syphilis.

The Chairman (Sir W. Fergusson) then introduced Dr. Ricord of Paris, who received a hearty welcome from the meeting.

Dr. Ricord, after acknowledging the reception which had been accorded him, said he had not prepared an address, as he had not come with the intention of speaking; but Mr. Acton had caught him and obliged him to speak, which was a trick. (Laughter.) He had come to listen and to learn, but not to teach. However, he must say something, though there was no necessity for him to say much, as Mr. Acton had so nearly stated his views and his mode of treatment that there was very little for him to add. There was one great question in regard to syphilis, and it was this: could it be cured radically? In former times all venereal affections, no matter what, were considered as belonging to syphilis, and certainly there was then an immense number of radical cures by mercury or any other means. In this way swellings of the glands, soft chancres, even warts, and other things not belonging to syphilis, were easily enough cured, radically cured; and there were no after-consequences, no secondary symptoms. This explanation would account for the immensely large number of cases of (reputed) syphilis which used to be radically cured. But, since syphilis had been correctly diagnosed, the inquiry to which he had devoted a large part of his life was to see what belonged to syphilis, and what resembled it without belonging to it. There had been great differences in the results of treatment—so much so that a doubt, as Mr. Acton had said, had arisen whether real syphilis could be cured. That doubt as to the curability of syphilis was not recent; it was a doubt which old authors had expressed; and one particularly, with a curious name, which they would probably remember—"Mercurialis"—thought that now and then an armistice might probably be made with syphilis, but that there was no real cure. In fact, they frequently saw that a long time—months, years—after the symptoms had been treated new symptoms appeared. And so the doubt whether syphilis could be radically cured, or whether the cure was only temporary, with a prospect of the symptoms returning, might still remain; he (Ricord), however, had established the law of the unicity of the diathesis of syphilis. The law of syphilis was the same as the law of small-pox, cow-pox, or measles. A man could have but one attack so long as the disease remained in the constitution—that was to say, according to his opinion a new attack could not take place while the system was still under the influence of the old diathesis. Well, it was exactly so with syphilis; as long as a patient was laboring under the diathesis of syphilis, another *infection* of syphilis could not occur—it was impossible. For instance, after indurated chancre, and the appearance of secondary symptoms, it was not possible for the patient to contract a new indurated chancre, with swelling of the glands, manifestation of skin disease, and so on. After one attack the patient could not have another infection as long as the influence of the first remained in his body; a second contagion could not take possession of the system at the same time. If, perchance, something of the kind took place, the symptoms would not follow the regular evolution. So, when a patient had constitutional syphilis, if a new chancre appeared to be hardened they would not find the glands swell, or the early manifestation of skin disease appear; and so of other symptoms. Superficial ulceration might take place, just as a spurious form of vaccination might arise on one who was still under the vaccine influence; but it was not a true case, it was not attended with the sequelæ. But if the constitutional disease were cured, if the syphilitic disposition were completely eradicated, then the patient would be able to contract a fresh indurated chancre, with all the subsequent symptoms. If this were the case—and he had observed it with great care, his experience dating back forty years—it proved that syphilis could be cured; and if syphilis could be eradicated, to ascertain whether a patient was cured or not when all the symptoms had disappeared there would be nothing else to do (though he knew that could not be done) but to try inoculation from an indurated chancre. If vaccination did not take, they were sure the vaccine disposition continued; if it did not continue, vaccination could take effect. In regard to syphilis, the proof had not been carried to this extent; but he had been able to observe that as long as the syphilitic influence continued, a patient could not contract an indurated chancre anew, and that, consequently, if cured, a new infection might take place. This was a great point gained in science, and it proved what he had said, that syphilis could be radically cured. Now, as to the treatment of the disease. As he had told them, Mr. Acton's ideas were completely his ideas, explaining his manner of treatment and his practice. He would first speak of the treatment of the first stage—that was to say, the primary sore. As soon as he had ascertained that there was a hardened chancre, with a swelling of the glands—not inflammatory, because the glands in this case never suppurated,—he immediately instituted the mercurial treatment. There was one point on which there was some difference of opinion: many believed that it was impossible to prevent the accession of the secondary symptoms, the first manifestation of constitutional disease; many thought that no matter what treatment was employed the sequelæ would appear. Well, he had ascertained that if the treatment were soon begun and well carried through, the bursting out of the first secondary symptoms, the roseola, the swelling of the glands of the neck, &c., might be prevented. If this were not frequently the case it was because the treatment was resorted to too late, when the disease had had time to take root, and secondary symptoms were about to show themselves. In such cases it was not astonishing that secondary symptoms should appear, and the treatment ought not to be blamed; if the treatment were steadily continued they soon disappeared. But if the treatment were begun early, the observation of forty years gave him the assurance that secondary symptoms would not appear. When secondary symptoms had appeared the best treatment was, as Mr. Acton had said, mercury. If they wished for a perfect cure, this treatment must be continued. In general it was not persisted in long enough; it was dropped as soon as the symptoms disappeared, or a short time after, and then it was not astonishing to see them reappear. But if the treatment were continued five or six months, having regard at the same time to sustaining the constitution in general, relapses would be found to be infrequent. He observed very few cases of relapse, and there would not be many when the treatment was well kept up—when the patient had patience enough, and the physician sufficient courage. After six months of that treatment and no symptoms reappearing, then the treatment with iodine must be begun, and continued for five or six months more. When a patient went to him, he said, "You will have a year's treatment—do you consent to that?" "Yes." "Very well; we will go on. If not, Good bye." There were cases in which syphilis occurred in a healthy person—the only disease was syphilis. Then treatment was very easy—the case was a simple one; they had but one enemy to fight—all went on regularly. But, unhappily, in many instances syphilis was not alone; there was something else—scrofula, skin disease, scurvy, low constitution, poorness of the blood. They must understand that such complications as these altered the case; the treatment did not act so powerfully as it would do in the first case, as many of these complications were aggravated by the treatment. For instance, syphilis and scurvy might coexist—and the characteristic of the latter was poorness of the blood, while that of the former was a plastic condition of the blood. Here, therefore, was a counteracting influence to the treatment for syphilis. Now one thing must be known. Perhaps he was speaking too long? (No; go on.) Well, in many instances syphilis became the secondary consideration, and they must begin with the constitution of the patient, as debility was the disease that required first treatment. They must attack the strongest enemy first. Syphilis was sometimes quiet, and stopped and waited till they came to it. So, when they had improved the constitution, they might commence with the treatment, and they must begin by treating the constitutional complication. The best treatment was the proto-ioduret of mercury. The stomach bore this well in general. Sometimes it gave rise to a little diarrhœa, which was an easy thing to moderate; but when the stomach was not tolerant of the remedy, one capital treatment was that which Mr. Acton had told them he had confidence in—namely, rubbing-in. If this were not an unpleasant and disagreeable operation, certainly it would be in general about the best; he himself should prefer it. In rubbing-in, the action of the remedy was powerful and quick, and the stomach was not at all troubled with it. If it were not so disagreeable, and were a thing that could be done without being noticed, he should give it the preference. However, there were cases in which the skin was otherwise affected, in which there was a skin disease, and then friction could not be used. In a case of complication of syphilis and herpes rubbing-in could not be resorted to. In general, patients bore the iodide of potassium well, and in large doses. For his own part he frequently employed forty, sixty, eighty, even a hundred grains a day, and more. They must bear in mind that if they gave too small doses to some patients they would have no result; it was a remedy that passed through the body with great rapidity. He had had great experience of it, and he had found that in half an hour it had passed away in the urine. Iodide of potassium was a sort of broom of the blood. So they saw that the methodical treatment was this: mercury, iodide of potassium. But only one for the first stage, and only the other for the later stage of syphilis? No, the rule was absolute that as long as there were secondary symptoms well marked, mercury must be given; when there was a mixture of secondary and tertiary symptoms, mercury and iodide; for tertiary symptoms, iodide. To treat some patients with iodide would not advance them in any way. Why? Because there was frequently in the constitution, in the blood, something of the second stage, something that required the mercurial treatment. This might not show itself, but when iodide of potassium ceased to do good, the disease remaining stationary, let them go back to mercury again, and they would have a splendid result where they had thought there was no further possibility of curing the patient. This was what Mr. Acton had said, and he was completely and absolutely of Mr. Acton's opinion. But there was another thing. When syphilis had lasted for a long time, and had had a great effect on the constitution, it in some way disappeared, and left the patient with a complication existing that was not existing before. Sometimes a long course of treatment brought on a new disease—wasting of the constitution, poorness of blood. They must then stop all the specific treatment, and, applying themselves to the principal symptom, restore the constitution by preparations of iron, bark, tonics, and proper food, so bringing the patient back to the possibility of undergoing anew a regular methodical treatment, either by mercury or iodide, or a combination of these two remedies. In former times, when a person was thought to be syphilitic, physicians seemed unable to entertain any other idea than that of syphilis, and acted exclusively against a specific disease, neglected everything else, and in that way they experienced all the bad effects and accidental symptoms which a bad administration of the symptoms would produce. Mr. Acton had spoken of the use of bromide of potassium. His views were exactly the same as Mr. Acton's with respect to the use of the remedies at different stages, the necessity of having regard to the complications that might exist, and of dropping the treatment for a while till the constitution was restored. This was regular and methodical, and his own manner of practice. But now, was bromide of potassium an anti-syphilitic remedy? He did not believe that it was. He might be mistaken; but he had experimented with it in syphilitic symptoms, and without any apparent result. But it was a splendid remedy in complications of syphilis. In some cases of symptoms referable to the nervous centres, bromide of potassium was an adjunct, and came to the help of mercury or the treatment by iodine. In some cases of brain disease with syphilis, and of disease of the spine or epilepsy, bromide of potassium did wonders. So that they would see it was a remedy to be applied in nervous complications that might occur, but they must not depend on it as an anti-syphilitic remedy. Now, there were symptoms following syphilis which were not syphilitic, and these must not be treated with mercury or iodide of potassium. For instance, there might be necrosis. Well, they could not bring a dead bone back to life, no matter what quantity of mercury or iodide of potassium they might give. A physician must know these things, and he (M. Ricord) ought almost to apologise for bringing them forward. It should be observed that specific remedies did not always act specifically. Certainly, there was no specific effect without a specific cause, but specific causes did not always act specifically. So there were some effects of syphilis, such as disease of the bones, that would afterwards act as a common irritant. In syphilis there might be an ulcerated bone in the nose or mouth, bringing on suppuration; mercury or potassium would not remove that, but let the diseased bone be removed, and the patient was frequently cured. They must take note of all these conditions—the nature of syphilis, the manner in which it conducted itself, its action on the constitution. Let them particularly take note that the general law of syphilis was the same as the general law of small-pox, vaccine, and measles. If they were sure of this from what he had said and from their own experience, then they might be sure that syphilis could be perfectly, radically cured. They could tell their patients that, and give them courage and hope. If the patient had courage to go through with the treatment, and the physician had courage enough to stick to it, the patient might be radically cured. He thanked them for the reception they had given him; it reminded him a little of his hospital in Paris.

A question was asked whether Dr. Ricord was a believer in salivation.

Dr. Ricord replied—No, surely not. Salivation was an accident following the treatment, and it must be avoided as much as possible. There was but one case in which he approved of salivation, and that was in disease of the eye—iritis. When this occurred, and salivation was brought on, the inflammation of the iris subsided.

Dr. Gross asked whether the soft chancre was capable of contaminating the constitution.

Dr. Ricord said his opinion was that a soft chancre, when accurately diagnosed, never gave rise to constitutional disease. This was a law as absolute as possible. But they must be careful, or errors of diagnosis might be made. It was not always easy to establish the difference between soft and hard chancre, but when the diagnosis was certain, they might be sure they would not have any constitutional disease after the soft chancre. On the contrary, even as long as six months after hard chancre secondary symptoms would appear. This was one of the most clearly established facts in practice. But the hardness of the chancre was not always well marked (*bien formulée*); it might be very superficial in those varieties that were attended with excoriation. When there was a something like parchment at the base, a chancre was very easily taken to be soft, but was not so; and he had had cases sent to him as instances of soft chancre which had been followed by secondary symptoms, but which were well characterised by the parchment-like base. However, there was a symptom of more value than the parchment base, a symptom that was one of the most important witnesses to constitutional affection, and that was the non-inflammation of the glands—they were cold and dull. In general several of them became enlarged; it was very seldom that only one was found to swell after hardened chancre; and not only were the glands swollen but the enlargement frequently occurred on both sides, in both groins. The enlargement of the glands was of much value as a characteristic of hardened chancre. The enlarged glands appeared very early, even during the first fortnight of the existence of the sore. With the soft chancre the glands did not always swell; in a great many cases there was no swelling. They would never find a real hard chancre without swelling of the glands; and they would also find many cases of soft chancre with swelling, these cases depending upon surgeons confounding the hard chancre with thickening dependent upon inflammatory infiltration of the tissue immediately around the sore. But if the glands should swell after soft chancre, it was probable that suppuration would come on. With hard chancre there was no inflammation and no suppuration. The older writers directed their efforts to cause an indurated sore to suppurate, in the belief arising from the practical observation that when a bubo suppurated there was no constitutional disease, and therefore they were under the belief that the poison was thrown out of the body. In their quaint way of putting the fact "they did not like to shut up the wolf within the fold." But they could not bring on specific suppuration in the case of indurated glands; it was impossible. He had tried all means of doing it, and could not succeed in the cases of specific suppuration. In the instance of soft chancre what had they to do—await the occurrence of suppuration, which might either be attended by simply inflammatory or specific bubo? With the soft chancre the inflammatory bubo appeared sometimes two, three, or four weeks after the occurrence of the chancre, and it had the characteristic pus of the soft chancre. There was such a difference between hard and soft chancre that it was difficult to make a mistake. When a patient consulted him (M. Ricord) suffering from soft chancre he said to him, "Be quiet; you may have a bubo; that will suppurate, but your constitution will be unaffected; you will not be liable to secondary symptoms." With a hard chancre he could predict indurated glands, attended by constitutional symptoms, within six months, provided proper treatment were not followed. He would add, that when it was decided that the case was one of hard chancre or soft chancre, the treatment was very simple. When there was a doubt as to the nature of the chancre, he waited till some characteristic symptom arose. But there were cases in which the existence of a soft chancre did not prevent a patient from contracting a hard chancre. The patient might have the two species at the same time, contracted from different sources. The two species, hard and soft chancres, do not depend upon the difference in the ground, but on a difference in the seed (*contagium*). So that the new comer who had relations with a woman suffering from the two species could take his choice. If the patient had a true indurated chancre and well diagnosed secondary symptoms, he might catch the soft chancre as often as he pleased, and it would be unattended with specific constitutional disturbance.[*]

Mr. Lord (London) asked Dr. Ricord what was his experience of municipal interference in respect to contagious diseases in Paris, and what was his opinion as to the effect of such interference in promoting immorality and degrading the character.

Dr. Ricord said it was surely a very good thing to have the women examined. It made the disease less frequent—no doubt of it. From what had already been done in France he saw that the same practice would be beneficial here. It was already a great thing that English sailors no longer brought the disease into France; the French would take care it did not return back into England, and that was a free exchange.

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[*] Dr. Ricord has established a law on which he sets great value, and for the verification of which he thinks the present and future generations will owe him a debt of gratitude. It is that of having discovered and described the UNICITY of the syphilitic diathesis—in fact, subjecting syphilis to the LAW which is common to small-pox, cow-pox, measles, &c.—ED. L.

The Chairman announced that the Council had resolved to make Dr. Ricord, Dr. Demarquay, and Professor Gross honorary member of the Association.

Foreign Gleanings.

ASPIRATION OF MORBID FLUIDS AS A MEANS OF DIAGNOSIS AND TREATMENT.

In a note communicated to the Academy of Sciences by Claude Bernard, Dr. Dieulafoy, of Paris, has summed up the results of the various interesting experiments which he has been conducting for two years with his capillary aspirator, an instrument which is now being extensively used in Paris with the very best results. M. Dieulafoy strongly insists on the use of the aspirator as a means of diagnosis and treatment. It serves, in the first place, to show with certainty the presence, seat, and character of various pathological fluids, and also to dry up the source of such fluids. Dr. Dieulafoy, emboldened by the results obtained, does not hesitate in asserting that it is always possible to reach with certainty, and with no kind of danger, any collection of liquid, whatever may be its situation and its nature; that the aspirations may be frequently repeated in the same position without any inconvenience, and almost always with the effect of drying up the source of the fluid. He lays down the following law as the result of his experience:—When a fluid, whatever may be its nature, gathers in a serous cavity, or in any organ, and when that cavity or that organ is accessible without danger to the patient, our first care should be to withdraw that liquid; if it again forms it must be again withdrawn, repeating the operation frequently, if necessary, until the serous membrane is exhausted by a purely mechanical and quite inoffensive means, and without attempting to modify the secretion by irritating and sometimes dangerous agents.

The following is a list, divided into three groups, of the various diseases to which M. Dieulafoy's aspirating method has been applied:—1. Fluids accumulating in a serous cavity: hydrocephalus, hydrorachis, pleurisy, pericarditis, hydrarthrosis, synovial cysts. 2. Fluids formed in the deep parts of organs: abscesses or hydatid tumour of lungs, abscess or hydatid of liver, liquid tumours of the spleen and of omentum, ovarian cysts, retention of urine, strangulated hernia of the intestine. 3. Liquids formed within the cellular tissue of various regions: congestive abscess, bubo, perinephritic phlegmon, iliac phlegmon, peri-uterine phlegmon.

CHLORATE OF POTASH IN CASES OF DYSENTERY IN THE ADULT AND INFLAMMATORY DIARRHŒA IN CHILDREN.

In one of the recent numbers of the *Medical Record*, Dr. J. B. Amisy relates his experiences of chlorate of potash, especially in the above cases. In one case of dysentery, the details of which are given, opium and various other drugs having proved useless, the chlorate of potash was tried, being the only medicine the stomach could tolerate. The substance was administered in doses of ten grains every three hours for some days, and then ten grains every four hours until complete recovery, which took place at the end of a month. Dr. Amisy states that he has employed chlorate of potash in inflammatory diarrhœa, and in all the forms of dysentery in children, and that he considers the medicament as a specific for such diseases. In the treatment of typhoid fever chlorate of potash is, he adds, the only drug which he has exclusively and trustfully employed for ten years.

THE IRON OF BLOOD.

In a note communicated to the Academy of Sciences of Paris, M. Boussingault gives the results of fresh experiments touching the character of the iron contained in the blood of animals. The subjects of this last series of researches have been white-blooded animals: the crimson-yellow garden snails. The blood of these animals is almost colorless, or slightly yellowish, and contains a small number of ovoid globules about the size of the globules of cows' blood. It furthermore contains some mineral substances, forming granules with a crystalline structure. One hundred grammes of the blood afforded 8·9 gr. of solid matter, which on being burnt gave 0·007 gr. of iron. The same quantity of the muscular flesh of snails only afforded 0·0011 gr. of iron; so that in snails, as in the vertebrated animals, there is a little more iron in the blood than in the muscles.

TWO CASES OF VEGETATING ENDOCARDITIS.

At a recent sitting of the Paris Academy of Medicine, Dr. Oulmont presented the morbid specimens of two cases of vegetating endocarditis. Both hearts showed a considerable quantity of long, loose, floating pediculated vegetations attached to the endocardium. These vegetations, the origin and cause of which it was not possible to make out, did not seem to produce in themselves any serious inconvenience, but through their fragmentation they gave rise to the migration of numerous embolisms, which obstructed the capillaries and bloodvessels of smaller size, and produced bloody suffusions, softening, &c., thus causing death in both cases. M. Oulmont compared this variety of vegetating endocarditis with ulcerating endocarditis, which it resembles in its embolic formations, but from which it differs by the course of the disease and the general typhoid and pyæmic symptoms, which had not been observed.

CHARACTER AND TREATMENT OF MALIGNANT SCROFULOUS ANGINA.

Dr. Constantin Paul, in a long article published on the above subject in *l' Union Médicale* (number ber for July 23rd), reviews the whole question of malignant scrofulous angina, as elucidated by the details of twenty cases which he has been able to observe. The chief points of the paper may be summed up thus:—There *does* exist a malignant ulcerated angina of a scrofulous nature, the characters of which are now sufficiently recognisable to enable us to form a correct diagnosis and to prevent us from pursuing an injurious mercurial (anti-syphilitic) treatment. Cod-liver oil and the general remedies used in scrofula are to be employed. Locally, tincture of iodine and perchloride of iron have not been very successful in Dr. C. Paul's hands, but he has found the acid nitrate of mercury to be of service.

PALLIATIVE TREATMENT OF ECTROPION.

In a clinical lecture published by the *Presse Médicale Belge*, Dr. Thiry lays great stress on the palliative treatment of ectropium and the very successful results which he had obtained by means of the subnitrate of bismuth. In combating the frequent inflammations of the palpebral conjunctiva so often met with in the course of ectropion, as also the spasms which depend upon these inflammations, and which constantly tend to increase the eversion of the eyelid, Dr. Thiry has employed with great benefit the following preparation:—Subnitrate of bismuth, one drachm; starch, two drachms; glycerine, three drachms: mix. Fifteen to thirty grains of acetate of lead may be added when there is need to exert an astringent action on the tissues.

DIABETIC PHTHISIS.

There is some difference of opinion in respect to the real character of phthisis in diabetic subjects, some considering it to be ordinary tubercular phthisis, whilst others deny it to be such. At a recent meeting of the Société de Biologie of Paris Dr. Granchet related an interesting case of cheesy pneumonia in a diabetic sufferer, in which the post-mortem showed that there did not exist real granulations, but that the interalveolar partitions contained a considerable number of embryonic elements. Dr. Granchet argued that, if it was not well-characterised tubercle, it was at least the tubercular process, and therefore he was opposed to Lancereaux and others, who do not admit the tubercular nature of phthisis in diabetes.

A MEANS OF RECOGNISING THE PRESENCE AND CHARACTER OF METALLIC SUBSTANCES IN GUNSHOT WOUNDS.

M. Desneux suggests the following proceeding, which he has employed in three cases. A flexible stem is covered at one of its ends with lint steeped in a solution of nitric or acetic acid, or simply vinegar. The lint is introduced into the wound for a few minutes, and then put into contact with a solution of iodide of potassium, so that reaction may take place. If there is any lead in the wound, the yellow color of iodide of lead is immediately produced.—*Bulletins de l'Académie de Médecine de Paris*. Sitting of July 16th, 1872.

USE OF MILK DIET IN CANCER OF THE STOMACH.

The invaluable benefit of milk diet in cases of cancer of the stomach has been forcibly brought out in an instance recorded by the *France Médicale* for August 24th. The patient, under the care of Dr. Siredey, at the Hôpital la Riboisière, had not been able for two months to take any kind of food without immediately throwing it up. Milk, in small quantities at first, was then ordered as diet. It was not brought up, and consequently during thirty-six days it was used in any quantities, and without inducing sickness. At the end of this time other sorts of food were given, and properly retained.

SUBPERIOSTEAL RESECTION OF THE ELBOW.

M. Ollier, of Lyòns, has just made known the results of subperiosteal resection of the elbow in forty-eight cases, twelve of which were traumatic, and the others caused by chronic arthritis. M. Ollier states that, with proper care and skilfulness in dissecting the entire periosteal sheath, the bones, are not only reproduced in their continuity, but with their heads and articular tuberosities, so as to permit the play of the joint in all its movements. It is necessary to employ minute precautions in the after-treatment for obtaining the above results. Slight movements must be made at an early time with the joints. Anchylosis is not to be dreaded, but rather too great mobility of the joint, especially in individuals of from forty to fifty years of age. In these cases the bones must be cut to a certain length, so as to avoid such mobile articulations.

DIAGNOSIS OF INCIPIENT CANCER OF THE NECK OF THE UTERUS.

Prof. Spiegelberg states, in the *Arch. f. Gynæc.*, vol. iii., part 2, that in the incipient stage a distinction between cancer and simple induration of submucous cellular tissue can easily be made in the following manner. The mucous membrane, in cancer, will be found firmly glued and immovable, which is not the case in hyperplastic thickening and induration, and when the sponge-tent is introduced, the latter thickening will become looser, softer, and thinner, whilst in cancer the neck remains hard and unyielding.

News Items, Medical Facts, &c.

Macaroni.—This, the national fare of Italy, deserves much more popularity than it enjoys among ourselves. Weight for weight, macaroni contains from two to three times as much flesh-forming material as good household bread. This is the opinion of eminent analytical chemists at home and abroad; while Dr. Hassall claims for it far more nutrient power than any of the cereals employed as food in this country. Now that butcher meat has become so high in price, the use of macaroni, as a substitute, twice or thrice a week deserves consideration. It is susceptible of varied culinary treatment, and is not only palatable and appetising, but of high nitrogenous value when cooked with cheese.

A curious but by no means unusual lawsuit was lately decided in France. A surgeon extracted a piece of cartilaginous meat from the trachea of a lady, and charged her £20 for the operation. The lady considered the amount excessive, and said that Nélaton himself would not have asked such a sum. Dr. Nélaton was then appealed to, and said that, taking into consideration the position of the lady, the charge was moderate. Ultimately a jury awarded the surgeon £12.

On some parts of the Continent excellent results are said to have been obtained by treating chancres locally with chloral.

The "Georgia Medical Companion" reports a case of glanders in a man cured by the internal exhibition of carbolic acid.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.

THE LANCET.

A Journal of British and Foreign Medicine, Physiology, Surgery,
Chemistry, Criticism, Literature, and News.

JAMES G. WAKLEY, M.D., M.R.C.S., EDITOR.

PUBLISHED MONTHLY.

No. 12. NEW YORK, DECEMBER, 1872.

Clinical Lecture

ON

EPITHELIAL CANCER OF THE TONGUE.

Delivered in King's College Hospital, June 7th, 1872.

By John Wood, F.R.S.,

Surgeon to the Hospital and Professor of Surgery in the College.

Gentlemen,—The subject of our consideration to-day is a disease which is only too common, and of which, during the present and last winter session, I have had no less than six cases under treatment in the hospital. It is that kind of cancer which assumes the form of an irregular and exuberant deposit of cells, identical with, or at least resembling closely, those cells which form the normal covering or investment of mucous membrane and skin, and which are called "epithelial," and are, so to speak, formations germane to the part affected.

The common forms of the disease are included by Hannover and Bennett under the name *epithelioma.* It is considered by the best pathologists, both in this country and abroad—Paget, Virchow, and Rokitansky,—to resemble so closely in its course and effects the real carcinomatous diseases of scirrhus and cephaloma as to entitle it to the fatal distinction of being placed among the cancerous diseases; while others consider it to be cancroid, or resembling it, in its characters. The most characteristic features are—its uncontrollable growth; its progressive invasion of neighboring tissue; its supplanting or pushing aside and destroying the growth of the normal structures; the very severe darting pains which attend its course, caused by its involving the ultimate fibrils of the nerves of the part, in the same manner, perhaps, as a corn affects them under the pressure of the boot; its liability to invade the lymphatics, and so to make its way into the system; and its occasional recurrence in the deeper organs and viscera, sometimes as epithelial deposits, and sometimes as one of the more decided malignant growths—namely, encephaloma. On the other hand, it is more tardy in its fatal progress than the more virulent forms of carcinoma, and is much less apt to recur in the deep-seated viscera as a secondary deposit, though its progress resembles in many important points that of scirrhus or hard cancer, especially that aspect of the latter in which the advocates of its local origin and slow continuously progressive course view it. And sometimes its microscopic elements can, taken by themselves, with difficulty be distinguished from some of the cell forms of scirrhus cancer. This is especially the case in that form of endogenous cell designated by Paget as "brood cells," where more than one nucleus is seen enclosed in a mother cell, as is usually the case in hard cancer. In some of its more mild manifestations epithelial cancer can scarcely be distinguished, even microscopically, from an ordinary warty growth, such as that which is common enough on the hands of young persons. In both may be observed the masses of clustered, more or less flattened epithelial cells, lying upon each other concentrically, like the layers of an onion, with little or no fibrous or connective tissue, and forming, by the intimate adhesion of the cells themselves, a very tough, hard, and resisting mass. It is only by the persistency and recurrence of the growth, the pain which attends it, its gradually spreading beyond the site originally affected, and its effects upon the general health, that epithelial cancer can be distinctly diagnosed. In many cases the disease is said to become developed out of so-called benign warty tumours, which are, consequently, usually suspiciously viewed by practical surgeons. And it may be said that the more closely adherent, hard, and slow-growing the tumour, the less disposed it is to ulcerate, and the less painful in its manifestations, the less dangerous its character may be estimated to be; while, conversely, the more loosely its cells are held together, and the more disposed it is to become disintegrated and to form an ulcer, the more rapid is its course and the sooner do the lymphatic glands become affected. Cases are recorded in the Pathological Society's "Transactions" in which the amount of fibrous stroma produced by the development of fibre-cells in the mass is so great that the disease assumes a more doubtful pathological character, and may be assigned to another place in the classification of pathologists more closely approaching to the scirrhus form of carcinoma. Some cases of so-called scirrhus have, I believe, been of this kind. Again, the disintegration of the cell growth may be so rapid that the features of the disease may assume the form of an open ulceration of a malignant character rather than that of a tumour; and many such cases I doubt not have been denominated "cancerous phagedæna." In fact, the more advanced and minute pathological observations become, the less disposed are practical men to draw any broad line of demarcation between these diseases. In well-marked cases the signs are plain enough, but at a time when diagnosis is most valuable the indications are usually the most slight.

In the tongue, the especial seat of the disease to which I wish to call your attention, these varied characters of what I consider to be essentially the same disease are commonly found. In the cases brought before you we have some commencing as a small primary fissure or cleft, very painful in character, with hard, raised, and thickened edges, and intractable progress. This has been described by some as the superficial form of the disease. Others have commenced as a somewhat deep-seated and very hard tumour, attracting the attention of the patient from the first by its tenderness, darting pains, and interference with the use of the organ in articulation and mastication, rather than by any sore or ulcer on the surface. This form was well illustrated in the case of James D——, aged thirty-four, who was in the Victoria ward from April 6th to May 11th of this year. In this patient attention was first called to the disease last July (a year ago), by a pain on the right side of the tongue, first felt after smoking a foul pipe. On the application of a little alum and abstaining from his pipe it became better, but got bad again in September last. He then went into the country and recruited his general health, and then felt nothing of it till two months ago, when he noticed a hard lump about the size of a nut. This rubbed against the sharp edge of a bad tooth, and then became excoriated. There was no history either of syphilis or cancer. He has always been temperate, but has suffered from rheumatic gout and from frequent attacks of bilious vomiting and dyspepsia. Now there was a good deal of cachexia about the man. He was thin, with glistening eyes, anxious look, and suffered agonising pain night and day. He presented a decidly jaundiced tinge, but there was no enlargement or tenderness perceptible about the submaxillary glands. After the removal of about one-third of the right side of the tongue along its whole length, followed by the free application of the chloride of zinc, the patient had an attack of jaundice, indicating either some already decided organic change or great functional susceptibility in the liver. From this, however, he in a few days got better, and finally made a very good recovery, picking up strength wonderfully quickly, feeding well, and being free from pain. On the 11th of May he left the hospital with the wound nearly healed, and expressing himself greatly relieved by the operation. He has since attended as an out-patient, and has had lately another piece of the tongue removed for a suspicious hardness which appeared at the base of a small sore left at the front part of the tongue.

Characterised similarly by a hard, solid lump appearing in the side of the tongue, and gradually involving two-thirds of the organ, was the case of Wm. C——, aged forty-three, a stout, thick-set, healthy-looking cab-driver in the Albert ward, who was operated on in March last. In this case there were present, when I first saw him, clefts and fissures on the side and dorsum of the tongue, which gave a strong suspicion of syphilis, of which there was some sort of a history of twenty years before. He was treated for some months as an out-patient by my colleague, Mr. Bell, and took large quantities of iodide of potass without benefit. The disease getting worse, he was admitted for operation, and again, under my own observation, an antisyphilitic treatment (iodide of potass and mercury) was followed, and nitrate of silver applied regularly. The disease, however, steadily progressed, and to relieve the poor man from his great suffering I removed under chloroform three-fourths of the tongue, including the tumour. The microscope showed both the tumours in these cases to be decidedly epithelial. They still visit the hospital occasionally as out-patients, and continue in good condition so far.

In these cases ulceration commenced secondarily as a slight abrasion of the surface, smooth and oval in shape, in one case, and in the other as fissures or cracks. This form has been described by some as the deep-seated variety, and has been alluded to by Sir James Paget as "a first formation" in masses of circumscribed infiltration of tissues beneath healthy skin or mucous membrane. After being thus formed the hardness and bulk of the lump cause the surface of the mucous membrane which covers it to become abraded by any exciting cause of irritation, such as the sharp edge of a decayed tooth or a tobacco-pipe. But such secondary causes would have no such effect without the previous formation of the diseased induration. If it were not so, we should find the disease in many more of the multitude of smokers with bad teeth who are to be found in these days. It would be very much more common than it actually is.

In one case, indeed, that of J. G——, aged sixty-eight, admitted into Victoria ward on May 3rd, a florid and not a sickly-looking man from Somersetshire, the patient smoked regularly, but had the habit of keeping his pipe on the right side of his mouth, whereas the disease appeared on the left side of his tongue in the usual place, at the edge midway between the root and the tip. He had noticed pain and soreness in this spot so long as ten years ago. Thinking it was irritated by the molar teeth on that side, he had these removed, although perfectly sound, four years ago. Twelve months ago he observed the part for the first time to be rather swollen and hard. The pain becoming worse six weeks ago, he began to emaciate, and the tumour got rapidly larger and more painful, and the ulceration spread further. On admission he was found to have a tumour about the size of a bantam's egg on the left border of the tongue, hollowed out in the middle by an extensive ulceration, covered with an ash-colored slough, and surrounded by fungous, raised edges, with a very fetid discharge. Frequent hæmorrhages had occurred from the middle of the sore. A submaxillary lymphatic gland could be felt enlarged and hard on the same side. There was a copious and distressing flow of saliva, and the poor man suffered excruciating agony both night and day. The effect of loss of sleep and suffering speedily became visible in his rapid emaciation and weak accelerated pulse. This patient had occasional pain in the ear of the same side, on which he was also rather deaf. On May 3rd, under chloroform, and aided by the use of the gag (which I shall presently describe), I removed two-thirds of the tongue, cutting clear of the tumour, and afterwards taking away some considerable portions which appeared to me to be rather suspicious in feel and appearance. Free hæmorrhage occurred from numerous vessels, which was with some difficulty controlled, the substance of the tongue being very friable, and giving way easily under the ligatures. The chloride of zinc and the perchloride of iron were freely applied, both separately and in combination. A blackened slough was formed all over the sore, which, after some days, came off, showing a smooth, healthy, and regular healing surface. The sore rapidly healed after a few applications of the solid chloride of zinc, which was employed to smooth down all irregular granulations; and on the 25th of May the man, looking vastly improved in health, free from all pain, and able to eat and speak with more comfort than he had had since the disease appeared, was discharged with the sore almost healed, at his own request, in order to look after his little business in the country.

The subsequent history of these cases is illustrated by that of Thomas M——, aged fifty-four, whom I now present to your notice. He was first operated upon on the 18th November last. The tumour was on the left side of the tongue, small, but extremely painful, with two small superficial sores on the surface and no glandular enlargement. The man had a very cachectic look. There was no syphilitic history; but, following my usual practice, before operating I put him on a preliminary course of iodide of potass and tartrate of iron, touching the sore twice a week with solid chloride of zinc. No relief resulted from this treatment, and the poor man got gradually thinner and more cachectic, from the constant wearing pain day and night, and the interference with his power of masticating food. Much relief followed the operation, which was attended with but little hæmorrhage. The patient picked up strength rapidly, now that he could masticate his food with ease and comfort. The chloride of zinc was applied regularly three times a week until Dec. 1st, when he was discharged apparently well. In February last, however, he came again to the hospital, complaining of pain and smarting in the tongue when he took his food; and it was found that an ulcer had again appeared at the site of the operation, with a considerable hard thickening at its base. Still there was no glandular enlargement, and the pain was of a darting, stinging character, passing backwards along the course of the gustatory nerve. I now determined to remove more of the organ than I had done before; and on the 7th February I applied a stout ligature thread after the fashion I employ for ligaturing nævus—namely, by a single hempen thread, transfixing the tongue with one loop well behind the tumour and another in front of it, the two ends of the string being brought round the edge of the tongue opposite each respective loop, and then passed through and tied tightly on the dorsum to the median line of the tumour. I then removed the tumour with a curved bistoury, taking away with it the greater part of the sublingual gland, towards which the hard diseased mass seemed to have progressed. The ligature had the effect of restraining the hæmorrhage very much. The wound was then freely cauterised with liquefied chloride of zinc. The ligature came away in a few days, and with it a considerable slough of the whole surface of the sore. The wound healed well and rapidly, and he was again discharged with it quite healed on the 6th March. None of the glands about the root of the tongue had become affected. Lately this patient, after some months of comfort and immunity from pain, has again applied with a small painful sore on the old cicatrix, to which we are now applying the chloride of zinc twice a week. He has again assumed the worn and cachectic appearance.

Another case illustrative of the subsequent course of these painful cases is that of Jas. G——, aged thirty-three. This poor man, at his comparatively early age, looked quite an old man and worn down by pain when he first applied in February last. He had previously been operated on twice by different surgeons without chloroform, and each time the disease had returned in the same way as at first—i. e., by the formation of a superficial sore without induration, which latter came on secondarily in each instance. There was no history of syphilis or cancer in the family. He had been put under a course of iodide of potass. The part began to give him trouble four years before. The first operation was done two years and a half ago; and the last, eight months before his admission. The sore was about the size of a fourpenny-piece, in the usual place on the side of the tongue (the left side), with a hardened base, extending by this time considerably beyond the sore. He was very sallow and cachectic-looking, and the glands on both sides below the angle of the jaw were of the size of filberts and very hard. After preliminary treatment by blood-tonics and all kinds of sedatives, which failed to give him anything like the relief obtained from the former operations, I, at his urgent request, operated under chloroform, and, with the aid of the gag, removed more than one-half of the tongue, with but little hæmorrhage; operating in the same way, and applying the same combination of styptic and caustic, as in the last-described case. The wound healed up very rapidly after the first large slough had come away, and notwithstanding the repeated application of the chloride of zinc. He experienced great relief from the operation, and left the hospital, after one month, very much improved in comfort and health. We have since seen him several times, and, so far, he has remained in a condition which may be considered satisfactory. The glands have not increased in size, and he can speak and masticate without pain, and is certainly improved in looks.

The last case I shall bring to your notice is a patient who has just come into the hospital from Somersetshire, in whom we find a well-marked and advanced specimen of the disease of the tongue under consideration. George B——, aged forty-five, a delicate-looking man you see, evidently much worn by suffering and interference with the taking of food. On opening his mouth you observe a very large, solid tumour, of the size of a hen's egg, embracing the whole of the left half of the tongue, commencing, as in all the other cases, opposite the bicuspid and anterior molar teeth. The surface of the tumour is covered with a deep ulceration, with raised, ragged edges and deep sloughy-looking fissures, and exhaling a very offensive odor and a putrid discharge. There is no affection of the glands apparent on careful examination; but the patient complains of deafness and much pain in the ear of the same side from implication of the gustatory and chorda tympani nerves. He has great mechanical difficulty in mastication and deglutition, and has at times lost much blood by hæmorrhage from the ulcerated surface of the tumour. Pain, want of food, and hæmorrhage have reduced him to this distressing condition.

The disease in this case commenced only in December last, and has pursued a much more rapid course than in any of the other cases before described. It began at the very first with ulceration, attributed as usual to a decayed tooth; and the substance of the tumour is very friable, easily breaks down, and bleeds profusely. To-morrow I propose to excise nearly the whole of the organ under chloroform. And as it is important in his present weak condition to prevent him losing blood as much as possible, I shall, before cutting away the tumour, pass a ligature across both the ranine arteries at the base of the tongue, and tie them up tightly before using the knife, applying the chloride of zinc and perchloride of iron freely after excision.

You may, perhaps, think that in such a case it would be preferable to excise the whole organ; and I think that, with the use of the gag which I shall presently describe, this could safely be done, under chloroform, through the mouth of the patient, without the necessity of performing the formidable operation practised by the late Professor Syme—viz., division of the lower jaw at the symphysis and wide separation of the rami; or that of Regnoli, of making a T-shaped incision into the floor of the mouth, and drawing the tongue through it before excising. But one-half of the organ is soft, mobile, and healthy; and I hold strongly that when an operation is to be done chiefly with a view to the relief of the patient's sufferings, and without much hope of its entire eradication, as in these instances, no more than is necessary of the healthy parts need to be taken away; and, moreover, that the disease is just as likely to return in the scar at the base of the tongue as in the side of it which is left behind. In fact, I consider that it is more important to cut away the tissue *deeply* in the direction of the course of the lymphatics—viz., towards the base of the tongue, than to cut *laterally* towards the healthy side, always supposing that the diseased parts are completely removed.

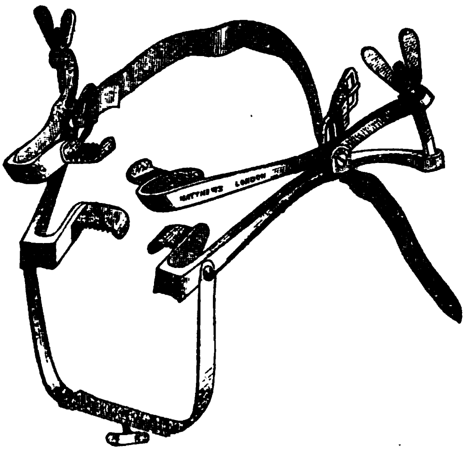
An important consideration in these cases, apart from the relief afforded to the patients, is whether the duration of this disease in the tongue previous to the destruction of life is so protracted as to hold out a fair prospect of lengthening the term of years and affecting relief to the patient by one or more operations. It is often said that epithelial cancer of the tongue goes more rapidly to its fatal termination than in other parts. This I certainly believe to be the case with respect to the lips, which offer but little impediment to the prehension of food, as in the frightful case now under the care of my eminent colleague, Sir W. Fergusson, in this hospital, or to the uterus, which is not directly concerned in the vital processes. But it is not true in the case of the œsophagus or any other part of the intestinal tract, where the mechanical impediments of the growth become an important element of destructiveness. With the exception of the last case of George B——, the disease has lasted already in the patients I have had under my care lately from two to four years. In other recorded cases it has lasted even much longer, progressing very slowly. In one case recorded by Mr. Holmes Coote in the "System of Surgery," the disease lasted twelve years, and in another sixteen years. In a case recorded by Mr. Ceely, of Aylesbury, under what I cannot but consider as the inappropriate title of "soft cancer," the disease, after being removed, had lasted thirteen years, and the poor patient was still alive. These have been, doubtless, cases in which a solid growth of closely adhering cells has been the originally prevailing and characteristic element of the disease. In other cases, where rapid disintegration and ulceration come on early, and form the most striking manifestation of the disease, it kills in much less time, as in that of the late Mr. Bransby Cooper, which lasted only ten months, and proved fatal from arterial hæmorrhage.

The conditions, then, of the disease which we have to consider in determining its treatment are these: a very painful and distressing course, wearing down the vital powers by constant suffering, by loss of rest, by interference with those processes of nutrition ministered to by the prehension and mastication of food, by the debilitating and offensive discharges; usually a sufficiently protracted career, from one to three or four years, giving time for the influence of treatment and the checks of operative interference; a less liability than in the more malignant forms of cell-growth to attack independently the internal viscera; and a more tardy course in invading the neighboring lymphatic system. All these seem to me in favor of an early and repeated application of operative measures, not so much, perhaps, with a view of ultimately effecting the cure of the disease, though the possibility of this result even must not be lost sight of, but, as in a great many other maladies which we have to treat, to avail ourselves of the most likely means to arrest and retard its progress; and, what is very considerably conducive to this, and at the same time is probably more welcome to the sufferer, to relieve his tormenting pains far more effectually than any combination of drugs whatever.

To remove the disease the least painful, most manageable, and most effective plan is, no doubt, by the application of the knife or scissors. Caustics in this situation work too slowly and superficially, and too painfully and uncontrollably to be valuable in any other way than as secondary to the absolute removal of the diseased mass. Used in this way they are of great value, and are always employed by me in the form of the solid or a saturated solution of the chloride of zinc, sometimes combined with the perchloride of iron, and frequently applied to destroy any remains of the disease which may have escaped the knife. The latter has also the additional advantage of stopping any bleeding which may still further lower the resisting powers of the patient to the invasion of the disease.

Up to the present time the operation for the removal of the tongue or any part of it has been considered by most surgeons as one of those in which the blessings of anæsthesia could not with safety be afforded to the patient, for reasons which will be at once apparent—viz., first, the removal by the anæsthetic agent of the voluntary assistance which the patient can render in all operations upon the interior of the mouth, by keeping the jaws and mouth wide open and the tongue steady; and secondly, the supposed danger, in profuse bleeding into the pharynx, of suffocation by the blood passing into the larynx and trachea. It is very evident that if we cannot in these cases offer to our patient a means of relief unattended by immediate pain, and that of a most severe character, at least half of the advantages of, and inducements to, the treatment involving such an operation are thrown away, and patients will be naturally indisposed to listen to the only promise of relief which can honestly be held out to them. If, then, we can render by any means the necessary operations painless, we confer, I think, a boon upon humanity not by any means small to the poor creatures thus afflicted.

By the use of the instrument which I now show you, made for me by Messrs. Matthews, we obtain a perfect and steady command over the jaws, without encroaching upon or interfering with the cavity of the mouth. (See figure.) It consists of four teeth plates, which act on both sides upon the upper and lower bicuspids by means of a pair of powerful cross levers, placed outside the cheek, and opening independently of each other by a screw on each side through a simple hinge joint. Combined with these levers as they bend into the mouth is a groove to receive the angles of the lips, which are thereby kept wide apart, and out of the way of the light, which is so necessary in securing bleeding arteries in a deep cavity like the mouth. The two sides of the instrument are connected and kept together by a bent piece of metal, which passes under the chin altogether out of the way of the operator, and which is capable of being adjusted to the width of the lower jaw by a sliding groove and fixed by a screw. The whole is held firmly upon the patient by a strap passing round the back of the neck and buckling on to the opposite piece. This instrument has been adopted after a great many trials of different kinds of gags, including that most valuable one for operations upon the palate invented by Mr. Thomas Smith, but which, from the embarrassing presence of the tongue-plate, proved inadmissible in operations upon the tongue. In some respects the present instrument resembles this, and the differences are mainly such as refer to leaving free the tongue and holding the lips well stretched out. I may add that the gag is equally effective in operations upon the tonsils and larynx, and that Sir W. Fergusson employs it, with entire satisfaction, in cases of staphyloraphy.



More difficulty than you would imagine has attended my efforts to get a gag that could be depended upon to resist the struggles of a patient under chloroform, and to meet the ridiculous ease with which the patient, by simply opening his jaws a little wider than the angle at which the gag has been fixed, and at the same time pushing with his tongue and cheeks, as in the act of spitting violently, which so commonly occurs under chloroform, can render nugatory all your precautions to secure a full command over his mouth. The ordinary dentists' gags are often more than embarrassing and worse than useless in these cases. One of the most frequent difficulties arises from the absence of the teeth so commonly drawn to prevent irritation of the tongue on the affected side in these cases. The independent action of the two sides of the gag just described enables one to be opened wider than the other to meet the extra width required by the absence of teeth. A one-sided gag is much more easily displaced by the patient and is more difficult to fix than a double-acting one.

Having thus obtained the necessary control of the movements of the patient's lips and jaws during the insensibility produced by chloroform, we have only to look to those precautions against bleeding and the passage of blood into the larynx which are usually found most efficacious, such as turning the head down to one side and clearing the throat with the finger. If the knife be used the tongue must be held well out of the mouth by a good-sized vulsellum forceps, with hooks sufficiently large and widely placed to prevent their tearing out of the sound fleshy substance of the tongue into which they should be inserted. If the tumour is likely to be very vascular, as may be in some measure divined from its appearance, I have usually passed first a stout ligature across the ranine arteries under the base of the tongue, by means of a curved needle in a handle,—the one, in fact, which I devised for the performance of the operation for the radical cure of hernia. This controls the hæmorrhage until the cut ends of the vessels have been secured. The tumour is next seized by a pair of stout double-toothed lion forceps, such as those most useful ones which I now show you, and which we know in this theatre as Wilcox's forceps. I have found these by experience to be less apt to slip than any other I have tried. Then a curved, sharp-pointed bistoury, to transfix the tongue close to its base, and to cut first antero-posteriorly and then laterally across the vessels and nerves, will suffice rapidly to clear away the diseased mass. In securing the arteries much steadiness on the part of the assistant is required. The muscular substance of the tongue is so friable under the circumstances that it is readily broken down by too much vigor in drawing the knot; while the depth at which the cut vessels lie from the surface necessitates dexterity in slipping the thread over the forceps, the points of which should be conical and taper quickly to the end. The proper tying of the vessels, and the prompt application of the tincture of the perchloride of iron and chloride of zinc by a strip of lint tied round a stick, will render the use of the actual cautery rarely necessary to subdue the hæmorrhage. It is astonishing how rapidly the patients recover after this operation; and, if too much blood has not been lost, how soon they regain strength and a more healthy appearance. Freedom from pain, undisturbed rest at nights, improved appetite, and revigorated digestive powers account for this satisfactory result.

Lectures

ON

THE SURGICAL TREATMENT OF ANEURISM

IN ITS VARIOUS FORMS.

Delivered at the Royal College of Surgeons, June, 1872,

By TIMOTHY HOLMES, M.A., F.R.C.S. Eng.,

Professor of Surgery and Pathology to the College.

LECTURE III.—PART II.

IN Professor Lister's well-known tract "On Ligature of Arteries on the Antiseptic System," he produces evidence which in his belief justifies the following opinion:—"It appears, then, that by applying a ligature of animal tissue antiseptically upon an artery, whether tightly or gently, we virtually surround it with a ring of living tissue, and strengthen the vessel where we obstruct it. The surgeon, therefore, may now tie an arterial trunk in its continuity close to a large branch, secure alike against secondary hæmorrhage and deep-seated suppuration, provided always that he has so studied the principles of the antiseptic system, and so carefully considered the details of the mode of dressing best adapted to the particular case in hand, that he can feel certain of avoiding putrefaction in the wound. For my own part, I should now without hesitation undertake ligature of the innominate, believing that it would prove a very safe procedure."

Now, I have purposely avoided speaking about the principles of the "antiseptic system" as laid down by Professor Lister,—not that I do not recognise the great interest of the question, but because I have neither the time nor the materials required for its adequate discussion. But I will not refrain from saying, that whether Mr. Lister's explanation of his method of dressing wounds be or be not the correct one, that method is very frequently (I will not say uniformly) successful in producing rapid union, especially in the deep parts of the wound; and if this be granted, which I think no surgeon will deny who will trouble himself to obtain practical experience of wounds so treated; and if it be also granted (which cannot be denied, for it is a matter of demonstration) that the catgut ligature, under favorable circumstances, will gradually melt away in the tissues without setting up ulceration,—how can it be denied that an artery tied with this substance, and so treated as that the deep parts of the wound may rapidly consolidate, has a chance of that union by first intention which John Hunter and his followers sought after. Such rapid union is impossible in the ordinary method, where a silk thread is keeping up irritation and ulceration in and around the tied artery, and propagating inflammation from the bottom to the surface of the wound.

In my own case of double distal ligature, the wounds were not treated in any special manner. I did, indeed, dress them immediately after operation in the manner recommended by Mr. Lister; but as the man's restlessness during the ensuing night disarranged all the dressings, and the air had been freely admitted to the wounds for many hours before I saw him next day, I discontinued these dressings, and applied merely wet rags. The exact condition of the tied arteries is shown in the preparation and in the drawings before you. You will notice that the carotid artery was obstructed only by a small quantity of clot, which has fallen out of the vessel in making the section. That the obstruction, however, was really complete during life was proved by the fact that only the faintest pulse was at any time perceptible in the temporal artery, and none at all for a calendar month after the operation: so that there could have been no premature softening of the ligature and reopening of the channel of the artery, such as occurred in Sir P. Crampton's case, and in a case where Mr. Spence tied the carotid; and there can be no doubt, I think, that, had the man lived a little longer, a permanent diaphragm would have replaced the clot in the carotid, and its condition would then have resembled that of the subclavian. The latter vessel is closed entirely by an obstructing material, or diaphragm, which is accurately represented in this drawing, and which to the naked eye resembles a fibrous material, but which I have not had examined with the microscope, in order not to interfere with the exact condition of the artery. The diaphragm seems not to be more than about a quarter of an inch in thickness.

This case then affords definite anatomical proof of that which we might have inferred from the experience above referred to of Mr. Lister, Mr. Bickersteth, Mr. Heath, and myself—namely, that it is possible to tie a large artery in the human subject in such a manner that the wound may unite by first intention, and the patient never be in any danger of secondary hæmorrhage. And my case shows further, what no experience unconfirmed by dissection could show, that the catgut ligature may be removed by absorption, the vessel remaining undivided.

Now in this latter particular our experience differs from that of Porta. He, as is well known, instituted most laborious experiments on the action of the catgut ligature in the lower animals, besides practising the operation on the human subject to which I have referred; and he came to the conclusion that in all cases a ligature, of whatever kind, tied tightly round an artery, produces a softening of its cellular (or external) coat, and the division of the vessel by a process which he distinguishes from one of ulceration, though the nature of the distinction is not made clear. The exact nature of the process, however, is immaterial to our present purpose. The only point of importance is that in Porta's operation the arteries were always divided, and when divided secondary hæmorrhage is of course probable. In my operation they are seen to be undivided, and this is, I believe, the rule when arteries are tied in this manner. Now if the external coat remain undivided secondary hæmorrhage is only possible in case of such small apertures as the carotid exhibited in my patient, and these can only give rise to hæmorrhage when the tissues in the neighborhood are suppurating. If the latter remain firmly consolidated the only result will be (as that preparation shows) the extravasation of a minute drop of blood.

The differences which exist between such operations as I have described with the carbolised catgut and those which are related in Porta's great work are obviously two—the different preparation of the catgut, and the more rapid union of the deep parts of the wound. With respect to the first particular, no material can be imagined better adapted for a ligature than catgut long steeped in carbolised oil; it is perfectly tough, perfectly flexible, and perfectly smooth. It excites no irritation, absorbs no putrescent fluid, and though it gradually melts away, yet holds the artery firmly enough to close its tube permanently. Having used it in all large operations for about two years I can testify to the latter fact, and I can say that I have never seen any sign of irritation produced by it, nor witnessed the escape of any of the little knots from the wound, and that I hold it to be a very much better and more convenient hæmostatic than torsion, and infinitely superior to acupressure. In fact, I have found it to possess all the comfort of the silk ligature without its disadvantages.

If catgut is imperfectly prepared, it has not all these good qualities. If it be softened too much (as may easily be done when it is steeped in water), it will relax, and not hold the vessel permanently closed. If it be too little softened (as may easily be done when moistened in the mouth), it is very probably too harsh and irritating, and may be absorbent.

But I do not imagine that catgut, or any other ligature, can be applied to an artery with perfect success, by which I mean so as to close its tube yet not interrupt its continuity, unless the artery itself is kept as much as possible free from inflammation—that is, unless the tissues around become rapidly coagulated by first intention. An artery exposed in the middle of a suppurating cavity will, I believe, always soften and give way; and it was in all likelihood because they failed to secure this prompt union of the deep parts of the wound that Porta in most of his experiments and operations, and Sir A. Cooper in the operation which he performed after his first successful one, failed to obtain this perfect result. At any rate, the fact that such results are possible must have its bearing on the question whether Brasdor's operation in its integrity is or is not applicable in aneurism of the innominate artery. For I think we have in the facts I have produced ample evidence that if the carotid and first part of the subclavian are obliterated, the coagulation of the entire tumour is the natural result; while the ligature of the carotid and third part of the subclavian, though it may possibly produce a cure, cannot be certainly trusted to do so.

In those aneurisms, then, of the innominate artery which appear to be advancing after distal ligature of the carotid, the surgeon is justified, I think, in proposing to tie the subclavian in the first part of its course.

The diagnosis between innominate and aortic aneurisms is never easy, and in some cases it is impossible. It is also impossible to say whether, in an aneurism judged to be innominate, the aorta will be found implicated or not, or to say whether the aneurism is of the lateral or the peripheral form. I cannot stop to discuss these questions, especially as I do not know that I could add anything to the rules of diagnosis which are laid down by other inquirers. Nor do I know that it is very necessary; for I believe that the indications for operation are rather to be drawn from the progress of the case than from the opinions (necessarily uncertain) which we may form on the above heads. If an aneurism which affects the innominate artery is definitely extending towards the trachea and up the course of the carotid, its growth in that direction will almost certainly be checked by the ligature of the carotid. And all the more sure should we feel of this result if we could feel confident that the aorta was unaffected, and the aneurism of the sacculated or lateral variety. The definiteness of the bruit at a certain point and its becoming weaker as the stethoscope is removed from that point, the absence of any bruit near the valves, the difference of pulse on the two sides, and the absence of the general symptoms of aortic enlargement or tumour near the arch, are the main features of such a favorable case.

With respect to the ligature of the left carotid for aneurism of the arch of the aorta, I have no doubt that there are cases in which it is very beneficial, though there are no theoretical grounds for believing that a cure can be effected by this operation. The cases or Rigen and Tillanus, which are those most generally cited, are by no means sufficiently definite to afford any proof of the kind. Pirogoff's successful case is incomplete. Morrison's, though greatly benefited for a time, ultimately proved fatal; and Mr. Heath's, though here also the temporary benefit was most striking, is yet under observation. Nevertheless, in all these cases the relief from the operation was so marked as completely to justify the course pursued; and in a case such as that of Mr. S. Lane, where the aneurism appears to be wholly or almost wholly carotid, there is no doubt that the operation is urgently indicated.

I conclude from the analogy of the other cases of distal impaction which I have produced, that the benefit in these instances depends upon diminution of the size of the tumour consequent upon the obliteration of that part of the sac which was previously distended by the current of blood passing into the left carotid. The operation, therefore, holds out definite prospects of benefit in all cases in which the growth of the tumour can be traced upwards, towards the trachea on the left side, and, by parity of reasoning, the distal operation on the right side is equally indicated when the growth is on the opposite side of the windpipe; and it is in this way that I explain the benefit which followed the ligature of the right carotid and third part of the subclavian for aortic aneurism in Heath's case of simultaneous double distal ligature. In many other cases of thoracic aneurism growing upwards, the relief of spasmodic dyspnœa and the subsidence of the tumour, immediately after ligature of the carotid, appear to be explicable only in this way.

Another method of treatment which has been much employed of late years in aortic and mixed aortic aneurisms is galvano-puncture. In the case which I have already mentioned as having been treated by me with the double distal ligature, and which was mixed aortic and innominate, this treatment was employed with fatal results, but at the same time, as the preparation and drawing of the case show, with the effect of producing a very large amount of coagulum in the sac.

In fact, it may easily be shown, by direct experiment, that the constant current passed through any albuminous fluid, such as blood or white of egg, will generate an amount of coagulum in it which varies according as the positive or negative pole is used, and of course also according to the strength and duration of the current, the positive pole producing rapidly a firm albumino-fibrinous coagulum, and the negative a large frothy mass formed of minute coagula mixed into a foam by the gas which is generated in the decomposition of the blood, and around which coagulation may extend. The following experiment may be quoted from a tract by Dr. J. Duncan and Dr. Fraser, on "Electrolysis in Aneurism":—

"The left femoral artery of a large retriever mastiff was exposed and divided, and into each portion a nozzle, connected with the india-rubber tubing of the glass aneurism, was inserted. The entrance of blood was easily controlled by attached clips, and a temperature a little above 85° Fahr. was maintained by placing the globe in a hollow water-bath. On opening the clips, the blood flowed freely into the aneurism, and, as soon as the contained air had been expelled, the galvanic circle was closed by means of a key. Gas-bells in a short time rose to the upper part of the aneurism, and surrounded the electrodes which were there situated, and through the glass wall we could readily observe frothy masses slowly growing and accumulating round each pole, from which now and then a very small cluster of bells detached itself and was carried off—first in those irregular transits which marked the course of the blood-stream in our aneurism, and then suddenly down the exit tube to enter the distal portion of the divided blood-vessel. We had already found that in this apparatus the blood might be expected to coagulate spontaneously, in a very few minutes. After eight minutes, pulsation was yet perceptible in the exit tube, when the galvanic circuit was opened, and the entrance of blood stopped by closing the india-rubber tubing. The electrodes were withdrawn, and on unscrewing the hemispheres a little fluid blood was found, and a large coagulum, possessing nearly the exact configuration of the interior of the glass aneurism. At the surface two round holes were seen, which had been occupied by the electrodes. Immediately surrounding the orifice of the positive pole was a black ring, and beyond this a firm clot with entangled gas bubbles. The position of the negative electrode was surrounded by a quantity of froth intermingled with clotted blood. Both were perfectly distinguishable from the normally coagulated blood, and both possessed greater consistence and tenacity than it. A section was made through the globular clot at the points where the electrodes had entered it. It was found that the electrolytic clots extended an inch into the mass, in a conical form. The negative had a diameter of 0·75 inch at the surface or base, and had a markedly alkaline reaction; the positive was 0·5 of an inch at its base, and was strongly acid. A few black lines were also exposed in this section, which were apparently connected with the positive electrode, and proceeded from it in wavy and fantastic courses, to be lost in the general blood mass. A microscopic examination of the blood revealed many of those curious marginal modifications of the corpuscles which Rollet and Neumann have fully described."

Experience has also shown that this electric action, this coagulation of the mass of the blood in which there is no mechanical separation of the fibrine from its other constituents, is capable of producing the total and final cure of an aneurism—an additional proof, if an additional proof were required, that the slow method of coagulation, by which what Broca denominated "active" clot is generated (i.e., the fibrine separated from the other constituents and deposited in laminæ), is not the only, though it may be the safest, way of curing an aneurism. In proof that the clots produced by electricity may be as efficacious in the cure of an aneurism as those produced by other means, I will read Mr. Bland's summary of Abeille's case of the cure of an aneurism of the subclavian artery, to which I shall have hereafter to refer again when speaking of the treatment of subclavian aneurism.

"Case 60.—Size of hen's egg. Under etherisation, four steel needles were made to traverse the tumour to a depth of three-quarters of an inch, and the galvanic pile put in action for not more than five minutes' contact at a time. For about a minute the patient remained insensible, but then immediately cried out and experienced convulsive movements in all parts of the body. The arm of the affected side became especially and extremely violently convulsed. The tumour soon began to diminish, and became more tense and resisting; pulsation in it gradually subsided, and the radial pulse reappeared [? disappeared]. This condition lasted thirty-seven minutes, the patient being in a profuse sweat; and no operation ever caused such torture. When the tumour had become hard and deprived of all pulsation, the needles were withdrawn and cold applications applied, a compress of one kilogramme weight was placed above the tumour on the course of the artery, and maintained for ten hours. On the following day there was slight vibration in the tumour; the radial pulse had completely disappeared; the whole arm, forearm, and hand cold and engorged, and with sensations of pain and tingling. Venesection on fourth and fifth days; motions of the fingers lost. On the third and fourth days the radial pulse reappeared, but very slightly, and she had intense cephalalgia, requiring venesection, repeated also on the following day. The eschars became detached from the site of the puncture, with but little suppuration. From the fourth day the tumour decreased, and on the twelfth had diminished to one-half the size. On the thirty-seventh day the tumour was no longer prominent, and it entirely subsided at the end of three months. The case was seen two years afterwards; there was no reappearance, and the pulse was rather smaller than on opposite side."

On the other hand, it must be allowed that there is at present much uncertainty as to the value of the method, and great ambiguity as to the immediate effects of any given operation; that those most conversant with the electrical processes and their effects when applied to the living body are far from being agreed as to such important details as—which pole it is best to use; what is the best apparatus; whether coagulation should be produced rapidly or slowly; whether the action should be long continued or not, frequently repeated or almost solitary; and whether the metallic needles should or should not be coated over to defend the superficial parts from the electric action. But such details will be gradually cleared up as our experience of the method increases. As far as my limited practical acquaintance with the subject goes, I should incline to the belief that the action of both poles is desirable, and that the positive pole ought not to be used too long, especially if the battery is a powerful one; that a moderate action should be employed, and that it should be continued for a considerable time (under anæsthesia), until a very decided physical effect has been obtained; and that the soft parts covering the tumour should be defended by coating the stalk of the needle with vulcanite.

We must admit further, in forming an estimate of the value of electricity, that the coagula formed by it appear very frequently not to be stable, but to be liable to melt down again into the blood-stream. At any rate, it seems a frequent phenomenon in this method of treatment to find an aneurism cease to pulsate for a time, and the pulsations to recur very shortly afterwards. Thus, a short time since, with Dr. Althaus's assistance, I applied the galvanic current to a patient of Dr. Ballard, suffering under aneurism of the arch of the aorta. After several previous ineffectual applications of the negative pole, we introduced both poles into the blood of the sac, and kept up a mild continuous current for a considerable time. The effect was considerably to reduce the pulsation and to produce much consolidation. This was very visible in the more superficial parts of the tumour, and particularly in one point where a prominence of the sac had so closely approached the skin as to produce those suggillations of bloody serum which immediately precede the rupture of the sac. The patient experienced considerable relief, and what was very remarkable was, that after he had fully recovered from the anæsthesia under which the operation had been performed, he was astonished to find that the tumour had ceased to beat. As he had been distressed night and day for more than a year by the beating of the great tumour in his chest, it seemed impossible that he could be mistaken; and his wife (who, as well as himself, is a person of intelligence) confirmed his assertion that no pulsation could be felt in the mass. After two hours' quietude a fit of coughing appeared to reproduce the pulsation, but it subsided again for an hour, after which it was reproduced, and when seen next day the pulsation was only a little less than before the operation.

Again, in a case of subclavian aneurism under Dr. Clifford Allbutt's care, to which I shall again refer, the pulsation was quite suppressed for a day or two, and then slowly returned.

Many other instances might be cited of the same fact, while on the other hand there have been cases in which the coagula produced by electricity seem to have been as stable as those formed naturally. This seems to me not so much an objection to the method in itself as a proof that we do not as yet know enough of its detail. The uncertainties and ambiguities referred to appear to me, however, to be a rational and proper ground for discarding electro-puncture, at any rate in the present state of our knowledge, in cases of external aneurisms situated on accessible arteries, where the ordinary processes of pressure and ligature offer safer and more reliable means of cure.

But no one can have read the literature of this subject, even if he has had no personal experience of the method, without convincing himself that by appropriate manipulation, coagula may be produced, in the part of the sac which is acted upon, without any grave inflammatory reaction, or any very serious risk of hæmorrhage, of embolism, or of gangrene opening into the tumour.

Original Papers.

THE RELATIVE FREQUENCY OF DISEASE BETWEEN THE RIGHT AND LEFT SIDES OF THE HEART;

DEGENERATION OF THE HEART, ITS CAUSES AND MEANS OF AVOIDANCE.

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(*Continued from November No.—p. 580.*)

Heart of newly hatched trout—How affected by the negation of oxygen and an increase of carbonic acid—Influence of carbonic acid upon the contractile fibre of the heart—Effects on the human heart of pre-breathed air—Defective ventilation a frequent cause of sudden death in heart disease—Importance of pure air in cardiac degeneration.

The object of my last communication was to show that the relative frequency of disease between the two sides of the heart depended, as a rule, upon age; that the left side of the heart was the one to suffer before the age of forty, the right side of the heart after that age. It was also shown that diseases of the left side of the heart are generally the result of inflammatory action; diseases of the right side of the heart, of tissue-degeneration, or of mere mechanical influences, as of backward pressure of blood upon the right cavities of the heart from obstruction to its flow through the branches of the pulmonary artery.

It was further stated that, according to the ordinary law of life, tissue-degeneration, or natural decay, usually begins at the age of forty; that, with its advent, the left side of the heart becomes less prone to inflammatory disease, and approaches more and more closely in its tendencies to the diseases of the right side of the heart; and that the degenerative tendency of the latter is, after this age, greatly influenced by the three great vital functions—the action of the brain and nervous system, respiration, and circulation.

It was thus shown that, to maintain the heart's structures in as healthy and sound a condition as they could possibly be maintained under the law of natural decay, these vital functions must neither be overworked nor fall short of their necessary duty—that, in fact, regular, natural action is their law of life.

I showed the effect of impure air in promoting the degenerative tendency in the structures of the heart, and especially in those of the right side of the heart, after the age of forty. I was thus led to a passing consideration of the baneful influence produced upon the heart by badly-ventilated houses, schools, manufactories, pits, theatres, underground railways, and all places of a similar character.

To this point I return, to show the influence of an increasing quantity of carbonic acid in the air respired upon the contractility of the muscular fibres of the heart.

I take for my example the newly-hatched trout.

During the winters of 1869–70 and 1870–71, I hatched some thousands of this fish, many of which I daily submitted to microscopic examination. The result of my investigations, in reference to the action of the heart and to the influence upon it of a decreasing quantity of oxygen and an increasing quantity of carbonic acid in the water in which the fish was confined, shows,—

That, on placing the fish in a glass trough containing a quantity of water, the heart is seen, under the microscope, to be affected in the following manner:—

In the first few moments of examination the venous blood, collected by the veins from the head, back, and yelk-bag (the first two of which unite to form a bulbous vessel into which the third opens), is seen to be projected with considerable force and rapidity into the upper (auricle) of the two cavities of which the heart is composed, and thence as instantaneously into the lower (ventricle) cavity, which contracts with equal rapidity, and forces the blood into the branchial artery, which conveys it to the gills. The projection of the blood into the auricle, its passage into the ventricle, and its expulsion therefrom, are but the work of an instant. As the blood enters the auricle both it and the ventricle seem to anticipate the charge of blood; but especially is this the case with the ventricle. Before the blood well touches the valve which guards the entrance from auricle to ventricle, the latter is observed to shorten its longitudinal diameter, to visibly meet, as it were, the coming charge of blood from the auricle, and to force it instantly into the branchial artery. There is no delay whatever of the blood in the auricle or ventricle. It is shot in a straight line from the vena cava through the auricle and the auriculo-ventricular valve, caught by the contracting ventricle, and deflected and forced, without a moment's delay, into the branchial artery.

From these observations it was evident that the contraction of the heart was not excited by the distention of its cavities, but that it was induced by the mere impingement of the blood upon its lining membrane. In contracting the ventricle was seen to roll about one-third upon its axis, by which a portion of that part of it which was previously out of sight was brought into view. As soon as it had delivered its blood into the branchial artery, it relaxed, and increased again its longitudinal diameter, recoiling from systole with an energy and a rapidity equal to those of its contraction.

In three or four minutes the heart was observed to contract both less quickly and less energetically. A very short time after this the blood can be seen gently pouring into the auricle, and thence into the ventricle, which latter now allows itself to be about one-fourth filled before it contracts. It now expels its blood, and again dilates; but its dilatation, like its contraction, is not so instantaneous as it was when first observed under the microscope. In a short time longer the sensibility of the heart is greatly diminished, for the blood is seen to be accumulating in both the auricle and ventricle, but especially in the former, from both of which cavities it is now only partly expelled by the contraction of the heart.

At length, just before death, the blood is seen to flow from the auricle into the ventricle, thence into the branchial artery and along it, the heart being passive during this time, and only now and then at long intervals manifesting a very slight and partial contraction. During the whole of this time the blood is coagulating more and more in the auricle and ventricle, but especially in the former; and when at length the heart has ceased to beat, the auricle and the vena cava opening into it are fully distended, whilst the ventricle is only partly distended with black-red blood. In the last moments of life, after the heart has ceased to beat, the branchial artery is seen to be pushing forward its slender current of blood, and to become at length quite empty and transparent.

Here, then, as the oxygen dissolved in the water in which the fish is confined becomes exhausted, and as the carbonic acid increases, the sensibility and contractility of the heart are diminished, and at length entirely destroyed. The negation of oxygen, and the increase of carbonic acid, have culminated in the death of the fish.

Precisely the same effect is produced upon the human heart by an accumulation of carbonic acid in the air respired.

In the ordinary condition of the atmosphere, in which carbonic acid does not exceed one part in a thousand parts of that medium, its effects upon the heart are inappreciable. When, however, the carbonic acid has accumulated to the extent of one per cent. of the air respired, it begins to produce a slight feeling of faintness, and some degree of uneasiness across the brow. At 2 per cent. the heart's action is quickened, the sense of faintness is greater, there is slight giddiness, with heaviness and constriction of the head, together with nausea. At 3 per cent. all these symptoms are increased. There are vertigo, fluttering of the heart, nausea and sickness, followed by an overwhelming sense of muscular prostration. At this moment the contractions of the heart become very feeble, the skin relaxes, and is bedewed with a cool clammy perspiration. These symptoms deepen with the increasing quantity of carbonic acid in the air respired until the utmost limit of toleration is reached, beyond which life can no longer be maintained. At this stage lethargy supervenes; and, at the moment of its occurrence, the heart begins to beat less frequently, and much less powerfully than before. This condition is the parallel of that observed in the young trout, when the blood begins to accumulate and to coagulate in the auricle and ventricle, and when the heart's sensibility and contractility are reduced in the greatest degree.

From these effects it is certain that confinement to an atmosphere impregnated with carbonic acid, even to the extent of one per cent. only, quickly deranges the function of the heart, and ultimately deteriorates the tissues themselves of that organ.

The greater the percentage of carbonic acid in the air respired, the more quickly and the more profoundly are these effects produced. The constant breathing of air containing one per cent. only of carbonic acid proves fatal to life; but if it is respired for a short time only, functional disturbance alone is then and there produced. *It is however, certain that in this functional disturbance lie the germs of organic mischief, and that frequent repetition of the cause will undoubtedly end in organic disease. Hence the impure atmosphere of the bedrooms of the poor, and, indeed, of many of the middle class, proves a sharp spur to the degenerative tendency manifested by the heart, and especially by the right side of the heart, after the age of forty.* Such bedrooms are generally small in their superficial area, low-pitched, and often lighted by a diminutive window, which at night is kept constantly closed; and having a door which opens to the interior of the house, but which is also closed during the occupation of the rooms. Nay, to prevent the slightest admission of fresh air, the crevices of both window and door are most carefully stopped; and to render the matter still worse, a fire is not unfrequently kept burning during the winter nights.

What must be the effect produced upon the air of such rooms under the conditions named? I take for example an average-sized bedroom in the cottages of the poor—say, a room twelve feet long, ten feet wide, and eight feet high. This gives a cubical space of 960 feet, which is not more than half the cubical space allowed each patient in our best arranged hospitals. In this room, with its diminutive window and door constantly closed, three, four, and frequently a greater number of persons pass the night of eight or ten hours' duration. No provision has been made for the admission of fresh air—none for the escape of the carbonic acid exhaled during respiration. What little provision did exist in the crevices formed by the badly-fitting door and window has been carefully abrogated by sand-bags, rolls of rags, and stuffing of every description. Thus the air of the room becomes poisoned with carbonic acid, and in this condition it is breathed and re-breathed, to the manifest injury of the heart.

A simple calculation will enable us, if not to determine with exactitude, at least to approximate to the amount of carbonic acid exhaled by each sleeper, and consequently to the degree of vitiation which the air of the apartment undergoes. I fix the number of respirations at its minimum—14 per minute; the quantity of air exhaled at each expiration at 20 cubic inches; the quantity of carbonic acid contained in the expired air at 4 per cent.; and the duration of the night at 8 hours. Hence, $14 \times 20 \times 8 \times 60 = 134400$ cubic inches, or 77·77 cubic feet, of air expired by each sleeper during the night of eight hours. This expired air contains 4 per cent. of carbonic acid. $\therefore 100 : 77{\cdot}77 :: 4 = 3{\cdot}11$ cubic feet of carbonic acid exhaled by each sleeper during the night. Suppose the number of sleepers occupying the bedroom to be four, this will give $3{\cdot}11 \times 4 = 12{\cdot}44$ cubic feet as the quantity of carbonic acid which is exhaled by the four sleepers during the eight hours of night. The room itself contains 960 cubic feet of air, and through this 12·44 cubic feet of carbonic acid would have been diffused by the termination of the night. It therefore follows that, if no fresh air entered the room, and if in consequence the carbonic acid had no means of escape, the air of the apartment would, at the end of the eight hours of night, contain 1·29 per cent. of this gas: a quantity sufficient to produce serious results.

This statement, however, does not represent all the facts of the case. It must be remembered that the oxygen contained in the air of the room would be constantly undergoing reduction by respiration during the night. If the quantity thus consumed were determined from the quantity of carbonic acid exhaled, allowing for the fact that 15 per cent. more oxygen is taken into the blood than is contained in the carbonic acid of the air expired, it will be found that from one-third to one-half of the oxygen originally contained in the air of the room would have been consumed by the end of the night. This reduction in the quantity of oxygen, and the great increase of carbonic acid, would affect the body in two ways—firstly, by a deficiency of oxygen, and secondly, by an excess of carbonic acid, in the air respired. Hence the reduction of the one and the increase of the other would render the air far more injurious than if only one of these changes in its constitution had taken place.

The actual result is not, however, in strict accordance with this calculation, because fresh air, although in limited quantity, does find its way into the room, and carbonic acid does, to a limited extent, find its way out. These, therefore, would modify the constitution of the air of the room at the close of night; but they would still leave it with an excess of carbonic acid injurious to life.

It is found that when air moderately impregnated with carbonic acid is inspired it greatly impedes the exhalation of more from the lungs; and that the greatest quantity of carbonic acid which exists in pre-breathed air never exceeds 10 per cent. It is much to be feared that to this degree of vitiation the air of the bedrooms of the poor and of others not unfrequently rises by the too prevalent system of excluding fresh air, and by the frequent absence of provision for the escape of that which has already passed through the lungs.

Can it then be a matter of surprise that death from diseased heart should so often occur during the night?

In thousands of instances of cardiac disease life is thus sacrificed, where had but proper ventilation of the bedrooms been observed, the subjects of such disease might, despite the cardiac mischief, have continued to live for an indefinite time.

It has frequently been my duty, during a practice of nearly thirty years in the midst of a large community prone by the habits and particular avocations of the people to heart disease, to investigate cases of "found dead" in bed, and I have often been compelled to refer the immediate cause of death to the effect of carbonic acid liberated by respiration and confined to the apartment, in destroying the sensibility and contractility of the heart, rather than to the direct influence of the diseased heart itself.

I remember that on one occasion I was summoned to a case which had occurred in a bedroom 15 feet long, 12 feet wide, and 8 feet high. In this room, with the door and window closed, no fewer than twenty persons slept night after night! Can anyone doubt that the air of such a room would be charged to excess with carbonic acid exhaled by respiration? Those who perished in this manner were beyond the age of forty; and, in every instance examined, the right side of the heart was either primarily affected by tissue-degeneration, or by disease consecutive to mischief in the left side of the heart and lungs.

Often indeed in the dwellings of the middle and higher classes of society the provisions for ventilating both their bedrooms and their day-rooms are miserably inadequate to preserve health. The consequence is that cardiac disease is promoted to an inconceivable extent. *There is no other disease in which the demand for cold, fresh air is so urgently pressed by the patient as in cardiac disease. There is none in which a constant supply of pure air is more needed—none in which it is more grateful to the patient, or in which it has a more immediately beneficial effect.* At all times and seasons—in the depth of winter—by day and by night—a patient suffering from a paroxysm of cardiac asthma will hurry to the open door or window, and there, with his body hanging half out, will remain, with scarcely any vestments upon him, breathing the cold air until the paroxysm has ceased. Ought not this urgent, this powerful supplication of nature to teach us the importance of ventilation, and of a full supply of fresh air in the treatment of heart disease?

I hesitate not to say that free ventilation—the free admission of pure air into the apartment by day and by night—is one of the most important remedial measures which can be adopted in the treatment of this disease.

Where this means is defective, but where, nevertheless, the vitiation of the air of the bedroom does not exceed 1 per cent. of carbonic acid, a sensible effect is produced upon those who have slept within its influence. They complain, on leaving their bed, of weakness; their limbs tremble; they feel somewhat giddy, and their heads feel heavy, or it aches. The least effort disturbs the heart's action, which is somewhat quick and feeble; their countenance is pale; the lips are not unfrequently somewhat blue; and the tongue is covered with a thin, whitish, and somewhat slimy fur. The appetite is in abeyance; there is a feeling of nausea; and the first evacuation is generally dark in color.

What is the pathological condition of such patients at this moment? Simply this. The blood contains an excess of carbonic acid, which circulating with the blood through every organ, disturbs the natural action of every organ, blunting its sensibility, vitiating its particular function, and interfering with those molecular changes which constitute healthy nutrition.

A person thus effected does not usually die. The body, removed to a pure atmosphere, begins at once to excrete the carbonic acid by the lungs, the liver, the skin, the kidneys, and the bowels, and in the course of a few hours the more visible manifestations of its baneful effects have passed away. It however often happens that a sense of weariness and muscular debility is felt for days afterwards. Night, too, frequently places such subjects in the same condition as before. The same bedroom is occupied; the same inadequate means of ventilation continue; the same accumulation of carbonic acid takes place; and the same effects upon the bodily organs are repeated. Blood charged with an excess of carbonic acid again pervades every tissue of the heart, diminishing its vitality, lowering its sensibility, and assimilating its nutrition to that of the reptillian heart. But the low character of the nutrition of the reptillian heart does not accord with the comparatively quick circulation, rapid nutrition, vital power, and energy of action required by the human heart. The one cannot be substitued for the other. In man the change results in disease where disease does not exist—aggravate disease where it is already present.

To point out the particular changes which are thus produced in the tissues of the human heart will form the next subject.

Changes produced in the muscular fibres of the heart by hyper-carbonised blood—The carbonised heart—Its effects upon the respiration and upon the system generally—Modes in which it produces death—Indications of its existence.

The effects of imperfect aeration of the blood are traceable in the tissues of the heart. They vary, however, according to the degree of impregnation of the blood with carbonic acid and to the length of time which such blood has circulated through the nutrient vessels of the heart.

Before tracing these effects, it may be observed that those in whom the muscular fibres of the heart have, in consequence of confinement to close, ill-ventilated apartments, undergone the particular degeneration in question, and in whom the blood retains a greater than normal proportion of carbonic acid, are never well. It would be impossible, until somewhat late in the disease, to put the finger upon any one organ and be able to say, Here is the disease. Still, every organ is depressed in its functional manifestation, and the whole body is changed in its sensibilities, its impressions, and its powers. The prevailing indications of disturbed health are—extreme muscular debility, inaptitude for exertion, depression of spirits, frequently an exquisite sensibility of the cutaneous nerves, feebleness of the pulse, variable appetite, and weak digestion. The bowels are generally sluggish, and their evacuations darker than natural, from the presence of almost black bile. Such, too, is often the condition of the blood and of the containing vessels, that a slight bruise of the skin produces an ecchymosis of unusual extent, which is exceedingly slow in being absorbed.

All such patients occupy in the daytime rooms from which they exclude every breath of air, provided they have the means of doing so; and where poverty prevents them from fully gratifying their sensations, they nurse themselves accordingly. At night, the one who is able to command the "comforts of life" retires to a feather bed surrounded by curtains closely drawn together, with door and windows closed, gas flaring, and fire burning; the other, whom poverty oppresses, betakes himself to a small, low, dark, ill-ventilated room, where, with his wife and four or five children, huddled together in a couble of beds, he passes the night in an atmosphere rendered poisonous before morning by the accumulation of carbonic acid.

Years pass in this condition of health; middle age is reached; it is passed; œdema of the feet at length sets in; general dropsy follows, from which the passage to the grave is but short. A short time before death occurs—a little before or about the time of the commencement of the œdema of the feet,—tricuspid incompetency and jugular pulsation are detected. At length the patient dies, a post-mortem examination is made, and the following condition of the heart is found:—

It lies in its pericardium surrounded by serum, apparently somewhat enlarged, the enlargement being due to dilatation of the ventricles, but of the right ventricle chiefly. Its superficial veins are large and distended with black blood; and the general surface of the heart is of a dull, slaty-red color. This color pervades the entire thickness of the heart's walls and the endocardium throughout both auricles and ventricles. On being handled, the heart is found to be soft, flabby, and seldom indeed above the average weight. The walls of the right ventricle are larger but thinner than natural; and occasionally the left ventricle is also reduced in thickness. The curtains of the tricuspid valve, on being rubbed between the thumb and finger, are found to have lost their natural feel, and the impression of softness and laxity of tissue is conveyed. They no longer meet so as to close their respective apertures; for, although their edges may be so fitted and approximated as to render their closure apparently complete, yet when the heart is placed as in its natural position, and water is poured, however gently and however small in quantity, into the right auricle, it passes at once into the ventricle beneath. Less frequently the pulmonic valves manifest the same condition of incompetency—always a similar want of firmness. The same distension of auricle and ventricle by dark blood as was observed in the dying trout, is here found; but as in the one so in the other, the auricle is more distended than the ventricle.

If now thin sections from the muscular walls of the heart be examined under the microscope, the following important changes are observed:—The minute veins lying on the outer surface of the muscular fibres are distended with dark blood; the muscular fibres themselves are less firmly and less closely knit together, and show a disposition to separate into their ultimate fibrillæ; whilst the cells or discs of which these fibrillæ are composed are, in like manner, less cohesively bound together. Instead of forming apparently flat discs or cells placed face to face in close linear arrangement, it is found that they are increased in their longitudinal diameter, and that they have in consequence assumed somewhat of an oval shape. The effect of this change seems to be to increase the distance between the transverse striæ, and to render the latter much less distinct than in the healthy fibre of the heart. Here and there along the course of a fibre small fissures or rents are observed, through which the sarcous element protrudes, enabling the observer, by the eversion of the edges of such fissures, to discover a containing sheath or sarcolemma, as in the voluntary muscular fibre of other organs. In the course of the various fibrillæ of which a fibre is composed, one or more of the discs have assumed a round, cell-like form, which in most instances is distended with a dark-looking serosity, but which in other instances contains a clear, bright, highly refracting substance. The former fluid is a portion of the serosity of the blood which has escaped by transudation from the over-lying veins and permeated the under-lying cell-walls. Its particular color is due to a minute quantity of hæmatine furnished by dissolved blood-corpuscles. The bright, highly refracting contents of other cells are due to the presence of oil.

Not less remarkable are the changes found in the non-striated muscular fibres of which the heart's walls are in part composed. They are flatter and broader than natural, their edges are more irregular, the contents of the tubular sheath are less granular than in health, and they are of the same dark color as the contents of the altered cells of the striated fibres. Some of their nuclei have undergone more or less conversion into fat, and minute globules of oil are seen here and there along the course of the fibres. Fat, however, is not a necessary element of this form of tissue-degeneration, because many instances occur in which it does not exist.

The degeneration in question is not only characterised by the microscopic and other appearances already stated, but by the presence of a superabundance of serosity, which saturates, as it were, every individual fibre, rendering the whole mass of the muscular walls of the heart soft and diffluent to the touch.

In the musculi pectinati similar appearances are observed. The epithelium of the endocardium is extremely thin and badly developed, and in some places detached from its basement membrane, which presents the same dull, slaty-red color as the muscular structure of the heart. The basement membrane is likewise soft and pulpy, and the vascular layer consists of vessels so dilated and distended with dark blood that the interspaces are considerably reduced in size.

The connective tissue is generally free from any inflammatory exudation, but the fibres of which it consists lie bathed in the same kind of serosity which distends some of the altered cells of the striated fibres.

Now, this pathological condition of the heart, which is the result of deficient aeration of the blood extending over a long period, is distinct from every other known morbid process which affects the heart. It has no connexion with inflammation, and it is distinct from fatty degeneration, with which, however, it may, as we have seen, be associated. I therefore claim for it a distinct place in the nosology of diseases of the heart. It is, perhaps, difficult to adopt a term which shall sufficiently express the true nature of the disease; but as the changes produced in the heart are manifestly due to the action of a hyper-carbonised condition of the blood, the term *carbonised heart* may serve the purpose of distinguishing it from other affections of this organ.

From the condition in which the striated fibres are found, it is easy to explain why, during life, the action of the heart is so feeble and the pulse so small—why the patient always complains of feeling an amount of prostration which cannot be explained by general appearances—why there is such breathlessness on the least exertion—why death often occurs suddenly.

In the healthy heart the flattened discs forming the ultimate structure of the muscular fibrillæ are in two opposite conditions according as the fibrillæ are in a state of relaxation or contraction. When relaxed, the longitudinal diameter may equal or exceed the transverse diameter, and the muscular striæ are in consequence carried farther apart from each other; but when the fibrillæ are in the opposite state of contraction, the transverse diameter exceeds the longitudinal, and the muscular striæ are brought into closer approximation. In the diseased condition of the heart in question, the longitudinal diameter of the individual discs or cells of the fibrillæ is greater than natural. The tendency of the disease is, therefore, to cause the longitudinal to prevail over the transverse diameter, and, as the disease progresses towards this end, to weaken the heart's power of contraction; because power is represented by the degree in which the transverse diameter of the cells of the fibrillæ in action exceeds that of the longitudinal diameter.

It is manifest that, as the disease progresses, the cells of the fibrillæ lose by degrees their power of contracting, and that permanent relaxation is the ultimate condition in which they end. When that condition has been reached by a sufficient number of these cells to render the heart unable to force the blood through the extreme vessels, the patient may die from mere failure of the heart's power. Death in such a case has been gradually approaching for some time; but its advent is sudden at last. Upon the increasing debility which has now, perhaps, for weeks been patent alike to patient and to friends, there comes a sudden excitement—an unexpected stimulus to exertion, which causes the patient to put forth an unwonted power; and, in doing so, the heart is overwhelmed by the labor thus suddenly thrown upon it, under which it as suddenly falters—fails—dies!

In other instances sudden death occurs without any previous aggravation of the general condition of the patient. This occurrence is readily explained by the particular condition of the fibrillæ of the heart, as already stated. It has been shown that frequently some of the altered cells of these fibrillæ are distended by a dark serosity, others by oil; and that the sarcolemma in which they are enclosed is often marked by small fissures or rents. Now, if it happens—which is not unfrequently the case—that at any point of the heart this condition of the fibrillæ exists throughout the whole thickness or the greater part of the thickness of its walls, the natural efforts of the heart are sufficient to cause rupture at this point, extravasation of blood into the pericardium, and death. More frequently, however, it happens that, when death from this cause occurs suddenly, it takes place during the performance of some act of physical exertion, which, by throwing a sudden increase of labor upon the heart, excites it to unwonted efforts of contraction, under which the diseased fibrillæ give way.

Another mode in which death occurs in this disease is owing partly to the direct effects of the enfeebled heart upon the circulation, and partly to the secondary consequences of tricuspid or pulmonic regurgitation, or to both of these lesions combined. The tricuspid incompetency is a frequent, the pulmonic incompetency a less frequent, lesion in this disease. The former produces general dropsy; the latter a particular condition of the breathing, which demands a more extended observation.

When the pulmonic valves are unable to close their aperture, a portion of the blood projected by the right ventricle into the pulmonary artery falls back into the ventricle, and thus diminishes the quantity which would otherwise reach the lungs. This occurring at every contraction of the right ventricle, a period is soon reached when the vessels of the lungs are very inadequately filled with blood. The requirements of aeration increasing with its defective accomplishment, the demand for more air is immediately felt throughout the whole system, but especially in the lungs. To meet the urgent necessity which deepens every moment, the supplementary muscles of respiration are called into play, violent and quickened efforts are made to expand the chest, and the breathing becomes in consequence distressingly labored and panting. If this condition continues, a diminished supply of blood is quickly felt in the left ventricle, a correspondingly diminished quantity is projected into the aorta, the vessels of the brain being thereby only partially distended, the energy of the brain itself is not maintained, an indescribable feeling of faintness takes place, the countenance blanches, and life is grievously threatened. The recumbent posture at once assumed, a stimulus administered to rouse the energy of the heart, a counter-irritant applied to the precordia, and an abundance of fresh air, are the means to recover a patient from this threatening condition. When, however, this state of regurgitation has been reached, there is no hope of permanently allaying these paroxysmal attacks, and the breathing is ever afterwards liable at any moment to a recurrence of these painfully panting manifestations. But the regurgitation from the pulmonary artery into the right ventricle produces another injurious effect by causing a too frequent effort of contraction on the part of the heart. It will be remembered that, in detailing the result of my observations on the circulation in the newly-hatched trout, I stated that no sooner did the blood touch the lining membrane of the auricle and ventricle than they quickly, firmly, and resolutely contracted. In like manner with respect to the human heart, no sooner does the blood in the case now under consideration fall back from the pulmonary artery into the right ventricle than the latter, responding to the stimulus thus applied to it, contracts, and the repetition of this effort occurring more frequently than it is wont in the natural order of the heart's movements, quickly induces exhaustion in the muscular fibres of a heart already diseased, and thereby favors the occurrence of dilatation. To dilatation would be added hypertrophy if the nutrition of the heart were as active as it is in most other diseases of that organ; but as the condition of the blood is opposed even to the ordinary nutrition of the heart's structures, simple dilatation of the right ventricle is the most common form of enlargement which is found.

Where the tricuspid valve is incompetent, dilatation of the right ventricle is more rapid in its occurrence than in pulmonic incompetency; but where the two incompetencies co-exist, as is sometimes the case, the ventricular dilatation is both more rapidly and more extensively induced than where one or other valve alone is affected.

Practically speaking, our knowledge of diseased action is of no use unless we can recognise it during life. To simply discover its effects after death is of no avail to the living. The question therefore arises—Does this particular disease of the heart give any indications during life by which it can be recognised? Like every other disease, it has its particular and characteristic symptoms. Some of these refer to the heart itself, others to the general system.

Those which more particularly refer to the heart, show that the action of this organ is very feeble, that its sounds are but faintly heard in their accustomed areas, and that the space over which its impulse is felt is less than ordinary. It is sometimes difficult to determine the exact point at which the apex strikes the chest-walls; and when found, the apex-beat is felt as a feeble tap which seems to just touch the ribs and is gone again. When there is tricuspid regurgitation it is not always productive of a morbid sound either over the lower part of the sternum or immediately to the left of this bone over the body of the right ventricle. The regurgitation more particularly manifests itself by jugular pulsation, which is best seen when the respiration is suspended, and when the external jugular vein is compressed upon the posterior edge of the sterno-mastoid muscle. If the right ventricle is dilated, a feeble impulse or a mere flutter is felt in the epigastrium, which is more distinct at the extreme of expiration than at any other stage of the respiratory movement. To the evidence afforded, by jugular pulsation, of tricuspid incompetency there is soon added, if it does not already exist, that of general dropsy. When the pulmonic valves are incompetent, extreme breathlessness is felt on the slightest exertion, and paroxysms of panting breathing are, independently of exertion, apt to arise, during which the countenance of the patient becomes distressingly pale and betrays an expression of the greatest anxiety.

Of the general symptoms, the most prominent one is, as it has already been shown, a feeling of the most complete muscular debility. The sense of prostration is indescribably great. Where there is no valvular lesion, the breathing is nevertheless rendered short and hurried on the least exertion. The patient, after making the least effort, sinks into a chair, complaining of faintness, and gaspingly exclaims that "it is too much" for him. Hence the indisposition of the patient to move about.

In almost every instance the temperature of the skin is below the natural standard; nevertheless, there is frequently, especially when the patient is a female, a hyper-sensibility of the cutaneous nerves, which is resolved into acute pain upon the slightest pressure, and which renders the patient so sensible of the merest change of temperature, that the admission of fresh, cool air into the apartment is studiously avoided. A gentle tap upon the skin, the mere grasp of a part, or even the pressure of a finger upon any part of the surface, is followed by extravasation of blood, which is exceedingly slow in being absorbed.

The general hue of the countenance is a dirty pallor, and a faint, slaty-blue deepens in the early stages the natural color of the mucous surfaces, which gives place to a deeper blue of the lips and of other parts when valvular lesion is afterwards superadded.

The appetite is variable—never large—always, perhaps, below the healthy standard. Still, food is taken every day in moderate quantity. Uneasiness of the stomach generally follows the ingestion of food, and flatulent distension often adds to the miseries of the patient.

The bowels are generally more or less torpid, and the evacuations darker than natural, thus showing the attempt on the part of the liver to depurate the blood by removing an excess of carbon in its secretion.

Like the bodily functions, the mental manifestations are low. Depression of spirits, gloomy anticipations, and a nervous anxiety about particular sensations, especially the sensation of cold, are the leading features of the mind.

(*To be continued.*)

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OBSERVATIONS ON THE

PROBABLE CAUSES OF BRIGHT'S DISEASE

By Peter Hood, M.D.,

I have studied with peculiar interest the published abstract of the paper on Bright's disease, by Sir William Gull and Dr. Sutton, which was lately read before the Royal Medical and Chirurgical Society, and in which it was maintained that the affection of the kidneys is only a part of a more general disorder, and of one that, prior to the occurrence of albuminuria, may make itself manifest by impaired digestion, headache, distress about the region of the heart, dyspnœa, loss of flesh, and other related symptoms. The opinion that kidney disease is a general or constitutional, and not a local affection, is one that I have myself entertained for many years, and that will be found fully set forth in my work on the Treatment of Scarlet Fever, published in 1857. I have there stated my belief that in acute dropsy the renal is generally preceded by hepatic disorder, and have condemned the error, as it seems to me, of hastening to localise disease in a single organ, merely because its easily accessible secretion affords proof that it participates in a general disorder. These views, time and further experience have strengthened and confirmed; and, entertaining them, I offer no apology for venturing to make some observations upon the recent paper to which I have referred. It is much to be regretted that the distinguished authors of that paper have not seen their way to carry their researches a little farther back, and to enlighten the profession with regard to the initial source whence the disease is derived, and with regard to the cause or origin of the anatomical changes in which they think they discern its essential characteristics. In a practical point of view, it profits little to enumerate a series of vague symptoms, which may indeed precede the manifestation of Bright's disease, but which may also be wholly unconnected with it; nor does it elucidate the subject to state that the malady is general instead of being local. What we should desire is to become acquainted with the nature of the morbid processes which precede or produce organic renal changes, so that we may be prepared to recognise and oppose the former, as our only means of preventing the occurrence of the latter. I am fully aware of the objections which may be urged against any attempt to solve such a problem by an appeal merely to individual clinical experience, but am nevertheless impelled to make it by the consideration that we have hitherto gained so little, as far as this question is concerned, from pathological research. Independently of the structural changes in the arterioles of the kidneys and other organs, changes which I myself regard as results of pre-existent morbid conditions of a different kind, we have really no knowledge whatever with regard to the source from which Bright's disease originates.

It will, I apprehend, be generally admitted that no important change will occur, in an organ like the kidney, that has not been preceded by congestion; and that, when a state of congestion has been established, the vessels carrying the blood will be thereby rendered liable, by their own delicate structure, and by the distension they suffer, to undergo alterations of various kinds, first in the direction of hypertrophy, and next in the direction of wasting, unless the congestion itself were early relieved by spontaneous hæmorrhage, or by medical treatment. Assuming this to be true, we are next called upon to inquire by what immediate or remote causes, apart from chill to the surface or other external agency, a congested state of the kidneys may be occasioned and maintained. Among such we must, I am well convinced, assign a prominent position to derangements of the liver, and also to changes in the composition and properties of the blood itself. As regards the liver, it is perhaps sufficient to quote the words of Frerichs, who says: "There can now be no doubt that in this gland processes go on which exercise an important influence over the principle vegetative functions, sanguification, and the metamorphosis of tissues." As regards the blood, I hope to make it evident that the condition of this fluid is a chief cause of some of the derangements which lead on to destruction of the kidney.

In former times, when venesection was commonly practised as a remedial measure, there were three chief points to which the attention of the phlebotomist was directed. The first of these was the amount, consistence, and color of the clot formed by the blood on cooling; the second was the appearance of the surface of the clot; and the third was the color, density, and odor of the serum. When the clot was firm and solid, giving evidence of the presence of a large proportion of fibrine and hæmatine, this was considered to afford evidence of the strength and tonicity of the patient, and of the safety with which blood might be abstracted; while a contrary indication was afforded when the blood was slow to coagulate, and the clot small and incohesive. When the surface of the clot was buff-colored and deeply concave, or, in the language of that day, "buffed and cupped," these conditions were held to demonstrate the existence of inflammation; and the proportion borne to the clot by the serum, the color and density of the latter, and the presence or absence of odor, were considered to be important aids to diagnosis. In some exceptional conditions of the system I have known the blood to have a smell as if of absolute putrescence.

The only condition in which "buffed and cupped" blood was not distinctly associated with inflammation, and in which the buffed and cupped condition remained in spite even of successive bleedings, was in acute rheumatism; so that in this disease it was conjectured by some that the blood itself was inflamed.

In the time of which I speak, as is well known, blood-letting was continually practised without medical advice, as a sort of quasi-domestic remedy, not only at the "spring and fall," but whenever such symptoms as fulness, drowsiness, difficulty of breathing, or uneasiness about the head and heart, were experienced. It cannot be doubted that much harm was often done by the improper abstraction of blood under such conditions; but no well-informed practitioner ever ventured to take blood from an anæmic person, or from one who, in the phrase of the day, "could not afford to lose it." The decision would depend upon the state of the pulse and heart, and on the presence or absence of organic disease; and whenever there was a doubt, a small trial bleeding would be made, in order that the character of the fluid withdrawn might assist in determining the doubt. It may fairly be questioned whether the absolute disuse of blood-letting may not be as harmful as its indiscriminate employment; and whether serious consequences, especially the increased prevalence of some forms of chronic disease, have not been produced by the abandonment of a tried and powerful remedy. I have been assured, by the late Dr. Bright himself, that, he was distinctly of this opinion; and I am fully convinced that an insufficient degree of attention is now paid to states of the blood as the chief factors in the production of various diseases. It has been shown by Dr. Richardson, for example, that in various inflammatory or other morbid states the fibrine becomes increased from 3 parts in 1000 to as much as 10 parts; and it is not difficult to perceive how easily such a change, by increasing the density and viscidity of the fluid, may obstruct the circulation in the smaller vessels, and may produce congestion of various organs—thus subsequently determining the occurrence of anatomical changes to which congestion has primarily contributed. If this reasoning be correct, it appears to me that we may deduce from it the farther inference that an excess of fibrine in the blood must be regarded as one of the chief proximate causes of Bright's disease, as well as of the albuminoid or fibrinoid degeneration of the liver with which Bright's disease is so commonly associated. It is to the blood, then, that we must first direct our attention if we are to arrive at any satisfactory solution of the problems connected with Bright's disease. We see, every now and then, a patient who seems to be in perfect health, except that there are slight symptoms of indigestion or malaise —symptoms which yield readily, for a time at least, to a little medicine or a restricted diet, and which yet are proved by the future course of events to have been the precursors of organic renal mischief. I do not know how we are to arrive at an accurate prognosis of such cases in their early stages unless, by adopting the long-disused method of a trial bleeding, we obtain information concerning the quantity of fibrinous material present in the blood. No system of medicine can be considered perfect that excludes the experience gained during the hundreds of years in which blood-letting was almost universally practised.

We are indebted to Dr. George Johnson for the important discovery of the changes that take place in the vessels of the kidneys; and although Sir William Gull and Dr. Sutton differ from him with regard to the precise character of these changes, yet all observers are agreed with regard to their fibrinoid character. This point of agreement renders me great assistance in explaining my views of the original source to which Bright's disease may be traced.

We are taught by clinical observation that the nephritis which leads to contracted kidney may be divided into three stages: the stage of hyperæmia, the stage of exudation, and the stage of atrophy. In the first of these stages the kidney is found darkred in color and rich in blood—an appearance which points clearly to deficient oxidation and decarbonisation in the system, so that other glandular organs, and more especially the liver, would probably be in a similar condition. If blood were abstracted at this period we should expect to find it abounding in fibrine; and this condition would in former times have been held to show the necessity for a larger depletion. I entertain no doubt that such depletion was often the means of prolonging life, by relieving vascular plethora in the earliest stages of disease.

If we inquire now into the actual causes of the glandular congestion, we shall find that it occurs, as a rule, in persons who have passed the meridian of life, and who have partly or wholly laid aside former habits of active exertion. In most cases such persons have continued to take much the same amount of food that they had formerly been accustomed to consume, and they have often even increased their consumption of stimulants, on account of the temporary comfort to be derived from them. A person to whom this description applies is in reality standing upon the brink of an unseen precipice. He may eat, drink, and sleep as usual, and may follow his ordinary course of life, without suspicion of the dangers by which he is threatened. If he inherits any tendency to gout, this disease will probably soon show itself; and it will be well for him that it should do so, always provided that it is not treated specifically, and that it is accepted as a warning of the need for changed habits of life. An attack of rheumatism may even be hailed with satisfaction on account of the good effects to be obtained from the remedies necessary for its cure. If neither of these diseases should occur, nor any other form of acute illness, we may sooner or later expect evidence of congestion in one or more of the vital organs, and should be prepared for the organic changes which such congestion may produce. In cases of a different class the patients may have suffered from chronic dyspepsia with hepatic derangement, generally associated with constipation. I have frequently noticed that the instances in which persons addicted to good living, and to the free use of alcoholic drinks, have nevertheless preserved uninterrupted health, occur almost exclusively in those whose bowels are habitually relaxed; and I feel sure that the intestinal tract is capable of largely aiding the kidneys in the discharge of uric-acid compounds from the system. Where this aid is not given, and where, from imperfect digestion, the solids and fluids of the body are generally vitiated in their character, the kidneys often suffer from the excessive work that falls upon them.

It is probable that an early stage of kidney congestion, occurring in a robust and previously healthy person, would make itself felt by no more definite symptoms than some generally uneasy sensations—some feelings of fulness or oppression, of drowsiness or tendency to palpitation. Under such circumstances the tendencies of the case might easily escape even medical observation; and further, if these tendencies were perceived, the patient, in the present state of public feeling and professional practice, would be little likely to submit to the abstraction of blood, even as a test of his condition. If, however, he had sufficient good sense and sufficient confidence in his adviser to allow himself to be bled, and if his blood were found to contain an excess of fibrine, the discovery would at once afford the strongest possible argument for a changed course of life—as a means of warding off a too obvious danger.

It becomes important to consider what symptoms should lead to the suspicion of an excess of fibrine. I think the most significant is a peculiar tense feeling communicated by the pulse to the finger. The pulse need not be full—it may be even so fine as to resemble to the touch a tight cambric thread; but, if a pulse of this character be found for any continuous period associated with symptoms of oppression that medicines do not readily relieve, the additional evidence to be gained by a trial bleeding may become of the greatest possible value. Some years ago it was stated that an effectual "trial bleeding," *quâ* excess of fibrine, might be accomplished by a needle puncture. The drop of blood was to be compressed between two slips of glass; and it was said that the rapidity and degree of mottling in the fibrine would closely correspond with the degree in which a larger quantity of the same blood would be cupped and buffed. If this method be trustworthy, it would be extremely useful, and would afford great help in overcoming the prejudice now entertained against bloodletting.

An endeavor to show the effects of bleeding in warding off organic disease would be open, of course, to the obvious objection that we cannot be quite certain what would have happened if the bleeding had been omitted. But still, clinical experience may sometimes furnish us with evidence nearly approaching to proof, and of such I will give a single example. Many years ago I attended a gentleman, aged twenty-two, who had suffered severely from epilepsy from the age of fifteen. He had had the best advice London afforded. He had been subjected to all kinds of treatment, and had taken all the reputed remedies for the cure of his disease, without any good result. He had lost most of his front teeth from repeated falls on his face, and his scalp was scarred in several places. His chief complaint was the discomfort he experienced from palpitation of the heart, which frequently preceded an attack of epilepsy. There was no evidence of any disease of this organ, and it beat vigorously; but his pulse was singularly small, and at the same time tense; it felt beneath the finger like a fine thread. The discrepancy being so great between the action of the heart and the feeling of the pulse, I ventured to take an ounce of blood from his arm. When it had cooled it was both buffed and cupped, and the amount of serum was very disproportionate to the more solid elements. In a week's time I bled him again to two tablespoonfuls, when no alteration was observable in the appearance of the blood; and I adopted the same course every week for a whole year; and at the end of the year, the blood still being buffed, but not cupped, I bled him once a fortnight until the expiration of the second year, when his blood no longer presented any abnormal character. This gentleman's fits ceased after the first time he was bled. He married and had a family. Nine years after the cessation of his fits he suffered from fistula in ano. He was operated on by the late Sir Benjamin Brodie prior to the introduction of chloroform, and although there was only one cut made (with scissors) the agony was so great that he was unable to withstand it, and he was instantly seized with a violent epileptic paroxysm. The fits recurred with their former violence, and when he had recovered from the operation I resorted again to bleeding; but in the interval of nine years a change had taken place in the state of his pulse and the appearance of his blood—the pulse had lost its tightness, and the blood no longer presented any abnormal appearances. The bleeding now was useless, and I did not adopt it again. This gentleman has tried all the modern remedies said to be efficacious in epilepsy without avail, and is still subject to the disease.

It must generally be impossible to assign the exact date of the commencement of the states that I have been considering; but the chief point for the practitioner is to learn the period of accession of the symptoms of plethora or congestion, especially when these are coincident with an alteration in the habits or mode of life.

The second and third periods of kidney disease—the stages of exudation and of wasting—are of far less importance to the practitioner, because far less amenable to treatment. The period of exudation is usually that in which albumen first appears in the urine; and then, in chronic cases, the patient may usually be regarded as incurable. In the more acute forms of the malady—as, for example, in the kidney troubles following scarlet fever—complete recovery may often be obtained under a treatment in which active purgation forms the most important element.

The influence of congestion of the liver—whether itself a result of an increase of fibrine in the blood or of some other cause—in producing congestion also of the kidneys is one that cannot be overlooked by the careful and experienced practitioner; although it has been pushed out of the view of superficial observers by the greater facilities which exist for obtaining evidence of the congestions of other organs. If the secretion of the liver could be isolated and obtained for examination as easily as the urine, there can be no doubt that we should soon have an hepatic pathology of almost every disease that flesh is heir to. In the meanwhile, congestion of the liver is constantly overlooked, because we examine the abdomen only in the recumbent posture. In every case in which I suspect it, I have for many years been in the habit of employing percussion and palpation in the erect posture also; and I have in this way discovered enlargement of the liver, and have been led to devise measures for its relief, in many instances in which the true nature of the malady must otherwise have been overlooked.

The observations conducted by the pathologist in the post-mortem room, and those conducted by the physician upon patients whom he preserves from being conveyed thither, should mutually supplement and correct each other. It is chiefly from the latter point of view that I would urge the practical importance of early attention to over-fibrinated blood and to hepatic congestion; and I am convinced that the more carefully these conditions are studied the more they will be found to guide us towards the prevention of maladies which, if not prevented, it will too often be found impossible to cure.

Lower Seymour-street, W., July, 1872.

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THERAPEUTIC NOTES ON CHLORAL.

By John Barclay, M.D., C.M.

I would briefly record my experience of chloral hydrate in croup, insanity, delirium tremens, epilepsy, and asthma; and also draw attention to a special peculiarity which I have observed with regard to its action when combined with iodide of potassium—namely, the remarkable power it possesses of intensifying the constitutional effect of the iodide.

Croup.—My first case was that of a child of fifteen months, who had first tonsillitis, then hoarseness, and croup supervening. During the three days after the larynx became involved the case went on like an unfavorable case of croup, from bad to worse, and at the end of that time little or no hope was entertained of recovery. Tracheotomy was proposed twice; the first time it was postponed because, just when the operation was about to be performed, some improvement took place; and then at a later period it was again proposed, but rejected as holding out next to no hope of saving the child. The distress of breathing was extreme, and more to relieve this than with any hope of ultimate benefit, I ordered two grains of chloral, with two minims of ipecacuanha wine every two, three, or four hours, according to the effect produced. Ipecacuanha wine alone had been given before. Besides these sherry was given freely. I watched the case carefully and closely all along, and these were the observations I made. Before the chloral was given the respirations were from 60 to 70 per minute, and the pulse varying from 144 to 156, and weak; the dyspnœa was extreme, and the voice and cough now close and dry. Within two hours after the first dose great relief in the breathing was observed, the child slept for half-an-hour at a time, the respirations and the pulse fell a little, and, what was more curious, a very copious exudation of mucus began in the larynx and trachea. Indeed, so great did this become that it was necessary to raise the child every half hour, when a little water or sherry was given to produce a cough, by which the air-passages were cleared of the mucus. Had the child been allowed to lie it would soon have been suffocated, for after the chloral was given and the exudation had commenced, there was not the slightest inclination to cough. Gradually the pulse fell, and the respiration became slower, till at the end of twenty-four hours the former was only 108, and the latter 30. This treatment was persevered in for forty-eight hours, after which milk and sherry only were given. The recovery was slow but steady.

The next case was likewise one of inflammatory croup in a child of four years. There had been hoarseness and croupy cough for two days before I saw him. I ordered chloral in four-grain doses, with four minims of ipecacuanha wine, every four or six hours, and nothing else in the way of medicine. In two days the hoarseness and croupy cough had disappeared, the symptoms of improvement being the same as in the former case—namely, quiet sleep, exudation of mucus, and decrease of fever.

During the past two years I have treated several other severe cases of croup with the chloral and ipecacuanha, and have not been disappointed with the result. In all of them I have made it my first object to obtain a quiet sleep, and on the child awaking I have almost always found some decided improvement.

Insanity.—I have in private practice treated seven cases of insanity with chloral as the hypnotic. Five were females and two males. All had insomnia as a prominent symptom for some weeks previous to the manifestation of delusions and illusions, and in none of them did the chloral fail to produce sleep. All were severe cases, one of them during a previous attack having been in an asylum for many months. Half-drachm doses were given every night, usually with the effect of producing ten or twelve hours' sleep; and sometimes the same quantity was given in the morning also, when the patient was much excited. Indeed one of them, a lady, was kept asleep almost continuously for a whole week. In neither of the patients, however, did any sign of returning reason appear before the end of about three weeks, and in some of them it was much longer, though they all gradually became more quiet and more easily managed after eight days. I certainly agree with Dr. Campbell, of Garland Asylum, Carlisle, in thinking that chloral in these cases is much to be preferred as a hypnotic to hyoscyamus, bromide of potassium, and I may add morphia, as being a much more certain sleep-producer than either of these. In the most severe case of the seven, that of a lady, the excitement was so great, however, that I was first obliged to inject a full dose of morphia hypodermically, which lessened her power of resistance and rendered it possible to administer the chloral.

Delirium tremens.—I was called late one night to see a young man who was laboring under this complaint. He had attempted to hang himself, had been rescued, and was then very violent. About half an hour before I saw him he had got half a drachm of solution of morphia, which seemed to have no quieting effect whatever. I then injected hypodermically half a grain of morphia, and waited an hour; but at the end of that time he was still as violent as ever. I then injected half a grain more, and waited for two hours; but even then the violence was, I think, if anything, greater than before. I now sent for a drachm of chloral in solution, half of which I gave him. In ten minutes he was fast asleep, and he slept for ten hours. When he awoke he was slightly collected; he was well fed with strong soup and arrowroot, got the rest of his chloral draught, fell asleep again, and in twelve hours more awoke almost rational.

Epilepsy.—I have treated three epileptics, all females, with chloral. The first, a girl about eighteen years of age, came to me in June, 1870, to be treated for epilepsy. She had had fits from infancy, and had been treated perseveringly by many medical men. She was dysmenorrhœal. Believing that she had taken all the orthodox remedies for epilepsy before, I ordered ten grains of chloral three times a day. Before beginning to take the chloral she had a fit every day, and occasionally two daily; but after being put upon it she had only two fits during the next three months. At the end of that time she ceased to take the chloral, and the fits returned. After a little she resumed the medicine, and during the time she was in my neighborhood (about two months) she had no fit. I have since heard, however, that, having given up the chloral, the fits returned as frequently as they did before she took the medicine.

The second case was that of a girl, sixteen years of age, who had had epilepsy from infancy. Upon an average she had two fits every week. Nothing had been done in the way of trying to cure her. In May, 1871, I ordered eight grains of chloral three times a day, which she continued to take for six weeks. It is now upwards of a year since she began to take the medicine. I saw her this day (July 8th, 1872); and her mother informed me that she had two fits during the first week in which she took the medicine, but none after.

The last case was that of a woman, unmarried, and thirty-seven years of age, who had begun to take fits when about twenty. She had been treated in several of the London hospitals, and by many medical men throughout the kingdom; but nothing in the way of treatment ever did her the slightest good. When I saw her she was taking fits four or five times in the twenty-four hours, her memory was almost entirely gone, and she was rapidly becoming imbecile. Not hoping for much success, I ordered chloral in ten-grain doses three times a day; and the effect was that she had no fit for a week. At the end of that time she had a very severe one. Two days after this she had another, during which she had fallen out of bed. She was found lying by the side of the bed dead.

These three cases would seem to show that chloral does possess some power over epilepsy; but whether this be confirmed by more extended experience remains to be seen. Possibly a combination of chloral with bromide of potassium might be followed by more permanent results, especially if, which is not improbable, chloral be found to have the same intensifying effect on the action of the bromide that it has on that of the iodide of potassium.

Asthma.—I have tried chloral in a few cases of spasmodic asthma; and though it acted rapidly and beneficially in two or three attacks in each person, it soon ceased to have any effect whatever, even when the dose was very much increased. I have one patient who obtains *some* relief from a drachm dose repeated in an hour; and she says she finds most relief from first swallowing the large dose of chloral, and then breathing nitrite of amyl deeply. Upon the whole I think it is nearly a failure in asthma.

I now come to *the influence which chloral exerts upon the action of iodide of potassium.* From the middle of February to this date I find I have prescribed these two medicines in combination for fifteen patients; and it will be seen that of these ten were affected with iodism by the first or second dose of the mixture. The five who presented no symptom of iodism were three children, respectively seven, eight, and nine years of age; and two men, the one sixty-five and the other sixty-seven.

1.—Feb. 17th, 1872: J. M——, clergyman, aged thirty-five, after perspiring freely, was seized with a rigor, which was followed by acute pain in the lumbar region and over the left side of the thorax. Pulse 96; tongue foul; urine high-colored; no signs of pleurisy or other affection of the lungs or heart. Ordered a purge, a cataplasm, and a mixture, containing in each dose five grains of chloral and five of iodide of potassium, to be taken every four hours. The first dose so severely iodised him that I was called up during the night to see him, and he was unable to leave his bed for four days on this account alone. But no sooner did the symptoms of iodism begin than all the lumbar and thoracic pains began to disappear.

2.—April 22nd: B. D——, female, unmarried, aged thirty, was seized with a severe attack of erythema nodosum, and the pain in the spots was very great. She had the same mixture ordered for her as above, and after the first dose she was thoroughly iodised, though not so severely as Case 1. The pain of the erythema was almost entirely relieved. After this one-fourth of the original dose was sufficient to bring back the iodism.

3.—April 23rd: R. F——, aged eight, female, had great pain and swelling in both knee-joints and one elbow. Pulse quick; tongue foul; urine high-colored. Ordered three grains of chloral and three of iodide of potassium every four hours. Pain and swelling all gone in forty-eight hours, but no iodism.

4.—April 28th: F. S——, female, aged twenty-five, complained of great pain and swelling along the lower jaw near the sockets of the molars. Ordered poultice and same mixture as in Case 1. Iodism was established on the following morning, but no relief to the pain was experienced. Suppuration ensued.

5 and 6.—April 30th: These were cases very similar to No. 3: girls respectively seven and nine years of age. No iodism was observed in either, though the three-grain doses soon cured both.

7.—May 10th: Mrs. S——, aged thirty, about a fortnight after the birth of her fourth child, was seized with considerable pain and swelling of both knee-joints, both ankles and wrists. Ordered fomentations to the joints, and chloral and iodide in five-grain doses internally. Iodism was produced after the second dose; but after a day or two, all the pain and swelling had disappeared. When iodism began to subside, I again put her upon the mixture, in teaspoonfuls instead of tablespoonfuls, but the iodism returned, compelling her to give up the mixture altogether.

8.—May 13th: Mrs. S——, aged sixty-five, labors under chronic sciatica. She had had a great variety of treatment before getting the chloral and iodide. This combination produced slight but quite apparent iodism. She had, however, taken only one dose, and could not be persuaded to take another, "the feeling in the head and throat was so bad," she said. Very little relief to the sciatica was afforded.

9.—May 24th: G. S——, farmer, aged sixty-five, at the close of a severe attack of cellulitis of the lower and back parts of the thigh, and when the incisions made for the relief of this were healing up, showed several irregular enlargements over the lower part of the femur in front and at the sides. Chloral and iodide in five-grain doses each were given, and continued for a week, but no iodism resulted.

10.—June 8th: Mrs. M——, aged forty-six, had acute pain and swelling in the right ankle-joint. Ordered an irritating liniment to be rubbed into the skin around the joint, and chloral and iodide internally. The latter had the effect of relieving the pain and swelling, but produced distinct though not severe iodism.

11.—June 22nd: Mrs. M——, aged thirty-seven, had for two years labored under chronic rheumatism. She had one severe attack of the acute form a year ago, which was treated by blistering, with a very satisfactory result. The iodides of potassium and ammonium had been several times given to her in large doses, without at any time producing iodism; but now one dose of the iodide of potassium and chloral mixture produced distinct, and two doses severe iodism. She said, however, that no remedy she ever had before had done the swollen and painful joints so much good.

12.—June 23rd: Miss B——, aged thirty-four, one of a very phthisical family, had for several years complained of pain over the right ovary. Leeching, various forms of counter-irritation, and numerous remedies internally, had been at different times prescribed for her, but with a variable amount of benefit. I gave her the same chloral and iodide mixture as above, and warned her as I had warned all the patients who got it after No. 4, that she might suffer from iodism. And she did suffer considerably from it for one day. The pain was somewhat relieved. She refused to resume the medicine, even in small doses.

13.—July 2nd: J. M——, aged thirty-two, complained of acute pains in his shoulder and knee-joints. No perceptible swelling. Various things were tried with no good result. At length I gave him the iodide and chloral, which afforded relief in a few hours, but which iodised him. The medicine was omitted for one day, and then resumed in teaspoonful instead of tablespoonful doses, without again producing the symptoms of iodism.

14.—July 8th: A. B——, aged sixty-seven, has an attack of gout in both feet. Ordered iodide and chloral in five-grain doses each.—July 10th: Has continued to take the mixture every four hours with considerable relief to the pain, but no symptoms of iodism have been developed.

15.—July 13th: J. D——, brother of Case 2, aged forty-six, has great pain along the sciatic nerve; and numbness of the calf of the leg and foot. Ordered a purge of colocynth, galbanum and croton-oil pills, and afterwards iodide and chloral, the former in five-, and the latter in eight-grain doses every four hours.—July 15th: Was well purged. The first dose of the mixture was taken about 6 P.M., and by 10 P.M. distinct iodism was the result. A second dose on the following morning made the iodism severe. To reduce the dose to one-fourth.—July 18th: Has still some pain in the forehead and throat. Pain in the thigh nearly gone, but the numbness much the same as before.—July 24th: Cured.

It would appear from the above fifteen cases that all those iodised were of middle age, those unaffected being three young girls and two elderly men. Possibly there may be something else to account for the immunity those enjoyed than that derived from their youth or age, something perhaps in the nature of the complaints from which they suffered, or some peculiarity in their constitutions; but my observation and experience are as yet quite insufficient to enable me as to decide as to this. I merely make known the fact that in the greater number of patients to whom I have given the combination of chloral and iodide of potassium iodism has been produced, and in whom, had the chloral been omitted no iodism would in all likelihood have been seen. Iodism from doses of five grains of the iodide of potassium alone is very uncommon. I can remember seeing it in two cases only during the past ten years. And Lisfranc, in a hundred cases in which he prescribed the iodide in this and larger doses, saw iodism in none of them. As to whether this influence possessed by chloral over the constitutional action of iodide of potassium be found to be an advantage in therapeutics, further experience alone can determine.

Banff.

NOTES

ON THE

GENERAL PRINCIPLES OF CUTANEOUS THERAPEUTICS.

By Tilbury Fox, M.D., F.R.C.P.,

Physician to the Skin Department of University College Hospital.

(Concluded from November No.—p. 584.)

Class III.

I have still to take up the question of the general principles of treatment to be followed in regard to the third class of diseases—those in which the skin is diseased as a direct consequence of disturbance of the general nutrition. *General* treatment is everything, *local* treatment of little value *comparatively*, in these cases. I have nothing to say as regards the acute specific diseases, the eruptions of which are secondary matters, and require no treatment.

As regards the case of disease of the skin dependent upon the existence of special diathesis, there is a general agreement as to the modes and methods of treatment appropriate to each. The strumous diseases are met by cod-liver oil, iron and quinine, and good food; the syphilitic by mercurials and iodide of potassium; and so on. But there are one or two practical considerations calling for particular notice. In the first place, I would say that, as a rule, in treating skin disease, strumous tendencies of the middle-aged and the old are overlooked; in fact, in many skin affections, the minor evidences of constitutional disturbance are apt to be missed. An individual who has been very strumous in early age retains, to a greater or less extent, the tendency to free pus-formation &c. in later life, though it may not be so strongly marked or coincidently exhibited in connexion with conditions which at an earlier age are regarded as indicative of struma. Whenever I find in the middle-aged or old free pus-formation out of proportion to the degree of inflammation—that is, when I cannot account for the free pus-production by the intensity of an inflammation, and this is attended with exuberant flabby granulation in ulcerations,—I regard the case as one in which struma plays an active part. I treat my patient accordingly, and successfully as a consequence. In addition to the strumous, syphilitic, and leprous diatheses, there are other states—call them cachexias—that need to be more generally regarded as leading directly to disorders of the skin: lead-poisoning is one. I have seen unhealthy ulcerations come on in persons who were suffering from saturnine poisoning which healed up under the treatment appropriate to the latter. But perhaps this should have been referred to under the head of medicinal rashes. Then, again, there is the so-called "dartrous diathesis," the nature of which I am wholly at a loss to comprehend from a study of the writings of the French. I suppose it is the gouty diathesis. But I know of no eruption directly due to gout. Skin diseases may be influenced and modified by gouty habit of body, and this has been referred to in discussing the therapeutics of Class 2. The herpetic diathesis I do not understand, unless it be "rheumatic." The alteration of the general nutrition consequent upon malarial poisoning I think in some cases plays a direct part in inducing skin mischief, and especially attacks of pemphigus and bucnemia tropica, or elephant leg. The appropriate remedies in such cases would be quinine and arsenic and iron of course; and all the diseases named are relieved by these remedies. The connexion of malarial poisoning with skin disease is a matter deserving attention. It is a question how far Delhi boil and its allies are connected with malaria. I own, of course, malarial poisoning may hereby act as a debilitating agent, and give a patient afflicted with a cutaneous disease less chance of recovery than a person free from the effects of its operation upon him; and I see many cases in those returned from abroad in proof of this. The skin affection may be due in consequence of a part of the malarial mischief. But at present our information on this point is anything but satisfactory, yet I feel sure time and further observation will show that there is the closest connexion between the two. The "keloid diathesis" may or may not exist. In some cases the tendency to the development of keloid tumours in all parts of the body is marvellous; and the same may be said of "fibroma," but this may be due to a peculiar disposition on the part of the fibro-cellular textures of the skin, deranged—over-stimulated in some cases—by climatic influence acting upon it, and disposed thereby to undergo change—a disposition independent of the general state of the blood. The evidences, however, at present in our possession are insufficient in proof of the existence of a "keloid" or a "fibromatous" diathesis. It must be shown that these, if diatheses, are hereditary before I can accept them as diatheses.

Neurotic diseases are daily assuming an increasing importance, and properly so. They are connected with definite conditions of general ill-health, and deserve a notice here. Pemphigus, herpes, in some cases general pruritus, and very likely true prurigo, are illustrations. The principles of treatment involve the removal of over-work and fatigue of the body and mind, attention to the tendency to general deterioration of the tissues of the body, which is frequently present, and general wasting of the body; the securing of due rest, change of scene and air, as well as the institution of a special system of neuro-tonicism. It is in these cases that arsenic is frequently beneficial. The sympathetic nervous system is probably particularly involved in such diseases as pityriasis rubra, where hyperæmia seems to be due to a loss of tonicity in the vessels, itself consequent upon disorder of the sympathetic system, the more clearly evidenced by the coincidence of hyperæmic conditions of important internal organs largely controlled by the sympathetic and also the general character of the cutaneous hyperæmia. In these cases "general debility" is particularly marked, patients do not seem to be able to "pick up" strength, and specially to assimilate, and it is only by the most careful tonic regimen, and by care being taken that the powers of the body are husbanded, that recovery can be brought about.

Lastly, I think that we shall in course of time come to recognise that the lymphatic system is much concerned in skin diseases. No doubt it is itself disordered as a consequence of the operation upon the whole body of certain potent influences of ex-malarial poison, but it has yet to be determined how far in certain cases it may be primarily at fault, as in scleroderma. Thus I think that in all cases where the fibro-cellular structures of the skin are specially involved—fibroma, keloid, elephantiasis, &c.—we should seek to determine how far the sympathetic system is concerned primarily or secondarily in the disease. Quinine is a potent agent for good in all cases in which the lymphatics are actively involved in diseases of the skin, whether primarily or secondarily.

A few words are necessary about the local treatment of skin diseases immediately dependent upon, or part of, a general derangement of the system. The remedies should be used with similar intention as in the case of local applications spoken of as regards Class II. After all, they are quite subsidiary; and I will only say that, when used to remove exuberant growth or to heal ulcerations, the best time to begin their employment is when internal medicines are just beginning to make an impression on the disease. If the general condition upon which depends the development of an eruption or an ulcer be left uninfluenced by internal treatment, the use of irritants or caustics will often tend to spread or to favor the outbreak of fresh mischief, as might be anticipated. The exuberant granulations of a strumous sore or the unhealthy ulceration in syphilis are especially aided in their disappearance or their healing by the mercurial plaster or other application used in conjunction with the internal remedy at the time that the latter has begun to improve the general condition. However, this is self-evident. But I do think that local applications are sometimes the cause of harm, simply because they are too much trusted to for the cure, and are not subordinated, as regards their use, in due time to general remedies. The use of soothing applications in neurotic diseases to allay local irritation due to general causes is necessitated by the discomfort of the patient and the harm likely to be done by reflex action &c.

SUMMARY.

From the foregoing observations I hope it will be evident that the treatment of diseases of the skin consists in something more than the prescription of arsenic. I have asked one after another, "Why do you give arsenic?" and the invariable answer has been, "Well, one does give it, you know!" or "Well, what else shall we give?" I have attempted to prove that it is impossible to formulate a particular treatment for a particular disease—i. e., to say this or that man has this or that disease, and therefore he must have such and such treatment. He requires a combination of medicamentation suited not only to the disease itself, but also to his individual peculiarities and concomitant circumstances; and it needs all the knowledge and tact of the physician to make this combination happily. In regard to local measures, I am certain that they frequently do harm, from the injudicious employment of stimulants and astringents where simple soothing means are called for. But, thanks to our advancing knowledge of therapeutics, better principles begin to prevail.

Harley-street.

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REMARKS ON TRACHEOTOMY.

By Llewelyn M. Thomas,

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Every practitioner has, during his studies in the dissecting and operating rooms, had the subject of hernia in all its bearings, anatomical and otherwise, brought prominently and repeatedly before his notice, and though he may have no special liking for surgery, or may even feel diffident as to his operative powers, yet it must occur to him that as a general practitioner, especially in country districts, he may at some future period of his career be brought face to face with a case of hernia urgently requiring operative interference, and with no possibility of his referring to authorities, or even obtaining the assistance and advice of another surgeon. How often, however, do we meet with poor creatures actually *in extremis*, who have been allowed to go from bad to worse, the medical man attending them wasting the precious hours in palliative measures, either not recognising the gravity of the symptoms, or lacking the moral courage to urge an operation, or even altogether overlooking the strangulation of the hernia.

Another operation requiring generally even more prompt decision than hernia is tracheotomy, and the necessity for its performance may occur to any man at any moment; the operation, however, is comparatively rarely seen in the theatres of our medical schools, and I believe its anatomical details are less forcibly and persistently brought before the notice of the student in the dissecting-room than are those of hernia. For these reasons it is probable that some may have scarcely considered the operation in all its bearings until perhaps brought into contact with a case requiring an artificial air-passage, and then from want of previous consideration have found themselves in a dilemma, and though perhaps convinced in their own mind that they ought to operate, have yet feared to decide that an operation is quite "the correct thing." I remember the case of a man being admitted into a hospital with syphilitic ulceration of the larynx. He stood a laryngoscopic examination previous to admission, but that evening spasmodic breathing suddenly came on, and he was unfortunately allowed to die without operation. On the other hand, I remember a child being brought in who had a few days previously indulged in the luxury of sucking at a kettle of boiling water; its breathing was terribly impeded, and it had all the symptoms of œdema of the glottis. Three surgeons and a physician happened to be in the ward, and they all agreed that tracheotomy was imperative. The instruments were sent for, but in the interim that child most perversely began to breathe freely, and subsequently recovered with its trachea intact. A general practitioner informed me of a case in which he called in a consulting surgeon to see a child with what proved to be laryngismus stridulus; the surgeon wished to operate immediately, but a delay was urged by the parents, and that child also began speedily to breathe easily. It appears, therefore, that great skill is requisite in deciding on the necessity of operative interference in laryngeal cases, much more, in fact, than in hernia, which must be treated almost dogmatically; if you are certain of strangulation, even though the symptoms are not very urgent, and you have tried gentle taxis, ice, and chloroform without avail, do not trust to time and patience, but operate!

With regard to tracheotomy in croup, opinions differ immensely, and the evidence is conflicting; but this may be said that in England, at any rate, the operation is generally put off till the case is desperate, either in deference to the objections of the parents, or for other reasons, and therefore the operation rarely has a fair chance of success. Many surgeons appear to have an objection to the use of chloroform in tracheotomy; but it greatly relieves the spasm which is nearly always present in these cases, and I believe it to be almost impossible to operate safely on young children without its use; the patients may become livid and appear to be dead or dying at the close of the operation, but when air obtains access to the lungs they speedily recover.

The depth of the trachea in children cannot be too carefully borne in mind, and also the looseness of the areolar tissue and fascia round the trachea, which permits of the tube being pushed down in front of the trachea most readily, as I have seen occur.

A case which has happened in this hospital will illustrate some of the foregoing remarks. A man aged forty-four was admitted with great obstruction to his breathing; he made continuously, both asleep and awake, a loud, harsh, roaring, laryngeal noise, which could be heard at a distance of some yards; his face was pale and somewhat blue, his mouth always open, and eyeballs prominent and staring. He appeared, however, in some measure inured to his condition, and his state was never positively urgent. He spoke in a hoarse whisper, evidently with difficulty, and was troubled with phlegm which was unmixed with pus. He stated that he had been hoarse and impeded in his breathing for twelve months, but that only during the last six or seven weeks he had continually been roaring; no syphilitic history. His troubles came on gradually; at no time had he suffered pain, and he could freely bear pressure on the larynx. I examined him several times with the laryngoscope, but failed to see the vocal cords or the interior of the larynx satisfactorily; the epiglottis appeared to be rigid and to impede a proper view. He stood the examination well, as he had been examined at two other hospitals frequently. There was no evidence of aneurism or other thoracic tumour—in fact, the continual laryngeal noise pointed rather to local disease than to pressure on the recurrents. It was decided, as there was apparently no immediately urgent danger, to operate on the regular day, and thus give him the benefit of a consultation. He was in the hospital four or five days, and derived ease from a morphia, ether, and squill draught. I saw him on my rounds one evening, and spoke to him at 8.30 P.M., and at 9 P.M. a patient called the nurse, and said he could not hear him breathing: he had died apparently without a struggle. At the post-mortem the larynx was removed, and there was practically no opening through the glottis, the parts being so swollen and infiltrated; the larynx was divided with scissors, and in the middle of the cricoid cartilage, posteriorly, a small abscess the size of a pea was cut through; this was apparently between the cricoid and its perichondrium; its walls were ossified. The vocal cords were tightly strung over the surface of the mucous membrane, leaving a mere chink. This appeared to be all the mischief, but on passing a probe into the right sacculus laryngis, the upper surface of the cricoid was felt bare, ossified, and carious, and on opening the right crico-arytenoid joint, both surfaces were in the same state, resembling the carious surfaces of a diseased knee-joint; much of the cricoid was ossified; the thyroid was totally unaffected. The state of the vocal cords and the general swelling explained why nothing could be seen laryngoscopically.

If this man's trachea had been opened the parts would have been comparatively at rest, the crico-arytenoid joint would possibly have become anchylosed, and the cricoid abscess would possibly have dried up. What voice he would have recovered I do not feel prepared to give an opinion upon, but almost certainly his life would have been saved. As he had attended at two other hospitals, the case was evidently not considered urgent, but it gives a lesson that a patient with persistent stridulous breathing owing to laryngeal mischief, whether ulceration, polypus, or necrosis, is not safe without a tube. It is also remarkable that he never suffered pain even on pressure, or dysphagia.

Had laryngotomy been performed instead of tracheotomy, it would inadvertently have been a wrong selection, as the tube would have been in close proximity to the diseased upper margin of the cricoid, and so would have most probably aggravated the disease.

OBSCURE UTERINE DISEASE.

By Edward Clayton Ling, M.R.C.S., L.S.A.

The following case is, I believe, one of rare occurrence, and of sufficient interest to warrant its publication.

M. A. P.——, aged twenty-six, married, came under my observation for the first time on January 23rd. Her previous history, as obtained from her friends, is as follows:—She acted as cook in different families until twenty years of age, and, although never strong, always enjoyed fair health. At twenty she married, and at twenty-one was delivered prematurely of a child (seven months), which died soon after birth. Between that time and last September (an interval of five years) she miscarried on three occasions, the last time being in July, 1871. At each miscarriage she suffered from severe hæmorrhage, and twice her life was despaired of by her friends. In September she again became pregnant. From the commencement of pregnancy until death she complained to her friends of constant chilliness, never being able to keep herself warm, and of suffering from occasional attacks of faintness; for which, however, she did not seek medical advice. Her appetite remained good, and she slept well. Whilst turning in bed during the night of January 19th she felt something snap in her body, causing considerable pain; and at intervals during the following day she complained loudly of pain (which she referred of the epigastric region), followed by faintness. This condition continued more or less, with long intervals of relief, until the night of the 22nd, which she passed well; and on the morning of the 23rd performed her household work as usual without complaining. At noon she made a hearty meal, and *immediately* afterwards was seized with agonising abdominal pains, followed by intense syncope. At this time I was sent for. On my arrival, I found her in a state of collapse, with a cold clammy skin, glazed eyes, slow heavy breathing, and weak fluttering pulse. Under very energetic stimulant treatment she revived, but was immediately seized with a return of the pain (still referred to the epigastrium, though the whole abdomen was exquisitely tender), and vomited; this was followed by a return of the collapse. She was evidently suffering from internal abdominal hœmorrhage, but the exact seat could not be determined. The uterus felt, when placing the hand over its seat, as it usually does when about four months pregnant; but manipulation caused such excessive pain over the entire abdomen that it could not be much resorted to. The uterus remained perfectly soft during the pains. A digital examination per vaginam disclosed a normal state. No lumbar pains were complained of. Pain and collapse continued to alternate for twelve hours, when she expired.

Autopsy.—The body was well nourished, and without any external marks of violence to account for existing state. The skin was extremely white and clear. (The examination was made eighteen hours after death.) On making the usual incision through the abdominal parietes, the following appearances presented:—The epigastric, umbilical, hypochondriac, and lumbar regions were perfectly normal, and the peritoneal covering was quite healthy, not being the least inflamed. In the hypogastric and inguinal regions, immediately beneath the parietal peritoneum, a large flat clot was lying, reaching completely across the abdomen; this clot was perfectly free, not adhering to any structure, and was easily removed. The abdominal cavity was filled with liquid blood, three pints being mopped out with a sponge and much more remaining. Upon removal of the clot, the placenta was disclosed with its attached surface presenting upwards in precisely the position it would take if adhering normally to the fundus of the uterus. Between the edge of the placenta and the jagged edge of the uterus anteriorly the membranes were bulging, and, by the light of a candle, the fœtus could be seen lying in the usual position for a breech presentation. Posteriorly the placenta was adherent by a strip about two inches long and one-eighth of an inch in depth. The membranes were accidentally ruptured, when the fœtus was easily lifted from out the uterine cavity without causing any further laceration of its walls. The uterus and its attachments were then removed, with a small portion of the vagina, and separately examined. A large hole existed at the fundus—not merely a laceration, but a complete loss of structure, round in shape, four inches in diameter, and almost corresponding to the size of the placenta; the edges of the wound were jagged, but the substance of the uterus was firm and unyielding. The ora internum and externum were closely sealed. The placenta felt firm and appeared sound, though rather bulky in proportion to the fœtus. There was about the uterus a peculiarity worthy of remark—namely, a total deficiency upon the left side of the ovary, Fallopian tube, and ligaments, this side being perfectly free, and the muscular structure smoothly covered by peritoneum throughout. The ovary on the right side was well developed, showing several scars, and three Graafian vesicles in various stages of development, one being large, bulging, clear, and of a pink color. The Fallopian tube and its fimbriæ were also proportionately large.

I sent specimens of both uterus and placenta, cut off from their free edges, to Mr. Arnott of St. Thomas's Hospital, who kindly examined them for me microscopically, and gave the following report:—"The placenta seems to be perfectly normal. The uterine tissue is also generally healthy-looking, but in some parts there are only spoilt-looking granular remains of the muscular structure with extravasated blood: suggesting an injury and subsequent local metritis; or an inflammatory condition producing softening of the tissue and subsequent rupture, with extravasation of the blood into the muscular tissue."

Saxmundham, Suffolk.

CASE OF

HYDATIDIFORM DEGENERATION OF THE OVUM COMPLICATED WITH HÆMORRHAGE; REMOVAL OF THE MASS.

By William C. Wise, M.D. St. And., L.R.C.P. Lond., &c.

M. A——, aged twenty-eight, married, four children (youngest ten months, still at the breast), last catamenia two months, consulted me on October 3rd, 1871, for a discharge of blood from the vagina which had taken place at the same hour for several days prior to her seeking advice. She estimated the quantity discharged each time at from two to four ounces. She had also vomited every morning for two months. The vaginal examination showed the os to be patulous, and the cervix short and plastic, but the speculum revealed no ulceration or other lesion. I diagnosed early pregnancy, directed her to wean the infant, and prescribed rest and the acid infusion of roses with dilute sulphuric acid. As the bleeding continued, in a few days I gave gallic acid in five-grain doses every four hours, and advised her to suck ice and take iced drinks. This treatment being of no avail, I gave ten minims of the solution of extract of ergot (P.B.) with fifteen minims of the tincture of perchloride of iron three times a day, hoping either to stop the bleeding, which had now (Oct. 24th) much increased, or induce the womb to expel its contents; but neither of these results occurring, I substituted for the ergot and iron ten minims of the tincture of cannabis indica three times a day, and ordered vaginal injections of alum. No improvement taking place, the case was now seen by Dr. Mason, of Finsbury-square, London, who ordered fifteen minims of tincture of perchloride of iron three times a day, and a free supply of Wenham-lake ice, and considered that this treatment persevered in would eventually stop the bleeding. At the end of a fortnight the general health, which had previously suffered much, was improved, and the hæmorrhage, although not stopped, was certainly less. At a second consultation with Dr. Mason, as there was a certain periodicity about the discharge of blood, he suggested that two grains of quinine be given with each dose of the tincture of perchloride of iron. This was on December 7th, and on the morning of December 8th, on my visiting the patient, she told me she had had bearing-down, forcing pains since 4 A.M. at regular intervals. On examination, I found the os dilated to the size of a florin, and a soft, firm, flesh-like substance, which I could not break down, planted over the mouth of the womb. There was still hæmorrhage, though slight, which came and went with every pain. After waiting some hours and finding no progress, I gave two half-drachm doses of ergot at an interval of two hours, neither of which appeared to have the slightest effect. I now called in Mr. Mason, F.R.C.S., of Woolwich, who placed the patient under the influence of chloroform, while I with great difficulty worked my hand through the placenta-like substance planted over the mouth of the womb, into the cavity of the latter, from which I scooped out a large wash-basin half full of hydatid-cysts (so called), which resembled a large number of white grapes floating in claret. There was not the slightest trace of any ovum unless the fleshy mass referred to represented the placenta. We got good contraction of the uterus, the patient slowly regained her strength, and was quite convalescent by the end of a month.

Burrage-road, Plumstead, S.E.

A Mirror

OF

HOSPITAL PRACTICE,

BRITISH AND FOREIGN.

> Nulla autem est alia pro certo noscendi via, nisi quamplurimas et morborum et dissectionum historias, tum aliorum, tum proprias collectas habere, et inter se comparare.—MORGAGNI *De Sed. et Caus. Morb.*, lib. iv. Prooemium.

UNIVERSITY COLLEGE HOSPITAL.

PSOAS ABSCESS; ACUTE TUBERCULOSIS; DEATH.

(Under the care of Mr. ERICHSEN.)

THIS case is one that throws some light on the origin of tuberculosis. Since Villemin first announced that tubercle may be introduced into the lungs and other organs of the body by inoculation, the views on the pathogeny of tuberculosis have undergone great changes both in this country and on the continent. It is, however, chiefly owing to the advocacy of Buhl and Niemeyer in Germany, and to the results of experiments carried on by Drs. Burdon-Sanderson and Wilson Fox in this country, that the origin of tuberculosis from a pre-existing inflammation has come to be regarded as a fact. The only point under dispute is, whether every tuberculosis has such an origin; some observers answering in the affirmative, while others as positively deny this to be the only mode of origin. In all cases, however, it is acknowledged that there must be a certain predisposition in the patient to the acceptance of this condition; this predisposition Niemeyer calls a vulnerability of the constitution. The facts appear now to be too numerous to allow of the sequence of events being attributed to a coincidence.

William T——, aged twenty-three, said that up to two years before admission he was quite well, but that about October, 1869, he suffered rheumatic pain in the hip and lumbar region of the left side. For this he was treated at the Great Northern Hospital, and in about two months got well. About October, 1870, he felt some weakness in the back, so that he was unable to rise from the horizontal position without using his arms. In January, 1871, he had a severe cough, and spat up a large quantity of mucus, but no blood. He then lost appetite, and suffered pain in the chest. At the end of February of the same year he noticed a swelling in the left groin, and a week after this a swelling in the left lumbar region. These increased in size rapidly for three weeks, but since then have not grown much. At this time he was ordered cod-liver oil. At the beginning of July, 1871, he had a return of the rheumatic pains, but this time in the fingers and in the forearm of the right side. At the end of October he felt pain as of a cord tied tightly round the abdomen. At the time of admission, Nov. 1st, 1871, he had severe cramp-like pain, which radiated from the right hip down the outside of the thigh to the upper part of the calf. On Nov. 3rd he was examined, and it was found that he had a large, soft, but not circumscribed, swelling in the left groin below Poupart's ligament, and outside the femoral vessels. The swelling could be reduced by pressure, and fluctuation detected by palpation over the region of the psoas muscle above Poupart's ligament. There was another soft swelling as large as an orange in the left lumbar region, extending between the last rib and the iliac crest, and about one inch from the spinal column. When this was compressed, the swelling in the groin increased in size and tension, and *vice versâ*. When standing, the patient kept the left thigh slightly flexed, although he could straighten it. Over the tenth dorsal vertebra, which was prominent, and at the lower lumbar region, there was great tenderness. Impulse on coughing was perceptible in the psoas abscess, but not in the lumbar; but on emptying the lumbar abscess by gentle pressure, an impulse given to the psoas abscess could be felt in the lumbar one.

Nov. 6th.—In the afternoon the lumbar abscess was opened under carbolic spray, and about six ounces of pus evacuated; the psoas abscess was partly emptied through the lumbar.

7th.—11.30 A.M.: Temperature 99·5° F.—8 P.M.: Temperature 100·7°. To have a purgative pill at bedtime.

8th.—11 A.M.: Temperature 98·7°.

13th.—Very slight discharge of a serous character.

14th.—Complains of dragging pains along the spine and in the supra-clavicular region on taking deep inspirations.

15th.—The pain in breathing has gone. Last night the cough was troublesome, but it is better to-day. Temperature normal; breathing rapid.

16th.—For the last three mornings, at about four o'clock, there have been severe sweatings. Breathing easier. To have two grains of sulphate of quinine and ten drops of dilute sulphuric acid three times a day.

20th.—Breathing more oppressed; cough troublesome; large quantities of mucus expectorated; great pain in epigastrium.

22nd.—Cough about the same. Ordered a mustard poultice to be applied to the chest, and to take a draught consisting of fifteen drops of ipecacuanha wine, ten of spirits of chloroform, ten of tincture of squills, and five grains of carbonate of ammonia, three times a day.

24th.—Cough still troublesome. The discharge from the abscess in two days is only about a teaspoonful of serous fluid.

27th.—Cough nearly all gone; no pain in the chest. The sweating is not so profuse.

Dec. 9th.—No discharge in the dressings. There is still tenderness in the spine, but it is less. Patient sat up for two hours yesterday.

12th.—Sits up daily; cough slight; sweating much diminished.

30th.—A pain which he complained of for several weeks at the inner side of the thigh has gone, but there is still tenderness along the spine.

Jan. 19th.—Bowels confined; complains of headache. To have a saline aperient draught.

23rd.—Has had a headache for a week. On the 20th he was sick, and has continued so. The vomited matters are bitter and clear; appetite bad; some tenderness below the costal cartilages in the right hypochondriac region. The saline aperient draught to be repeated.

26th.—The quinine mixture has been vomited for some days past; there is a very severe occipital headache; tongue moist, pink, and only slightly furred; countenance dull and earthy-looking; vomiting continues. Temperature at 11.30 A.M., 99°; at 1.30 P.M., 99·2°.

27th.—Headache and vomiting persist.—11 A.M.: Temperature 99°. The patient sees double; there is no evident strabismus, but there is slight ptosis of the right eyelid. The pain in the head runs round in the line of the eyebrows from the back of the head; now and then there are paroxysms of pain. The sickness is not relieved by vomiting. Ordered a cantharides blister to the nape of the neck, ice to the head, an enema daily, to have ice to suck, and to take twenty drops of the solution of morphia at bedtime.

28th.—The diplopia continues; the images are of equal intensity, but one is placed just above the other. The patient lies with the legs drawn up; abdomen somewhat retracted.—11.30 A.M.: Temperature 100°; pulse 58.

29th.—He seems to be semi-comatose; answers when spoken to; is constantly muttering to himself, and draws up the bed-clothes and bites them.—11 A.M.: Temperature 99; pulse small, 80.

30th.—The patient died at 2 A.M. Just before death he had hiccough and vomited blood. After death the pupils were noticed to be unequally contracted.

Autopsy.—On removing the dura mater the surface of the brain was found to be dry; veins greatly distended and convolutions flattened. The ventricle were greatly distended with fluid, and on opening them the septum lucidum was seen as a delicate membrane. The grey matter was very dark, and contrasted strongly in color with the white substance. There were abundant grey granulations along the vessels in the fissure of Sylvius, and a few were to be seen elsewhere. There was a large quantity of lymph at the base of the brain, especially about the nerves. The heart and pericardium natural; both pleuræ adherent throughout by recent lymph, which was easily torn; lung-substance dense and congested. On section grey granulations were seen uniformly distributed from apex to base. There was some consolidation and caseous matter at the right apex. The peritoneum, especially over the surface of the liver, spleen, and mesentery, was scattered over with grey granulations. Grey granulations were found in and on the liver, and here and there some larger masses of yellow tubercle were to be seen. Spleen large, vascular, and soft. The kidneys were soft and friable, and some grey granulations were scattered through the right one. The stomach contained about a pint and a half of chocolate-colored fluid. Mucous membrane congested and soft, with numerous patches of capillary hæmorrhage, especially in the region of the pylorus; no ulceration could be detected. In the intestines a few tubercular ulcerations were seen. The testicles and bladder were healthy, but a cheesy mass was found in the prostate. On removing the peritoneum the psoas abscess on the left side was seen immediately beneath the fascia of the psoas muscle, and could be traced along the outer border of this muscle to the brim of the pelvis, where it crossed to the inner side and ran along the femoral artery as far as the profunda. About the middle of this course an offset passed backwards above the iliac crest; this was the abscess that was opened in the back. Joining this was another abscess which passed under the psoas, and ran along the brim of the pelvis below the muscle, till it joined the first one. On the right side was another abscess, reaching from the third lumbar vertebra downwards to half way along the brim of the pelvis. This abscess communicated with that of the opposite side, at the level of the body of the fourth lumbar vertebra. An abscess also passed down the front of the sacrum as far as the third piece. The bodies of the third, fourth, and fifth lumbar, and upper three sacral vertebræ were almost bare, being covered with only a thin layer of granulations, through which spicula of bone could be felt. The intervertebral substance between the three upper lumbar vertebræ seemed healthy, but that between the last lumbar and first sacral had disappeared, there being in its place a large cavity containing masses of diseased bone. The abscesses contained a thick creamy matter of a yellowish color, and of the consistence of cream-cheese.

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HOMERTON FEVER HOSPITAL.

THE COLD BATH IN THE TREATMENT OF ENTERIC AND TYPHUS FEVERS.

(Under the care of Dr. ALEXANDER COLLIE.)

WE purpose giving a short account of some cases of typhus and enteric fevers treated at this hospital by the cold bath. That in some of the following cases this treatment was very useful is beyond a doubt, and in none is there any evidence that the disease was made worse by it, not even when severe lung affection co-existed. The patient in the second case seems to have been *in extremis*, and it would require very great faith in the *vis medicatrix naturæ* to believe that the patient recovered in spite of the treatment, for the beneficial effect of the bath was constant and well-marked. It is further interesting to note that in those cases which ultimately did well the immediate effects were beneficial, the temperature being lowered and sleep produced.

Case 1 was a young woman, aged twenty-eight, admitted on October 29th, suffering from a severe attack of enteric fever complicated with bronchitis. She became convalescent on November 14th. On the 27th, however, diarrhœa came on, the respiration became hurried, and the tongue dry and fissured. On December 3rd the temperature in the rectum was 104·4° F.; pulse 132; numerous râles could be heard through both lungs; the abdomen was distended and the bowels loose, but there was no eruption. In this condition the patient was placed in a bath of a temperature of 80° F., which in fifteen minutes was lowered to 67°. In ten minutes after being put into the bath the thermometer in the rectum registered 100·4°. After being removed from the bath the patient immediately went to sleep. On December 4th the temperature in rectum was 105°, but fell considerably after a bath of 68°, lowered to 52° in fifteen minutes.

On the 5th, 6th, and 7th, baths were given. On the 8th the temperature was normal, but on the 9th it had gone up to 100·2°, and the bath was repeated, when it fell again to 98·6°. On the 10th, thirteen days after the relapse, the patient was convalescent; temperature normal. She was discharged on January 30th.

Case 2 was a young woman, aged twenty-five, admitted Dec. 5th with but slight pyrexia, but on the 10th well-marked typhus rash was present. Temperature 103·4°; pulse 160; tongue dry and brown; well-marked carphology, restlessness, and delirium. A cold bath was thought of as a means of producing sleep; but as the patient was in a state of extreme constitutional depression, it was feared that the bath might be followed by dangerous results. She was therefore ordered a pill containing half a grain of opium, and to have cold cloths applied to the head. The opium did not produce sleep, and from this time up to the 15th, probably the twelfth day of the disease, the patient exhibited subsultus, twitchings, floccitatio, deafness, and all the signs of threatening dissolution. Pulse 132, soft; temperature 100·6°. A bath was ordered of 80°, to be cooled in twenty minutes to 60°. During the bath the patient's pulse became alarmingly feeble, and stimulants were administered. In about twenty minutes after being put into the bath the temperature of the patient fell to 97·8°. After the bath three ounces of whisky and a raw egg were given, and the patient was wrapped up in a warm blanket, and soon fell asleep. By midnight the temperature had gone up to 101·4°. On the 16th (thirteenth day of disease) the temperature was 103·4°; a bath was ordered from 77° to 54°, but this time the temperature fell only one degree, but sleep followed the bath. On the 17th the temperature was 104·8°, and, notwithstanding the presence of severe bronchitis, a bath from 90° to 60° was ordered, and the temperature fell to 96·2°, and the patient slept well. On the 18th the patient was better, the nervous twitchings were less, and the bath was not repeated. On the 20th the temperature was normal. On Jan. 29th patient was discharged.

Case 3 was a married woman, aged twenty-eight, admitted Dec. 12th, who said that she had been ill ten weeks, but of what was not clear. A bath of 90° lowered to 52° in thirty minutes had the effect of bringing down the temperature from 103·2° to 100°. No immediate sleep followed the bath, but the patient slept well during the night. On the 14th the bath was repeated, and the temperature lowered, and the patient slept immediately after; and although by midnight the temperature had risen to 105°, the patient had a good night. On the morning of the 15th the temperature was 104·2°, but was lowered to 102·2° by a bath, but by midnight it had risen again to 105·4°. On the 16th the bath was not repeated, as the patient was sweating freely. On the evening of the 17th the temperature was 104·4°, but fell to 102·6° after a bath. In the evening of the 18th the temperature was 103°, but the bath was not repeated. In the morning of the 19th it was 102°. From this time to the 28th the temperature ranged about 100°, when it became normal, the patient being discharged on the 31st of January.

Case 4 was a boy, aged seventeen, admitted on Dec. 17th with enteric fever. On the 22nd, after a bath of 85° to 58°, the temperature fell from 105·2° to 102·8°, but no other good result followed. On the 23rd the temperature had risen to its former height, but as the patient objected to the bath it was not insisted on. On the 24th, however, the urgency of the symptoms suggested the bath, which was accordingly given, with the effect of lowering the temperature from 104·8° to 101·8° The patient died sixteen hours after this bath, and at the autopsy extensive peritonitis and perforation of the bowel were discovered.

The following cases do not argue so much in favor of the cold-bath treatment, yet they are not without interest, and at least hint a caution. The subject is worthy of a much more careful investigation than it has hitherto received; it is, however, to those who have charge of large hospitals, where this treatment can be easily carried out, that we must look for information.

Case 5 was a woman, aged thirty-eight, suffering from typhus fever. On the eleventh day of the disease she had had hardly any sleep from the commencement of the attack; eruption abundant; tongue dry; a few râles were to be heard through both lungs; temperature 104·4° F. After being twenty minutes in a bath of 90° to 70°, the temperature had apparently risen to 104·8°, but in the next ten minutes, the bath being lowered to 60°, it had fallen to 103°. Directly after the bath the patient slept for two hours and a quarter; eight hours after the skin was moist and covered with perspiration, the temperature being 104°. On the twelfth day of the disease the bath was repeated, and in thirty-five minutes the temperature was lowered from 104·2° to 102·2°. As no sleep followed the bath an opium pill was ordered, but without effect. Next day the patient had four hours' sleep after an opium pill, and the bath was not given. On the fourteenth day of the disease the temperature was lowered, after a bath of 90° to 60° for twenty minutes, from 104·8° to 104°, but the bath having been lowered in the next twenty minutes to 56°, the temperature fell to 102·6°. Sleep did not follow this bath, and the reaction was slow, the patient expressing herself as feeling "cold" for some time after. A few hours after the bath one ounce of whisky every hour was ordered. At 2 A.M. on the fifteenth day, the pulse was 136, stronger, and the patient had had intervals of sleep of about twenty minutes at a time, and on half a grain of opium being administered she slept three hours. On the morning of the sixteenth day the temperature was 104·2°; pulse 140, full and strong, the eruption well marked. The whisky was now reduced to twelve ounces in the twenty-four hours. The baths, not being considered satisfactory in their result, were not repeated. In the evening of the same day there was some increase of the bronchitis, for which she was ordered a mustard, to be followed by a "jacket, poultice," and to take one ounce of an ammonia and senega mixture every three hours. Notwithstanding constant feeding and stimulation she gradually sank, and died at early morning of the nineteenth day of the disease.

(Under the care of Mr. PERCIVAL.)

Case 6 was a woman aged twenty-four, admitted on the ninth day of the disease (Dec. 16th), with enteric fever; temperature 104·6°, pulse 140.

Dec. 17th.—The morning temperature of 105·2° was lowered to 101·2° by a bath from 80° to 56° in twenty-four minutes, and the patient slept soundly for some time after; in the evening, however, the temperature had risen to 104·6°.

18th.—In the morning the temperature was 105°, but fell to 98·4° after a bath cooled from 82° to 56° in twenty-three minutes. Slept well after the bath. To have twelve ounces of whisky per diem. Temperature in the evening 105·2°.

19th.—Morning temperature 104·4°, which a bath from 80° to 56° in twenty minutes brought down to 99·4°. The patient slept well after this bath, but as in the evening the temperature had risen to 105°, another bath from 86° to 52° in twenty-seven minutes was given, after which the thermometer in the rectum registered 101·2°. Great depression, from which the patient was with difficulty roused by enemata of whisky and beef-tea, as she could not retain anything on the stomach, followed this bath; nor did sleep ensue.

20th.—Temperature in the morning 103°, evening 104·8°; but no more baths were given, on account of the alarming depression produced by the last one. Râles at back and both sides of the chest.

22nd (fifteenth day of illness).—Temperature (evening) 105·8° after profuse diarrhœa during the day. The patient in a state of extreme exhaustion; a sloughing sore between the folds of the nates. Enemata of starch and opium ordered for the diarrhœa.

From this time the patient slowly improved; the sores cleaned and rapidly healed. Patient discharged in good health on Feb. 15th, 1872.

These cases are too few in number to justify any definite statement as to the indications for or against the cold-bath treatment in acute disease. There is no evidence that the course of these specific fevers is shortened by such treatment; on the contrary, the evidence points rather the other way, but in this may consist its chief remedial value, as the pace of the disease is, as it were, modified, so that the system gets regular intervals of rest, and is thus enabled to bear up against what might otherwise be fatal. This is not by any means a novel mode of treatment; for as early as the year 1702, Floyer strongly recommended it; and, in 1729, Hoffman said that, "employed freely and not too cold, it is the best remedy in acute diseases, and in chronic diseases one must prefer it to all other means." From this time to 1801 it was advocated by the German authorities Sigismund, Hahn, Unzer, Zimmermann, and others, when a new impetus was given to it by Dr. Currie, who adopted this treatment very extensively and successfully at the early part of the present century. It must, however, be remembered that Currie began his treatment often before a correct diagnosis could be made; and there is no evidence, in a large number of his cases, that the febrile states would have developed into any serious specific febrile disease. This practice did not continue long, and excepting isolated observations—such as those of Drs. Wilson Fox, Stallard, and Weber,—it has, in this country, ceased to be employed by the regular profession in acute diseases. It is of great importance to determine, not only that cold-water baths will reduce temperature in certain well-defined acute diseases, but whether this reduction of temperature is followed by other good results.

LONDON HOSPITAL.

SYPHILITIC DISEASE OF THE SKULL.

Mr. Hutchinson has lately had under his care a series of interesting cases of syphilitic disease of the cranial bones. One of the cases opens up the vexed question of syphilis lying dormant, as it were, many years, and then springing up into active mischief. These and kindred cases should teach us to be always guarded in our prognosis in a case of syphilis; for, although the disease may be apparently cured for many years, yet there is abundant evidence that symptoms may show themselves ten, twenty, or even thirty years afterwards, the patient probably in the meantime having married and had children, who may or may not be free from symptoms of hereditary syphilis.

Syphilitic disease of the skull, and probably of the brain or dura mater, with nodes and extensive ulcerations on other parts; fits, mental derangement; iodide of potassium ill-tolerated, but producing rapid healing of the ulcers.—Maria F——, aged forty-three, was admitted on Jan. 27th, 1872, in a most wretched state from destitution and the suffering caused by numerous large syphilitic ulcers. She had many deep ulcerations on the scalp, extending to the bone; there were besides large ulcers on the right shoulder, buttocks, and thighs; and a periosteal node on each tibia, ulcerated and exposing bare bone on one leg, unbroken on the other. The ulcers on the scalp were chiefly on the right side. Her memory was extremely defective; and she stated that she had had several "fits" within the last two or three years. She had a fit after having been a short time in the hospital, and, when called to her, the house-surgeon found her comatose, with flushed face, relaxed muscles, and heavy breathing. After this her mind was more or less deranged for several days; she thought other patients had designs against her, was restless, and talked incoherently.

According to her statements, which, however, could not be entirely relied upon, she had had no ulcerations or other eruption until about six months before admission. Fifteen months before admission her right eye was excised at St. Bartholomew's, possibly (from her account) for neglected glaucoma. She had been married fifteen years, and had been pregnant twice; the first ended in a miscarriage, and the second (a boy) died at the age of four months. Catamenia ceased when she was forty years of age. It is probable that her bone and skin disease was much aggravated by destitution, for she was half-starved. She did not bear iodide of potassium well: it salivated her. After a few days' intermission, however, she resumed it, and all the ulcers rapidly healed. Black wash was applied locally.

Syphilis quite latent for thirty to forty years from the primary disease; appearing after that interval as nodes on the cranium, probably induced by a blow on the head.—Antony E——, aged sixty, a robust, healthy-looking farm-laborer, was under Mr. Hutchinson's care in April last for syphilitic caries of the cranium. There were five or six sinuses in the frontal region, all leading to bare bone. Exploratory incisions were made after he had been in the house several weeks, to ascertain whether any of the bone was loose; it was still quite fixed, a surface as large as one's palm being quite bare. The man stated that the sinuses were preceded by the formation of lumps on the head, which enlarged and then broke; these had begun to form about nine months before he came under care, and he attributed them to a blow on the head from a falling wheatsheaf.

He at first denied ever having had venereal disease of any kind, but afterwards remembered that between thirty and forty years before the occurrence of the cranial nodes, he had had a chancre. It was not, so far as he remembered—and he was carefully questioned—followed by secondary symptoms of any kind, nor had he had the slightest reminder of it until the lumps formed on his head. He has brought up a large family without losing any children, and none of them appear to have had symptoms of hereditary syphilis; the children were not seen, however.

This man was seen again three months after his discharge from the hospital, and the dead bone was still quite fixed.

Syphilitic disease of the cranial bones; abscess in the brain; epileptic fits; evacuation of pus; relief.—Oliver H——, aged forty, probably had syphilis eleven years ago. He worked in a mercury mine many years, but gave up this employment nine years ago.

In June, 1871, he was admitted into this hospital with periostitis of the frontal bone, which appeared from the history to have commenced seven years before. When in the hospital there were repeated suppurations of the frontal nodes. In 1870 a piece of bone had been removed. Seven weeks before his admission in 1871 an abscess had formed about the left upper eyelid; this broke, and when the wound had healed there was considerable retraction of the upper lid, so that the eyeball was not covered by the lids. The cornea then inflamed and perforated, notwithstanding all the efforts made to keep it covered. The scalp was riddled with sinuses, leading to large boggy swellings, from which great quantities of fetid pus escaped. On June 28th the lens escaped from the left eyeball, and on the 29th the eyeball was extirpated. At the same time one small and two large pieces of bone were removed from the frontal region, leaving the pulsating dura mater covered only by thin granulations. On July 10th a large abscess formed about the right upper eyelid, and a large piece of bone was removed from its cavity. This continued to discharge pus for some time, but there was no retraction of the lid.

In January, 1872, another piece of bone was removed from the right frontal, and in February another piece of bone was found to be bare, but not loose.

On the 22nd of July he was readmitted with extensive suppuration under the old scar on the right side. Loose bone was felt. The patient also suffered from one-sided convulsions, commencing in the left fingers, and passing up the arm to the face on the left side, and then down to the leg on the same side, the right side not being affected. He complained of great pain at the back of the head, especially at the left side. On manipulation, and on removing the piece of loose bone, a large quantity of fetid pus was seen to come from the right hemisphere into which the probe could be passed for about four inches. A free escape for the pus was allowed. There was no optic neuritis, nor cardiac disease.

July 23rd.—No more fits; no pain; pus comes away freely.

24th.—No convulsions. There is some loss of power over the left arm; but the pus escapes freely. There is also slight loss of power in the left leg.

25th.—There is slight but decided facial paralysis of the left side.

Aug. 1st.—There is more power in the left arm. There is some exposed bone, which is, however, firmly adherent to the scalp on the right side. On passing a probe into the opening through the dura mater, a quantity of thin fetid pus escapes *per saltum*. The silver probe is at once tarnished by the fluid. A small piece of dead bone was removed from the left parietal bone.

29th.—The abscess seems to have healed. No pus now escapes on probing. The weakness of the left side is less, and the patient is progressing fairly well.

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ST. THOMAS'S HOSPITAL.

ACUTE EMPYEMA; THROMBOSIS OF JUGULAR VEIN AND CEREBRAL SINUSES; DEATH.

(Under the care of Dr. BRISTOWE.)

FOR the notes of the subjoined case we are indebted to Mr. B. Addy, house-physician.

E. N——, aged sixteen, was admitted into Christian ward on June 17th. He was taken ill on the 15th, with headache, sickness, and pain in the left side and his "bones." When admitted, there were the physical signs of fluid in the left pleural cavity. Temperature 105·2° F.; pulse 148; respiration 40.

June 19th.—Temperature 102·6°; pulse 120; respiration 40. Heart almost wholly to the right of the sternum; sounds healthy. Left side of chest motionless, and dull all over except at the apex, where the percussion-note is high-pitched; breath-sounds scarcely audible except at the apex and along the spine; vocal fremitus absent. Some ægophony along the spine; no crepitation, friction, or tubular breathing. Right side healthy, and measures 14 inches, left side 14¾ inches. To continue the mixture ordered on admission, consisting of carbonate and acetate of ammonia, and nitrous ether; to have beef-tea, milk diet, and brandy.

Paracentesis was performed, and eighteen ounces of purulent fluid withdrawn; after which the patient's condition improved.

20th.—Temperature 100·5°; pulse feeble, 100.

21st.—Temperature 99·4°; pulse stronger.

22nd.—Has been delirious every night since admission. Heart still beats to the right of the sternum. Left chest hyper-resonant in front; breath-sounds feeble and somewhat blowing; some subcrepitation at the base; no metallic sounds. Patient sensible, but dull. Temperature 99·1°; pulse 100; respiration 24.

24th.—Seems a little better; tongue cleaning; temperature 100°.

25th.—At 1.30 A.M, had a convulsion, which passed off before the house-physician could reach him; but in a few minutes had another convulsion, in which the head was retracted, right arm and leg violently convulsed, while the left arm and leg were motionless, and when raised dropped like a dead weight. Face not affected; pupils dilated, but reacted slowly to light. During the twitchings the right arm was bent at a right angle, the hand presenting the appearance of "wrist drop," and the fingers bent upon the palms. This lasted about half an hour, when the twitchings ceased, the patient being unconscious.—2.30 A.M.: More conscious; moves the left arm and leg quite well; but now there is some weakness in the right extremities. After this there were two or three more slight convulsions.—10 A.M.: Temperature 104·2°; pulse 140. Tracheal râles; breathing accompanied by groaning; frequent twitchings of the head, body, and limbs. Pupils dilated, equal, and reacting to light.—2.45 P.M.: Temperature 104·2°; left pupil dilated; tracheal râles.—6.40 P.M.: Died.

Autopsy twenty hours after death. (Notes by Dr. Payne.)—Before opening the thorax it was ascertained that the left pleural cavity contained air. The left pleura contained about twenty-two ounces of thick pus. Both visceral and parietal walls inflamed, and covered by a layer of lymph. Right pleura healthy. Left lung collapsed; no sign of hepatisation. The right lung distended, crepitant, and natural. A small branch of the pulmonary artery contained a mottled clot of some standing; but the corresponding portion of lung-tissue was not effected. Heart natural, but so displaced that its left boundary was the middle line of the sternum. The dura mater was injected. The small veins entering the longitudinal sinus and the sinus itself were filled with a rather firm coagulum, somewhat mottled, and probably some days old. The clot extended all through this sinus, and also into the descending and lateral sinuses of the left side, but not of the right; thence into the left jugular, which was quite filled; and thence into the innominate. All this clot was undoubtedly old. On the other hand, the subclavian vein was filled with soft black clot, evidently more recent, and yet not post-mortem.

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CANCER HOSPITAL.

INDURATED CYSTIC TUMOUR OF THE MAMMARY GLAND FROM NON-EMPLOYMENT DURING LACTATION.

(Under the care of Dr. MARSDEN.)

THE origin of tumours is generally so obscure that a case whose antecedents are tolerably clear is always interesting. The following history shows that the development of the tumour was in all probability due to the disuse of the gland in which it was formed during lengthened periods of lactation with the other gland. We are indebted for the report of the case to Dr. Crombie, house-surgeon.

E. H—— was admitted into the Cancer Hospital with a tumour in the right breast about the size of a small orange, irregular in outline, hard and nodulated on the surface, but without adhesion to the skin or muscle, and without implication of the axillary glands. It is to be remarked that, although the patient was only twenty-four years of age, and in other respects a tolerably healthy-looking woman, her sound breast was a very ill-developed one. The tumour was first observed about ten months ago, after she had discontinued for two months to suckle on that side, owing to soreness of the nipple. She had therefore suckled on both breasts for seven months, and for nineteen months on the affected gland, as she weaned her child only on admission into the hospital. She had followed much the same plan on three previous occasions, discontinuing to use the right breast after a time in consequence of soreness of the nipple, and going on suckling with the other for eighteen or twenty months. She had never observed any tumour in the neglected gland, but it had often felt uneasy, and had always continued to discharge milky fluid from the nipple as long as milk remained in the opposite breast. Iodine paint had been used for the tumour repeatedly, but without any beneficial result, there being a gradual increase in its size and hardness. It did not occasion much pain.

No perceptible effect having been obtained by the treatment employed, Dr. Marsden removed the whole gland on the 24th July.

On examination no trace of gland-structure could be detected, the tumour consisting of a dense solid material surrounding a number of small cysts of various shapes, full of rich yellow cheesy or creamy matter, evidently the inspissated secretion of the gland without any admixture of other materials. The color of these contents was most marked in the portions next the walls of the cysts, which were more deeply tinted, being of a brownish color; while the portions furthest removed were nearly white. The solid portion was made up of a hard, greyish, elastic material, without any appearance of structure to the naked eye; and a section examined under the microscope presented the appearance of molecules destitute of any cellular character, arranged in lines of considerable regularity. Although several portions were examined, nothing like a cell could be found. Strong nitric acid had little action when applied on the surface of a section, but hydrochloric acid acted powerfully—the opposite to their behavior when applied to the skin.

There seems little doubt that the discontinuance of the normal relief of the one gland while lactation was fostered for an undue length of time, in a woman with weak breasts, by the use of its fellow, was the occasion of the stagnation and deposition of the more solid elements of the secretion and the subsequent induration of the gland.

The wound healed slowly, but presented a good healthy cicatrix when the patient left the hospital on Aug. 6th.

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GENERAL HOSPITAL, BIRMINGHAM.

TREATMENT OF PLEURITIC EFFUSION BY DIURESIS.

(Under the care of Dr. WADE.)

IN the following case (for the notes of which we are indebted to Mr. E. Rickards, resident pathologist), pleuritic effusion had remained ten weeks, during which time various modes of treatment had been adopted for its absorption, but in vain. On admission the patient was ordered the diuretic mixture given below, and immediately the quantity of the urine was increased, and coincidently the effusion began to diminish, and rapidly disappeared. Three months before admission the patient, a boy aged thirteen, had got wet through; fifty hours after this he complained of a severe pain in the left side, which was worse on movement of any kind; he also suffered from a cough, was thirsty and hot. In a week the pain was gone, and the patient went to work, but in a fortnight he became worse, and was ordered to keep his bed. He was then freely purged, and had the left side of the chest blistered, but as he did not improve, he was sent to the hospital on June 22nd, when his temperature was 104° F., pulse 120, respiration 44; eleven ounces of urine being passed in the twelve hours. Over the left part of the chest there was absolute dulness, loss of vocal fremitus, and almost inaudible respiration. The dulness extended up to and above the clavicle and to two fingers' breadth to right of sternum. At the back the left side gave the same physical signs. On the right side the chest was healthy and breathing exaggerated. The heart was displaced to the right, the impulse not perceptible, the sounds being heard loudest on the right of the sternum. Ordered two grains of mercury pill night and morning. Also to take three times a day an ounce of the following mixture: iodide of potassium, twenty grains; citrate of lithia and bicarbonate of potash, of each two drachms; spirit of nitric ether, four drachms; infusion of digitalis, three ounces; water, to eight ounces. To rest in bed, and have the hospital milk diet.

June 24th.—Temperature 102°; pulse 100; respiration 40; urine, forty-four ounces in twenty-four hours. Dyspnœa less.

26th.—No dulness on right of sternum, nor under clavicle on the left side. Breath sounds in left infra-clavicular region more vesicular.

29th.—Dulness only extends to the left edge of sternum; percussion note over the second intercostal space good; heart sounds loudest to the left of sternum; vesicular respiration and redux friction down to the upper border of the second rib. Temperature and pulse normal; urine, sixty ounces in twenty-four hours.

July 2nd.—Heart in normal position; vesicular breathing and redux friction down to the upper border of the third rib; urine, sixty-eight ounces in twenty-four hours.

6th.—Leathery friction felt and heard over the whole of the left chest, front and back; dulness gone; vocal fremitus normal; breath sounds good.

10th.—Friction gone. The patient can walk very briskly without suffering dyspnœa.

Dr. Wade remarked that many cases of serous pleuritic effusion will yield to diuretics. Experience shows that small doses of iodide of potassium and blue pill will increase the power of diuretics and expectorants. The salts of lithia, of which the citrate is the best, will often increase the renal secretion when those of potash fail. It is well, however, to combine the two. Had diuresis failed in this case, blisters would have been tried, for in such a case a single blister may set up an absorptive action sufficient to remove the whole of the fluid. More frequently a second or third blister is required. But it will not seldom be found that the absorptive action set up by a blister, though well marked, is not continuous nor abiding. In such a case it must be repeated. It is of advantage to dress the blister with green iodide-of-mercury ointment. Blisters probably act as stimulants, or co-irritants, rather than counter-irritants; hence they should not be used in the early stages of acute pleurisy or pericarditis with a tendency to fluid effusion, which they would, under such circumstances, increase. In dry pleurisy a blister may be immediately applied if the pain be great, and a cure is at once effected. Such cases often depend on a rheumatic or syphilitic tendency. If diuresis and blisters fail, then tapping must be resorted to, care being taken to introduce the trocar at a point that has been previously ascertained to be free from adhesion.

Editorial.

RECENT MEDICINE.

It is well sometimes to take stock of medical work and note the drift of medical doctrine. It may seem to some reverant minds that there is no such thing left as medical doctrine; that each man is his own teacher and does what is best in his own eyes. And, no doubt, of late years there has been a good deal of iconoclasm. There has been much change of opinion and practice. Men have not gone to their old books with so much frequency or so much faith as they were wont to do. And yet an observant critic of the profession will be more struck with the conservatism of practice than with the changes of it. Extreme fashions of practice are altered, even as regards the bulk of the profession; but even in regard to these the great old principles are still accepted. Elimination, evacuation, counter-irritation, action on the liver by mercury, and even the antiphlogistic use of mercury, are ideas upon which the details of much medical practice are based. The professional mind is essentially conservative. It cannot readily disregard the past. It is not our intention to condemn this feature of the professional mind, but rather to recognise and rely upon it, in indicating a few of the points of recent medicine which seem most to distinguish it from the medicine of thirty or forty years ago. There is much that is vague and crude, of course, in the suggestions of modern medicine. But it is not the less to be respected because it is modest and slow to formulate binding rules of practice, and because it has worked largely in attempts to prove many errors of the older system of therapeutics.

Among the features of recent medicine is a disposition to take a very different view of pyrexia from that which our forefathers took. We have so often lately insisted on the significance of pyrexia and on the treatment of it that we need not go into the subject at any length. By the way we may say, that no medical man now should be without a clinical thermometer, which enables him accurately to gauge the degree of pyrexia. We shall only repeat here that the febrile state involves a large destruction of the nitrogen-containing tissues, which are hardly consumed at all in health. Every degree of fever heat, or of heat above 98·4°, implies so much additional destruction of the most important organs of the body, such as the heart and muscles and nerve-centres, as well as a corresponding addition to the work of the excreting organs. Such temperatures therefore imply, not strength, but a tendency to exhaustion, which is to be met by the introduction of food in such forms as can be appropriated. The elevation of temperature in itself may become a cause of rapid or almost sudden death, or, short of this, of great distress and of danger to the patient. Our own pages have contained some striking illustrations of the use of cold as a remedial agent in certain cases of rheumatic fever, typhoid, &c. We need not stay to contrast the present treatment with the older treatment of states of pyrexia, whether connected with local inflammation or with some general disease of the system like rheumatic or typhoid fevers.

Another change of a very practical kind in medicine has reference to the nature and causes of *lithiasis* as a persistent condition. This, again, depends very much on changes in physiological teaching as to the formation of urea. Lithiasis we all know to be a symptom of frequent occurrence. We speak of the conditions of the system in which, without much pyrexia, there is a tendency to red deposits in the urine, with more or less loss of appetite and proneness to gouty or rheumatic affections of the joints. The older notions of this condition tended to inculpate the renal organs. According to our present views, physiological and pathological, the liver is the organ more to be blamed, inasmuch as it seems largely or principally concerned in the formation of urea, from materials supplied to it either by the disintegrating tissues of the body, particularly muscles and nerves, or by excess of certain kinds of food. The liver which has been so much used and abused in medical practice, was rarely made responsible for the production of lithiasis. And yet there is little doubt, as Cyon and other physiologists have shown, that it is in the liver that urea is formed. Sir Henry Thompson has ably applied this doctrine to surgical purposes; and, indeed, one of the best medical expositions of the causes and cure of lithiasis will be found in Sir Henry's lecture published in our own pages in May last. His views as to implication of the liver in such disorders are consistent with the most recent physiology, and his instructions as to diet and medicines are worthy of the best informed physician. Sir Henry tells us, as a good justification of his treatment of the subject, that nineteen out of twenty calculi have uric acid for a nucleus. But it is not every surgeon that would think himself called on to go minutely into the most recent physiology and therapeutics of lithiasis. Sir Henry's views as to the non-use of mercury and of most alcoholic drinks other than Bordeaux or Rhine wines are especially worthy of note, as those of a practical surgeon.

It has been charged against recent medicine that, while very much like a department of physiology, it takes little interest in therapeutics, and especially little in the use and action of drugs. But the charge will break down when minutely investigated. Doubtless many fancies as to the therapeutical uses of drugs have been rudely demolished. This, however, is only a less valuable service than the demonstration of the real powers and properties of drugs. It is no slight advantage to know that mercury will not cure pericarditis or pneumonia. And there is a growingly large number of facts as to the wonderful powers, physiological or therapeutical, of medicines; and the accuracy with which they have been observed and recorded is no mean note of the medicine of the present day. The illustrations of the action of digitalis in toning the heart; of the action of the bromide of potassium in controlling the epileptic tendency; of the value of opium or morphia in inflammations—as of the eye, the stomach, or intestine; of the anti-pyrexial powers of quinine; of the value of mineral acids in various conditions and diseases of the system, and the use of mineral waters in others; and the way in which new and striking therapeutical effects have been got by pushing the administration of drugs, such as iodide of potassium and quinine,—are a few of the facts which show that faith in medicines has a sounder basis than it ever had. On the whole, if there is more faith in nature than there was, and a greater fear of thwarting any of the good tendencies of natural processes, there is also a more intelligent appreciation of the degree and way in which nature can be assisted.

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STRUCTURE OF THE CONNECTIVE TISSUE AND TENDON.

A paper contained in M. Brown-Séquard's *Archives de Physiologie* for 1869 by M. Ranvier has proved to be the starting-point for a new series of researches on the structure of connective tissue and tendons, which has recently been taken up by M. Boll and Dr. Mitchell Bruce, and which has led to a description of this tissue materially different from that contained in the text-books of some twenty years ago, as well as from the more modern doctrines of Virchow. The latter observer maintained that connective tissue was originally composed of cells, the walls of which became thickened and condensed, forming the intercellular substance, and ultimately underwent fibrillation. The central portion both of the cells and of the processes given off from them he believed remained hollow, and by their inter-communication constituted a canalicular system, ministering to the nutrition of the tissue. The researches of Recklinghausen on the cornea to some extent effected a modification of Virchow's views, by showing that the true cells of that membrane did not form, but were contained within, the stellate cavities and spaces, through which they moved by virtue of their amœboid activity. The researches of Kühne on the intermuscular connective tissue of the frog showed that in this animal the cells of that tissue are destitute of a cell-wall, and are not contained in canals, but wander freely through the meshes of the connective tissue. It followed that, in the frog at least, the general idea of a plasmatic system, afforded by the study of the cornea after impregnation with nitrate of silver, could not be extended to the connective tissue generally.

Ranvier commenced by examining the tendons in the tail of young rats, which, after removal from the body, were stretched on the slide, kept in position by a drop of sealing-wax, stained with carmine, and acted on with dilute acetic acid. On examination with a magnifying power of 100 diameters, the tissue was found to present parallel continuous red striæ, which, under higher powers, proved to be cylindrical tubes; and these were seen to be crossed by transverse lines, dividing them into nearly equal segments. On slight pressure being exercised, each of these little segments or cylinders split longitudinally, and, unrolling itself, proved to be a square cell-plate or lamina, with a nucleus near its centre. Around the cell-tubes is an elastic tube or sheath. The tendons are thus, according to Ranvier, permeated by a system of parallel tubes, the walls of which are composed of square cells rolled round till their edges meet, and placed end to end. The stellate figures insisted on so strongly by Virchow as representing corpuscles, he regards as being merely sections of the superficial layer of connective tissue investing the several fasciculi of the tendon, and in these septa his cellular tubes run. The stellate figures brought into view by staining in the subcutaneous connective tissue he also regards as purely illusory; and endeavors to prove it by producing artificial œdema with injection of various fluids into the living animal, and then freezing it before making sections.

A somewhat different view has been lately advanced by Boll. Boll describes the cells of connective tissue as granular, elastic, quadrilateral plates, which are arranged in rows and bend around the fasciculi of the tendon. Each plate has a median stripe, which he calls the elastic stripe, which is caused by a thickening of the cell-substance parallel to the long axis of the tendon, and is stained deeply with carmine. It is evident that a row of cell-plates will give a corresponding row of such stripes—that is, a continuous stripe; and this elastic stripe will assume a straight or wavy appearance, varying with the condition of relaxation of the tendon.

Dr. Mitchell Bruce followed Ranvier's method, but gives a different interpretation of the appearances presented. Accepting the views of Boll on the relation of the cells to the fibrillar bundles, he describes fine lines crossing the fasiculi and dividing them into quadrilateral spaces, in the centre of which a nucleus comes into view on the addition of acetic acid; whilst, after the more prolonged influence of this reagent, the fasciculi become considerably swollen and transparent, and the quadrilateral plates present irregular ragged borders. With nitrate of silver staining, an investment of cells, beneath which a system of serous canals appeared in some places, is brought into view. The cells are rendered still more distinct by the chloride-of-gold method, or by a combination of the silver and gold methods. His view is that the cell elements in the young tendon are plates so arranged as to bend round the fibrillar bundles, and that they consist of granular protoplasm enclosing a nucleus. The cell-plates are of nearly uniform size, and by their union form a connected whole, which may be aptly likened to a cloth composed of a number of stripes; and the fibril bundles are so enveloped in this cloth that each is surrounded to the extent of half its circumference by one stripe. As we understand it, the continuous expansion pervading the tissue formed by the cells resembles a sheet of corrugated iron. If two such sheets were superimposed on one another longitudinal spaces would remain, and these in the tendon are occupied by the tendinous fasciculi.

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HISTOLOGICAL PROCESSES IN DIVIDED NERVES.

UNDER the title of "Histological Processes in Divided Nerves," Dr. Benecke, of Königsberg, contributes an important paper to the last part of Virchow's *Archiv.* His investigations, he says, have extended over many years; and he has experimented upon cats, rabbits, crows, fowls, pigeons, and various small birds. Of all these he finds young cats the best adapted for research, as the nerve trunks in those animals contain very little connecnective tissue in their interior. The mode of operating consisted either in dividing the nerve with the knife, or in applying a fine silk ligature for a few moments, which is almost equally efficacious in cutting through the axis cylinders. The changes induced were examined at various intervals of time, ranging from two hours to six months, and both in the fresh state and after maceration in different preserving fluids. The first changes observed, and these take place within a few hours after section, are that the two extremities of the nerve adjoining the cut become of a greyish or yellowish-red color, cloudy aspect, and more or less strongly swollen. These changes are due to congestion of the severed bloodvessels, and to the imbibition of the serous transudate. In a short time the part of the centric extremity lying near the cut and the whole peripheric part assumes a cloudy-yellow tint, becomes somewhat attenuated, and tears easily. The sheath of Schwann undergoes, at first at least, very little alteration; the medullary sheath, on the other hand, coagulates, and breaks down into a finely granular detritus. The axis cylinder early appears enlarged and swollen, but soon attenuates, and ultimately disappears entirely. In the later stages of the process of disintegration it is impossible by any means to bring it into view. Benecke has never been able to find that the degenerative process progresses in an orderly manner in either direction from the lesion; but believes that it takes place *coincidently* throughout the whole length of the peripheral extremity, and in the immediately adjoining portion of the centric end.

But even whilst disintegration is advancing, the process of regeneration has commenced, in the form of multiplication by fission of the nuclei of the neurilemma, or sheath of Schwann. These soon become converted into elongated fusiform bodies, and after a time form the only contents of the empty and collapsed sheaths, which then, he thinks, disappear. The protoplasmic processes of the new elongated nuclei now undergo fusion, and unite to form pale slender bands coincidently in the cicatrix and in the peripheral end of the divided nerve, which is thus brought into organic connexion with the central stump. In the further progress of the regenerative process a medullary sheath is gradually formed around the primitive bands or cylinder axes originating from the nuclei. These last no longer retain their ordinary appearance, but become homogeneous, a few only remaining in each fibre, from which the normal sheaths of Schwann are afterwards developed.

Upon the whole, therefore, it would appear that after section the proper nervous tissue of the entire peripherical portion of the nerve undergoes disintegration and disappears, whilst regeneration is effected by a process precisely analogous to that which many observers have demonstrated to occur in the natural development of the nerves in the tail of the tadpole and in the embryo of the fowl; the cylinder axis being first formed by the coalescence of nuclei, to which the white or medullary substance and the sheath of Schwann are subsequent additions.

Medical Annotations.

"Ne quid nimis."

CHANGE OF WEIGHT UNDERGONE BY THE HUMAN BODY IN BATHS.

MM. Jamin and De Laurés give, in *Comptes Rendus*, an account of recent experiments on this subject as follows:—The question has not yet been clearly settled as to whether mineral waters can be absorbed by the human body. Some affirm that they can; others deny it. Profiting by a recent stay at the waters of Néris, they repeated the experiments that were made by Sanctorius. To comprehend the significance of these, it may be well to call to mind that a man of good constitution receives into his system about 4000 grammes of food daily; that he expels 1500 grammes as excretions; and that he assimilates 2500 grammes of matter, which disappears in twenty-four hours, by the lungs and by the skin. This last is an hourly loss of about 100 grammes. In reality the loss is not uniform; it reaches 125 grammes after dinner, and diminishes gradually throughout the night until breakfast on the following day. It was found to be about 80 grammes between 6 and 7 o'clock in the morning. After breakfast it again increases, and is greater when one is in exercise than when in repose. It reaches 340 grammes when one is walking in bright sunshine.

According to Lavoisier and Seguin, there is a loss by respiration of 30 grammes, while that by cutaneous evaporation is 60. These figures have frequently been verified when the body is immersed in water. The loss by the lungs will still continue about 30 grammes; that by the skin will be quite altered, and it may be measured by deducting from the total loss observed the 30 grammes evaporated from the lungs. If, for example, the weight of the body continues the same, it may be inferred that the skin compensates the loss from respiration, and absorbs 30 grammes of the liquid. If the weight decreases 30 grammes in the hour, the absorption by the skin may be regarded as *nil*; if there is an increase in the body's weight, the gain through the skin must be greater than the pulmonary loss. Seguin found that the body lost in water a little less than in air. Berthold, experimenting at temperatures between 24° and 28° C., perceived an increase in the weight of the body, indicating a cutaneous absorption of 62 grammes. These results were confirmed by Malden and others. M. Wilmin has experimented at temperatures varying from 30° to 34°. Out of 53 cases, he observed 20 in which there was an increase in weight, 21 in which there was loss, and 12 in which the weight was unaltered. But as the loss of weight (where it occurred) was always less than the pulmonary loss of 30 grammes, M. Wilmin inferred that in every case the skin absorbed liquid.

These experiments are quite accurate; and while their results differ somewhat, the variation in temperature must be taken into account. M. Durrien, recognising this important element, states the true law of the phenomenon. Each individual retains his weight unaltered in a bath which is at a certain moderate temperature (termed by M. Durrien "isothermal"). He gains weight if the temperature is lowered; he loses if it is raised, and this loss increases very rapidly when the temperature of the water is raised from 36 to 48 degrees.

The experiments at Néris confirm those of Durrien. They were undergone by a large number of individuals, including MM. Jamin and De Laurés. In commencing the loss of weight in air between 6 and 7 o'clock in the morning was observed. It was, on an average, 79 grammes. The subject then went into a bath, the temperature of which was 34·5°, and remained in it till 9 o'clock. A considerable loss was then observed—700 to 800 grammes. The body was again weighed, one hour after coming out of the bath, for the sake of comparison. The following table gives the results:—

	Loss per Hour.		
	Before the bath. Grms.	During the bath. Grms.	After the bath. Grms.
Aug. 25th	75	300	0
27th	80	180	40
28th	78	275	25
30th	82	358	32
31st	80	286	10
Sept. 6th	79	250	0
7th	83	220	0
11th	78	340	24
14th	75	230	50
Average	79	268	20

These results, besides confirming M. Durrien's observations, present one important feature which has not hitherto been observed. Before the bath, a person loses in the air eighty grammes hourly; in the hour which elapses after he has come out of the bath, the conditions are quite altered: the same person loses very much less weight, often none at all; there has even been detected in some cases a slight increase. This fact is well attested.

Now, seeing the quantity of matter emitted after the bath cannot be less than before it—that it ought rather to be greater, owing to the more humid state of the epidermis—the diminution in loss of weight after the bath can only be attributed to a diminution in the quantity of carbonic acid expired.

In ordinary conditions, the human body is impregnated, or rather saturated, with a normal supply of carbonic acid, and there is equilibrium between the quantity lost and the quantity reproduced in a given time. Immersion in water changes this equilibrium. It is probable that the bath dissolves a quantity of carbonic acid greater than that which was emitted in air, that the normal supply is diminished, and that hence there is a notable loss of weight. After coming out of the bath, the reverse takes place; the body reproduces its normal supply of carbonic acid, which process tends to increase the weight, but it continues to exhale vapor of water, and this tends to diminish the weight. The loss or the gain observed is the difference between these two contrary effects.

This explanation of an important physiological fact MM. Jamin and De Laurès propose to test further by analytical experiment.

THE PEPTONES OF FIBRIN.

J. Möhlenfeld gives an elaborate account of these peptones in Parts 8 and 9 of "Pflüger's Archiv" as the result of a long series of investigations undertaken by him in Hoppe-Seyler's laboratory. He used artificial gastric juice and the fibrin of the blood of the ox. The latter was divided into portions weighing from twenty to twenty-five grammes each, which were placed in flasks, and covered with from 350 to 400 c. c. of artificial gastric juice. The whole was maintained at a temperature of 37° to 40° C. for thirty or forty hours. Fresh portions of the juice were sometimes added, till complete, or almost complete, solution of the fibrin was affected. The product of digestion thus obtained was filtered, and then appeared as a yellowish fluid, transparent in thin layers, but slightly opalescent in thick, with an acid reaction and peculiar odor. It gave the following reactions: 1. On boiling, it became cloudy, and this turbidity was not removed by nitric acid. 2. A similar cloudiness resulted from boiling it with a concentrated solution of sulphate of soda, whether with or without previous addition of acetic acid. 3. The addition of acetic acid and of ferro-cyanide of potassium gave a slight cloud. 4. Carbonate of ammonia gave a slight precipitate. 5. Addition of solution of soda till the fluid was strongly alkaline, and subsequent addition of a very small quantity of sulphate of copper, gave a purple color. 6. The xanthoproteic reaction was very distinct. 7. Corrosive sublimate produced a cloud. 8. The same was caused by the addition of absolute alcohol. 9. Millon's reagent gave a distinct flocculent whitish precipitate; on boiling, the fluid became dark-red, the precipitate rolled together, and soon formed a greyish-red layer at the bottom of the glass, on which was a clear red fluid. 10. Tannic acid caused a strong precipitate. (If the action of the digestive fluid was allowed to proceed for many hours under certain conditions, as of temperature, some of these reactions were modified, or did not occur.) The peptone solution thus obtained was neutralised by baryta water, which, when carefully added, rendered the fluid opaque. It was then boiled to remove the body, which, as shown in reaction 1, still precipitated on boiling. When set aside for twenty hours an extremely small precipitate collected at the bottom of the vessel. The clear supernatant fluid, Möhlenfeld believed, contained only peptone, Meissner's parapeptone having been thrown down by heat, and Meissner's metapeptone not being present on account of the presence of chloride of barium, and the long duration of the digestion. The fluid was evaporated to a syrupy consistence, and precipitated with absolute alcohol; the precipitate dissolved in water, and a little dilute sulphuric acid was added to remove the baryta. Freshly precipitated oxide of silver was then added to remove the chlorine. The fluid was now alkaline, and gave a cheesy precipitate of yellowish color with absolute alcohol. Sulphuretted hydrogen was passed through the fluid from which the cheesy precipitate had taken place. A second precipitate now occurred, and hydrogen was transmitted through the clear filtrate. The alcohol was distilled off from the latter, and it was reduced by evaporation to a syrupy consistence. The addition of absolute alcohol caused an immediate and abundant precipitate, and this was washed with ether. Möhlenfeld then gives the reactions and the results of his analyses of the several precipitates. The last, he believes, has the formulo, $C_{143}H_{301}N_{40}S_1O_{67}$ thus differing considerably from fibrin. It turned the ray of polarised light to the left, and presented some peculiar reactions with Millon's fluid. The analysis of the cheesy precipitate mentioned above gave the formula $C_{41}H_{86}N_{14}O_{20} + S$. A substance closely resembling leucin was also found to be present. These experiments show that the products of the artificial digestion of fibrin, which were formerly thought to be only slight modifications of albumen or fibrin, are in reality very distinct, and possess a wholly different chemical constitution.

ELIMINATION OF ALCOHOL FROM THE BODY.

NOTWITHSTANDING the careful researches of Parkes, Dupré, Anstie, and Thudichum on this subject, it does not appear to be quite exhausted. Dr. Subbotin gives the details of a considerable series of experiments he has recently performed on rabbits in the *Zeitschrift fur Biologie* (Band vii., Heft 4). The mode of detection of the alcohol he employed was its acetification by chromic acid, or rather by bichromate of potash and sulphuric acid, the quantity being determined by the subsequent estimation of the distilled acetic acid by means of soda solution. The respiration experiments were conducted in an apparatus lent to him by Voit, and constructed on the plan of the large apparatus of Pettenkofer and Voit. Alcohol of the strength of 29 per cent. was injected into the stomach through the exposed œsophagus, and this tube at once ligatured. The results at which he arrived were: That, during the first five hours after the introduction of alcohol into the stomach, a considerable amount escapes by the skin, lungs, and kidneys; that at least twice as much escapes by the lungs and skin as by the kidneys; that the amounts he obtained, showing that from 6·79 to 7·4 per cent. were thus eliminated, were, from various considerations, certainly below the quantities really discharged from the system. These conclusions he arrived at in 1870. Quite recently, however, he instituted another series of experiments, the object of which was mainly to determine for how long a time after ingestion alcohol continued to be excreted by the skin and lungs. In one of these experiments he found that 12·6 per cent. of the alcohol was eliminated in eleven hours and a half through all these channels; in another instance 16 per cent. was eliminated in twenty-four hours, either in the unaltered condition or only changed into aldehyde. Subbotin maintains that alcohol cannot be regarded as in any sense a food, since, under its influence, the metamorphosis of tissue diminishes, the temperature falls, the amount of carbonic acid excreted lessens, and a smaller quantity of urea is discharged. Its action, he thinks, is direct upon the nervous system, affecting a change in the processes of nutrition, and it may also act directly on the blood-corpuscles. He maintains that there is a distinction (too subtle for translation into English) between Nahrungsstoffe, Nahrungsmittel, and Nahrung. "Alcohol," he continues, "which is neither a Nahrungsstoffe nor a Nahrungsmittel, in the ordinary sense of these words, yet plays an important part in the economy of the body, since it is a typical representative of that group of substances characterised by the general name of excitants (Reiz- or Geniess-mitel)." Prof. Voit adds a note to the paper, in which he does not appear to agree with Subbotin's views of the *rôle* of alcohol in the system, or quite to understand his distinctions. He calls albumen, fat, water, and salts, Nahrungsstoffe, because they form constituents of the body; but he also includes starch under this term because it can, by its oxidation, preserve the tissues. He does not define or refer to the term Nahrungsmittel, but he agrees with Subbotin in regarding alcohol as a Reiz- or Genicas-mittel (excitant).

THE AMOUNT OF CAFFEIN IN COFFEE AND ITS ACTION ON THE BODY.

DR. AUBERT, in an essay contained in the last part (xii.) of Pflüger's *Archiv*, states that though it is well known that coffee-berries and tea-leaves contain the same very poisonous alkaloid, caffein or thein, no researches have been undertaken to show how much caffein is contained in an ordinary cup of coffee. It is also an open question whether the coffee-berries should be lightly or strongly roasted, though it appears that experience has taught mankind the mode of preparation by which the largest quantity of the alkaloid is best extracted. Dr. Aubert has made a series of researches on these points in conjunction with Dr. Haase. Dr. Aubert's mode of determining the amount of caffein present in any given specimen is based upon its great solubility in chloroform, and especially in hot chloroform, whilst most of the other constituents of the berry are insoluble in it. His results show the presence of a larger amount of caffein than that given by most other experimenters, with the exception of Graham, Stenhouse, and Campbell, with whose analyses his own nearly coincide. The quantity he and they obtained is about 8 or 9 per cent. Aubert thinks that a cup of coffee prepared from about one ounce of coffee contains from four to five grains of caffein. His experiments on the effects of various degrees of roasting show that there is little caffein lost even when the coffee-berries are roasted black; in this respect he is in antagonism with Liebig. He obtains from the best Pekoe tea from 2·149 to 2·423 per cent. of caffein, the greater portion of which is extracted by the simple process of infusion.

As regards the physiological action of caffein he coincides with previous experimenters in considering that it causes increased reflex excitability and tetanus, the action resembling that of strychnia. If, however, one sciatic nerve be divided before the poisoning, that limb is not convulsed; hence it acts on the nerve centres. A frog is tetanised by the subcutaneous injection of a quantity not exceeding 0·005 of a gramme. 0·120 of a gramme injected into the jugular of a rabbit tetanises it, and a cat or dog is tetanised by 0·2 of a gramme. It is remarkable that by maintaining artificial respiration for some time the symptoms of tetanus entirely disappear. Its action on the heart of mammals is that it causes acceleration of the beats, with diminution of the blood-pressure; this last effect he attributes to the poison paralysing the ganglionic nerves of the heart.

ANALYSIS OF AIR.

DR. ANGUS SMITH gives the following very simple method of testing the amount of carbonic acid in the atmosphere, and approximatively the amount of impurity or deterioration. Take a clean wide-mouthed stopper bottle capable of holding seven ounces. Draw in air by means of a glass tube, taking care not to breathe into the bottle. Put half an ounce of clear baryta water (·08 of a gramme of baryta), close the bottle and shake. If the air should contain less than ·03 per cent. of carbonic acid, there will be no precipitate. If the bottle holds 5·42 ounces, there will be no precipitate unless the carbonic acid exceed 0·04 per cent.

As no dwelling-room should contain more than 0·08 per cent. of carbonic acid, its purity may be tested in a similar way by using a bottle containing 8 ounces. If there should be a perceptible deposit, more ventilation is required. Lime water may be used instead of baryta water, but in that case the bottle must be larger, owing to solubility of carbonate of lime, and the consequent difficulty in recognising the beginning of opacity. In using lime water it is necessary to draw out the required half-ounce with a pipette. By the following table it is within the power of anyone to test the purity of the atmosphere:—

Air at 0° *centigrade and* 760 *mm. barometer.*

Carbonic acid in the air per cent.	Size of bottle to be used with half an ounce of lime water: no precipitate produced.	
·03	20·63	oz. avoirdupois
·04	15·60	,,
·05	12·58	,,
·06	10·57	,,
·07	9·13	,,
·08	8·05	,,
·09	7·21	,,
·10	6·54	,,
·15	4·53	,,
·20	3·52	,,
·25	2·92	,,
·30	2·51	,,
·50	1·71	,,
1·00	1·10	,,

Ex. 1: No precipitation takes place in a 20·63 oz. bottle containing half an ounce of clean lime-water. This proves that the carbonic acid does not exceed 0·03 per cent., and shows that the air is in a state of great purity.

Ex. 2: By using a bottle of the capacity of 8 oz., the absence of precipitate shows that the carbonic acid is less than 0·08 per cent.

THE MUSCLES OF THE KIDNEY.

Prof. Eberth, in a recent communication to the *Centralblatt*, observes that Remak is the only author who has described the presence of smooth muscular fibre cells in the renal capsule of the ox, sheep, and the amphisbæma. He himself some time ago showed that such fibre cells were not very widely distributed, since they are absent in man, the cat, pigeon, and tortoise. Recent writers, as Arnold in Stricker's "Manual of Histology," make no mention of the presence of smooth muscular fibre cells, either in the capsule or in the stroma of the kidney. Henle and Ludwig both regard the stroma and capsule as essentially composed of fibrillar, connective and elastic tissue; the former referring such muscular fibre cells as come into view on breaking up the tissues to the vessels. Eberth states that he can demonstrate the presence of a wide-meshed plexus of smooth muscular fibres, the diameter of the fasciculi of which is about equal to that of the larger superficial veins. These fasciculi are in no way connected with the musculature of the vessels, though they often run in close contiguity to the vessels. Small processes of these muscular bundles may be traced into the cortex of the kidney, but they always remain quite superficial. Eberth has been unable to discover any fasciculi in the stroma of the kidneys. The fasciculi are most easily demonstrated in kidneys that have been hardened in alcohol, and from which superficial sections have then been made. The individual cells are fusiform, of various length, with dull homogeneous contents, and readily isolable with acetic acid and solution of potash. This muscular plexus is absent in ox, sheep, and pig. Its existence affords a ready explanation of many myo-sarcomata of the kidneys.

ANALOGY OF THE EFFECTS OF BITES OF SNAKES AND INGESTION OF POISONOUS FISH.

In Brown-Séquard's *Archives de Physiologie* (No. 3) Dr. Corre, of Brest, institutes a comparison between the symptoms produced by the inoculation of the poison of venomous snakes and those resulting from the ingestion into the stomach of poisonous fishes. The fishes were the tetrodon and the melette of the South Pacific Ocean, the results of the use of which as food have been recorded by Drs. de Rochas and Lacroix. The snakes were the trigonocephalus described by Drs. Guyon, Rufz, and Encognère, and some others observed by himself in Martinique. The effects of the bites of snakes seem to be rendered apparent in the first instance by their action on the organs of digestion, indicated by intense thirst, pain at the epigastrium, nausea, vomiting, and usually constipation. The secretion of urine is arrested; cold sweats appear upon the skin. There is dyspnœa, accompanied sometimes with sero-sanguinolent expectoration. The action of the heart fails; the nervous system is strongly affected, the principal symptoms referable to it being agitation, stupor, coma, pain, cramps, convulsions, and paralysis. Post-mortem examination shows the gastro-intestinal mucous membrane injected with capillary ecchymosis, the lungs gorged with blood, the heart soft, and the blood decomposed. Now, symptoms almost identical with these have been observed where poisonous fishes have been consumed. It would, therefore, appear that the poisonous substances present a remarkable analogy in their nature and action. It is curious, however, to observe that they enter the system in two quite different methods. In the case of the snake poison, it must be introduced directly into the blood, and is without action when taken into the stomach; whilst the opposite obtains in the case of the poison of poisonous fishes.

THE CHEMISTRY OF TETANUS.

M. **Danilewsky** forwards a provisional communication on this subject to the *Centralblatt*, and states that he has arrived at the following conclusions, some of which are already known, whilst others are orginal:—1. When the circulation of the blood is maintained, the amount of water in active muscle augments, the heart becoming the most watery of all. 2. The quantity of albuminoid substances diminishes in tetanus, though to an inconsiderable extent. The quantity of albuminoids in the heart is much less than in the muscles of the extremities. 3. The alcoholic extract of tetanised muscle contains more nitrogen than that of muscle at rest. The heart contains a very large proportion of nitrogen in the alcoholic extract. M. Danilewsky thinks this affords strong evidence of increased disintegration of complex nitrogenous compounds in tetanus. 4. His most recent researches show that in the decomposition of albuminous compounds by means of KOH, compounds containing sulphur, and soluble in warm alcohol, are produced, and hence on *a priori* grounds their presence might be anticipated in the alcoholic extract of warm muscle; and this is fully borne out by analysis. The amount of such compounds cannot, however, as yet be quantitatively determined. The heart, again, in this point of view occupies the first rank, and this furnishes additional evidence of the disintegration of albuminous compounds during muscular activity. 5. Alcoholic extract of muscle contains a phosphorised body (lecithin?) in quantity too large to admit of its coming from the nerves; and it would appear that this also is increased in muscle during long-continued exertion. M. Danilewsky considers that the warm alcoholic extract contains exclusively the products of the regressive metamorphosis of muscle.

A NEW REMEDY FOR OBSTRUCTION OF THE BOWELS.

We read in the *Western Lancet*, a monthly medical journal published at San Francisco, a somewhat curious history. A man confined in the State prison at San Rafael was attacked by obstruction of the bowels, followed in due course by stercoraceous vomiting, and was left by Dr. Taliaferro, the surgeon, in a state considered hopeless. After the doctor's departure, however, William Lebur, the steward of the prison hospital, took the case in hand, and devised a plan of treatment not only original and ingenious, but which had the additional merit of being successful. He first injected into the rectum a solution of carbonate of soda, then a solution of tartaric acid, and made firm pressures over the anus with a sponge, so as to retain the gas liberated during the effervescence. His injection was repeated three times, and he used four ounces of carbonate of soda, two drachms of tartaric acid, and three pints of water, for the three injections. The distress occasioned by the rapid distension of the large intestine seems to have been considerable, but the obstruction yielded, and copious evacuations were obtained. The patient seemed in a fair way to recover, when inflammation of one of the parotid glands set in, followed by extensive suppuration, and he died exhausted a fortnight after the injection. Dr. Taliaferro, who reports the case, seems to think that Lebur's injection, "in less explosive doses," might often do good service in such cases. He has found in "Wood's Practice of Medicine" mention of a case successfully treated in the same manner.

NORMAL PRESENCE OF ALCOHOL IN THE BLOOD.

In a paper published in the *New York Medical Journal* for 1872, Dr. Hutson Ford, of New Orleans, refers to the difficulty experienced even by Claude Bernard of determining the destination of the hepatic glucose formed in the liver. Claude Bernard found that in blood placed under, or otherwise submitted to, the action of oxygen, carbonic acid, nitrogen, hydrogen, arseniuretted hydrogen, or air, sugar sooner or later, but *invariably*, disappeared. With regard, however, to the mode of destruction of the sugar in these cases, or the nature of the product of its transformation, he suggested no explanation. Dr. Ford conceived that it might be due to a fermentative process, and that this would most likely be the lactic and alcoholic. From various considerations he was led to believe it was the latter, and by calculation estimated there should be rather more than 1-20000th of its weight of alcohol in the blood. He gives very full details of his method of inquiry, and finds the results of his analyses agree fairly with the proportion obtained by theory. Thus, in 20,000 parts of fresh lung tissue he obtained ·6 parts of alcohol, but from the putrescent tissue, in which fermentation of the glucose originally present had proceeded, he obtained ·76 parts. So from fresh thoracic blood he obtained ·16 parts, but from putrescent blood 1·52 parts in 20,000. The destination of the alcohol he believes he has demonstrated must be to a hæmal oxidation or combustion, and must form an important source of animal heat.

DEATH FROM THE STING OF A BEE.

On the 19th July, Miss Elizabeth Hough, of Twickenham, aged fifty-five, was stung by a bee behind the ear. Shortly afterwards she became unconscious, and died before the doctor who was sent for arrived. The brother of the deceased stated at the inquest that she was of a delicate and nervous constitution. In the autumn of 1870 she was stung by a bee, and the consequences were very peculiar. She became unconscious, and remained in that state for two hours. It is well known that many people endure far greater sufferings than others when stung by bees or other insects, and cases in which serious consequences have ensued are recorded. Beck, in his work on Medical Jurisprudence, quotes a case where a sting on the back of the finger was quickly followed by "vomiting, sweating, trembling, and great difficulty of breathing."

Dr. Tanner quotes a case from the *American Journal of Medical Sciences*, of a man who suffered very seriously in the month of August, 1819, from the single sting of a bee, and who being stung a second time (on the temple) in the following year, died from the effects thereof within ten minutes. Two or three other cases of fatal result are quoted by the same author, but happily such an occurrence is exceedingly rare. The advice given by the coroner, Dr. Diplock, to "give stimulants freely" in such cases, is undoubtedly founded on sound principles, the immediate cause of death being apparently failure of the heart's action.

TREATMENT OF SNAKE-BITE.

Mr. T. H. Webb, resident surgeon of the Creswick Hospital, writes to the *Australian Medical Journal* touching the treatment of snake-bite by the injection of ammonia into the veins, and he makes the following startling remarks on the careless use of the hypodermic syringe:—"The enormous ulcers this caustic creates, the way it burrows, destroying the cellular tissue wherever it touches it, is something incredible. Twice in as many years have I had to ligature the brachial, above and below, for sloughing into the artery, and in both cases have the patients nearly lost their arms from gangrene, and their lives from hæmorrhage." He gives the case of a man who had been "hypodermically injected with ammonia; as might naturally be expected, wherever the caustic has entered, an ulcer remains. And such ulcers! One could almost pursue a course of muscular anatomy on his feet, legs, and thighs, whilst at the bend of the elbow the deepness and extent of the slough has compelled him to constrict his brachial. He may well say, 'The evils that it hath pleased Heaven to affect me with I could have borne, but the evils *plus* their remedies—impossible.'" "I declare most emphatically," continues Mr. Webb, "that instead of the venous injection being a little operation, which anyone can on an emergency perform, it is one that none but a surgeon should attempt, and yet I have several times seen hypodermic syringes in the possession of laymen, kept for the purpose, though how they were going to use them Heaven only knows."

THE EMPLOYMENT OF COLORED GLASS IN MICROSCOPY.

M. Mouchet suggests, in the *Monthly Microscopical Journal* for September, the use of colored glass in the construction of microscopes, in order that sunlight may be employed and thus replace all the other systems of illuminations which are intended to supply the brightness that powerful objectives tend to destroy. He counts four positions that the colored glass may occupy: before the mirror, under the object, between the objective and the eyepiece, and finally, above the eyepiece itself. To place them before the mirror necessitates the employment of glasses of a certain size; to place them under the object increases the thickness of the plate, and prevents a very oblique light from passing through; whilst to fix them in the body of the instrument causes trouble and a loss of time, for the microscope must be taken asunder. Hence he concludes that the most suitable place is above the eyepiece, and with the object of placing it here he had constructed an apparatus consisting of a kind of revolving diaphragm, pierced by nine openings, each one centimetre in diameter, fitted with glasses colored to correspond with the rays of the solar spectrum, and placed exactly opposite the eyepiece. This arrangement he thinks will prove very useful in permitting direct solar light to be used.

WORMS IN URINE AND BLOOD.

It will be remembered that, during his investigations on cholera in India, Dr. T. Lewis discovered the embryo of a round worm in chylous urine, which is figured in his Report on Cholera. Dr. Lewis has since continued this inquiry in Calcutta, and has not only detected these embryos in large numbers in chylous urine in many cases, but has found them in very large numbers in the blood of patients suffering from chyluria. A single drop of blood from the finger has contained as many as nine live worms. And the identity of these blood-worms with those of the urine is, we understand, beyond doubt. Dr. Lewis's observations will be published in the next Report of the Sanitary Officer with the Government of India, and will be looked forward to with great interest.

A specimen of the embryo of the chylous-urine worm has been submitted to Mr. Busk, who considers it to be some kind of Filaria; and it may not be inappropriate to christen the new entozoon "Filaria sanguinis hominis." It is remarkably like some of the common tank-worms; and the mode of entrance into the body (probably with the water), its growth and increase there, and the effects it produces, are matters of the highest interest, which we hope Dr. Lewis may soon clear up.

STRYCHNINE AND CHLORAL.

M. Oré presented to the last meeting of the Academy of Sciences a new work on the question of the antagonism between strychnine and chloral. He finds—1. That not only does the intravenous injection of 1 milligramme of strychnine (a fatal dose for a rabbit of 4 lb. weight) fail to prevent the animal from succumbing to the influence of 4 grammes of chloral, but the presence of the alkaloid does not manifest itself by any characteristic phenomena. 2. The injection of 2·5 milligrammes of strychnia produces well-marked convulsions. In one case it appeared to retard the death of the animal, without, however, preventing it; for it died with all the symptoms of chloral poisoning. 3. In proportion as the dose of strychnine augments (3·45 milligrammes), death occurs with greater rapidity. 4. If the dose be raised to 7·5 milligrammes, the action of chloral is entirely obliterated, and the animal dies by strychnia poisoning. 5. If we try to combat the effects of chloral given in a fatal dose by the subcutaneous injection of strychnia, the animal always dies—most frequently from the action of the first substance, though sometimes from the second. Hence he concludes that strychnia is not an antidote to chloral.

REACTION OF LEUCÆMIC BLOOD.

In one of the last parts (Heft 1, Band viii.) of the *Zeitschrift fur Biologie*, Dr. Mosler states he has had an exquisite case of splenic leucæmia under his care, and that on examining the reaction of the blood he found that it did *not* present, as has sometimes been stated, an acid reaction. On the addition of a little tincture of litmus, and exposure to a warm room, the color turned red after four days. The defibrinated blood placed in a graduated tube with twenty-four divisions, and allowed to remain at rest for twenty-four hours, gave the following results:—1. The upper layer of clear serum occupied five divisions; beneath this was a layer of colorless blood-corpuscles occupying seven divisions, and the remaining twelve were occupied by red blood-corpuscles. The proportion of the white to the red corpuscles was thus very nearly as 1:2. The patient was forty-four years old, from Uckermünde, who had had ague twenty years, and again eight years, previously. A year after presenting himself he had pain and a swelling in the left side. He had well-marked retinitis leucæmica. He was treated, apparently successfully, with cold douche baths directed to the region of the spleen and with large doses of quinine.

PHYSIOLOGY OF THE BILE.

Some years ago Nasse demonstrated that the bile of the pig was capable of converting starch into sugar. He was unable to obtain evidence of the presence of any similar ferment in other kinds of bile. Von Wittich showed how the ferment could be isolated. Jacobson observed the saccharifying power of the fresh bile of the frog, pike, carp, sheep, ox, pig, rabbit, cat, horse, goose, duck, and fowl, but was unable to exhibit it in the bile of man; and this he attributed to the decomposition of the ferment after death (Pflüger's *Archiv*, Heft 3, 1872). Owing to the kindness of Dr. Hertz, v. Wittich has recently had the opportunity of examining some fresh bile of the human subject escaping from a fistula; and his experiments prove conclusively that it possesses a ferment capable of converting boiled starch into sugar.

QUANTITY OF NITROGEN IN MUSCULAR TISSUE.

Nowak (*Wien. Akad. Ber.*, lxiv., [2] 359) finds that the method usually employed for the determination of nitrogen in muscular tissue (conversion into ammonia by combustion with soda-lime) does not give accurate results. The percentage of nitrogen is always too low, and varies by as much as 0·8 per cent. in the same sample. The method of Dumas (combustion with copper oxide, and measurement of the nitrogen as gas) is thoroughly trustworthy, and by employing it Nowak obtained the following percentages of nitrogen in dry flesh: —Horse, 13·48—15·01; dog, 12·40—16·43; ox, 15·00—15·92; man, 14·61—16·02. If this observation is correct it will necessitate the revision of a great many important analyses. The flesh-formers of food are often estimated by determining the percentage of nitrogen and multiplying it by 6·3. It is clear that this calculation can no longer be employed if the above figures are correct, for one sample of dog's flesh would appear to contain 103·51 per cent. of flesh-formers. Of course a part of the nitrogen in flesh is not present in the form of albuminoids.

MEASURE OF THE ACTION OF PEPSIN.

A very interesting mode of measuring and exhibiting the action of pepsin is described by Gruenhagen in *Pfluger's Archiv*, v., 369. A piece of moist blood-fibrin is soaked in water containing 0·2 per cent. of hydrochloric acid until it is converted into a mass like jelly. This jelly is placed on a funnel, and when the acid has ceased to drip a few drops of pepsin solution (glycerine extract, for instance) is added. In two minutes the fibrin begins to liquefy and to drop, as peptone, from the funnel. The value of the digestive solution can be measured approximately by the rate at which the solution takes place. Von Wittich, commenting on Gruenhagen's process, says it is better to take, as the measure, the quantity of liquid which falls from the funnel in a given time. Von Wittich adds many interesting particulars on the chemical changes which take place in the digestion of fibrin.

Correspondence.

"Audi alteram partem."

PLASTIC APPARATUS IN SURGERY

To the Editor of The Lancet.

Sir,—I can corroborate the testimony in favor of the plaster-of-Paris, or Gyps-bandage, brought forward by Dr. St. John, of New York, whose paper on the subject is reviewed by you (The Lancet, November number, 1872), by calling attention to the fact that this bandage has been in use for many years, to the exclusion of every kind of splint, in the Clinical Hospital of Herr Langenbeck, at Berlin. Perhaps it may be of interests to some of the readers of The Lancet if I describe the mode of application in use at that hospital. The Gyps-bandage consists of book muslin, having dry plaster rubbed into the mesh. This is easily done by rolling up the bandage slowly in a vessel filled with dry plaster. It is well to roll the bandage round a pencil as a core, with the object of allowing water to permeate readily to the centre. These and others can be kept ready for use if lying in dry plaster. The plaster should be the fine sort, used by modelers. When a fracture is to be put up, a flannel roller is first put on, then a thin layer of cotton wool, then the Gyps-bandages, previously dipped in water for about a minute. Some plaster is mixed in a bowl, and as a layer of bandage is finished, this wet plaster, about the consistence of cream, is smoothed on with the hand. About three layers of Gyps-bandage are ordinarily used. The bandage possesses three merits. A soft elastic cushion is next to the limb, so that if slight swelling happens there is room for expansion to prevent pain, while the equal pressure supports the vessels. The hard case prevents motion, and is without difficulty fitted accurately to the limb. The bandage can be rendered impervious to water by pouring over it a solution of resin in sulphuric ether, one part to four. When in Berlin some years ago I saw this bandage applied to a fracture of clavicle, of scapula, of extremities, and after excision of joints. I am, Sir, yours, &c.,

H. Hy. Smith, M.B.

Haverstock-hill, August, 1872.

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PUERPERAL CONVULSIONS TREATED BY CHLOROFORM.

To the Editor of The Lancet.

Sir,—Some months ago Dr. Wiltshire recorded a case of puerperal convulsions successfully treated by chloroform; and I think the following case, in which such treatment was inapplicable, on account of the frequency of the convulsions and the length of the attack, may be interesting to your readers:—

S. M., aged twenty years, unmarried, in the sixth month of pregnancy, after suffering for a day or two from great depression of spirits, was suddenly seized with violent stabbing pain in the right breast. I saw her at 1·30 a.m., Sept. 12th, 1870. Patient flushed and feverish; breast swollen, hard, and painful; ordered poultices to breast, and gave febrifuge with opium—1 P.M.: Much better; fever reduced considerably; breast still painful, though much relieved; suffering from griping pains in bowels. On examination, os uteri quite closed and firm.—5 P.M.: Sent for; found patient in convulsions, recurring every twenty or thirty minutes; the whole body violently convulsed. Chloroform relieved the spasms, but did not increase the interval, nor in any way affect the recurrence of the fits; the os uteri still closed. At 1 A.M., on consultation by note with my partner, Dr. Webb, I decided to induce labour. Gradually dilating the os by passing the finger within it, I punctured the membranes with a stylet, and, action of the uterus commencing, a dead fœtus was born in about seven hours. The convulsions entirely ceased after the liquor amnii was evacuated. Patient was insensible for several hours, but made a rapid recovery, only retarded by the abscess forming in the breast. I am, Sir, yours obediently,

Wm. Milligan.

Wirksworth, Aug. 13th, 1872.

THE INTERNATIONAL OPHTHALMOLOGICAL CONGRESS.

At the commencement of the first morning sitting on Thursday, August the 1st, the following papers were read:—

ETHER IN OPHTHALMIC SURGERY.

BY JOY JEFFRIES, M.D., BOSTON, UNITED STATES.

Every week gives us substantial proof of the fatality of chloroform in operative surgery. A certain degree of anxiety, therefore, must always be present in the operator's mind, from which he cannot free himself, even if an equally competent person administers the anæsthetic. If these propositions are not true, then there is no force in what I have here to say. I do not advocate the use of ether because I come from the city where its employment in surgery was discovered and promulgated, but because I believe that there are others like myself who do not desire to run the risk of killing a patient with chloroform, and who, perhaps, would gladly avail themselves of ether were they rendered as familiar with its administration and harmlessness as we are in America. Let me be clearly understood. I use and advocate ether because it is as effectual as chloroform, and not dangerous to life. I should not hesitate to use chloroform, though perhaps not so freely as ether, did the latter not exist. As we have ether, I cannot feel justified in using chloroform, and never have done so. I should not unless forced to; therefore it is that I desire to induce others to familiarise themselves with the use of ether. I believe that those who now use chloroform exclusively, and who can never do it without danger to the patient, would employ ether did they understand how to do so. I would unhesitatingly say that I think this is largely the reason why ether is not made use of; and I opine there is some indefinable dread of it, caused by the fatality of chloroform, simply because ether is an anæsthetic. Now I desire to here say at once that I believe it is difficult to kill any one with ether, and that death never occurs accidentally while it is being inhaled. The accumulated evidence on this point is sufficient for me at least, as the accumulated evidence of the fatality of chloroform is sufficient to deter me from ever using it unless forced by necessity. That ether is more difficult to take and to administer I recognise and appreciate as well as any one. I do not wish to be harsh with my medical brethren either at home or abroad, but I cannot but think that the disagreeabilities of ether induce some to run the risk of the use of chloroform, which may be fatal—when, who can foretell? I do not propose to enter into any discussion as to the action of ether, or the special methods of using it. I would, however, here say, that many of the disagreeabilities of ether—nausea, vomiting, and headache—may be avoided or mitigated by the patients taking no, or but a light, supper the evening previous to the operation, and absolutely *no food whatever* the morning of the operation, which should, if possible, be done not later that 9 or 10 A.M. Ether, at the worst, is but a profound intoxication, and not unlike a drunken fit. On the other hand, thousands inhale it without trouble, as proved by their anxiety to take it a second time if necessary. A towel rolled into a cone, with a napkin or sponge pushed to the top of the inside, is all we need to pour our ether on, whilst our fingers can mould it over any mouth and nose. Some years ago I often heard in Europe medical gentlemen say, "But there are so many people who cannot take ether." I have yet to see one. The truth is, I believe, that surgeons who use chloroform are afraid of ether, and do not dare to give enough of it at once in the commencement. Now if the patient is warned that the ether will choke him, and told when this occurs to take long breaths to relieve it, and not to struggle and endeavor to push away the sponge, many will go to sleep quietly and without trouble to themselves or the surgeon. I have but one other point to speak of in reference to giving ether. When the patient, whether old or young, struggles, and asks for a respite and fresh air, do not yield. Hold them down by main force if necessary, and at any rate keep the sponge tight over the mouth and nose till they finally take long breaths and then soon go off into ether sleep. Doing this prevents their remembering anything about their struggles. It is absurd to stop the ether and try to reason with adults excited by the anæsthetic, and cruel not to push on quickly with children. This may sound almost puerile to my American brethren, but my personal experience tells me that those who use chloroform have somehow a sort of dread of ether, as if it was to be suddenly fatal, and hence fail to give a patient enough to intoxicate him quickly. This arises from lack of familiarity with its use and administration.

In ophthalmic surgery there are several special reasons for the employment of ether. In the first place, death during any other surgical operation might be allowed to pass without creating undue or severe comment, but no community would let pass without it a death occurring from any anæsthetic during the extraction of cataract, an iridectomy, or the removal of the globe. A possible fatal result is not included in ophthalmic surgery. In my own community I should not care to have a patient die from chloroform under my hands, and be myself tried for manslaughter afterwards. The prosecuting attorney could put scores of surgeons on the stand, whose evidence to the jury would be unanimous that I might have employed ether, which is not fatal, and hence the responsibility of the fatality of chloroform rested entirely upon me. It would be an ugly case. Another argument for ether is that it leaves the mind of the ophthalmic surgeon entirely free from any anxiety or thought of the anæsthetic. When the operation does not require him to have an assistant, he may dispense with one to administer the ether, and give it himself, as a little care will enable him to avoid benumbling his fingers with the cold, and any bystander can hold the sponge over the patient's mouth while he sleeps. I do not mean that an assistant is not a convenience, but that with ether we may dismiss thought for the patient's pulse, &c., since blueness of the face over which we are working tells us when to take off the sponge and when to re-apply it. Moreover, operations are very frequent in ophthalmic practice, and an assistant to whom we may commit the administration of chloroform is not always at hand or readily obtained, whilst the necessity for immediate operation may from various causes be very pressing. For myself, I find there are but few ophthalmic operations which I hesitate to undertake alone under ether, when compelled so to do. I do not find that medical gentlemen feel thus about chloroform. Here let me answer by *experience* the *theory* that ether will not act favorably in many ophthalmic operations, in consequence of the nausea and vomiting liable to follow its use. In the ophthalmic hospital with which I am connected, some fifty Graefe's extractions are annually done, almost always under ether, without the subsequent nausea or vomiting, provided this takes place at all, seeming to interfere with the usual course of the recovery or the final result. Some three hundred other operations are also performed under ether. We never use chloroform. I will say nothing here of the use of ether in the general hospitals of my own city, as I desire to confine my remarks to ophthalmic surgery, in which I consider ether a blessing to both surgeon and patient. Finally, I would sum up what I mean, and would here say that ether is never fatal in surgery, it can always be used in ophthalmic practice, as it does not interfere with the operation or its results, it allows the operator to work alone if compelled to, and free from thought of the patient's condition. I admit that it is not so pleasant to give or take as chloroform. With those who can administer this latter without anxiety, and can rest at peace with their own conscience and the community in which they live after a fatal case under their hands, my remarks can of course have no force, and for such they are not intended.

Mr. Brudenell Carter said: I much regret, Sir, the absence from among us of a gentleman who, more than any other in London, is in the habit of administering ether,—Mr. Warrington Hayward, the surgical registrar and chloroformist to St. George's Hospital. He has advocated very strongly the use of ether in general surgery, but his experience is, and I must say that mine entirely coincides with it, that ether as an anæsthetic agent does not produce sufficient muscular relaxation to fulfil all the requirements of the ophthalmic operator. As we have had it administered at St. George's Hospital, we have certainly found that the recti muscles have not been rendered passive in the degree that I should desire, and after some experience both Mr. Hayward and myself have determined to lay it aside, and return to our old and trusted friend chloroform, of which I must say we have no fear, and which we have never had any reason to regret using. It is with great deference that I venture to question the statement of Dr. Joy Jeffries about the safety of ether, but unless my memory plays me altogether false, there have been deaths from ether recorded in surgical history. I think that when anæsthetic agents were first introduced, and ether was the only one considered to be of any practical value, certain deaths did occur. I shall be grateful, Sir, if Dr. Jeffries will come to St. George's Hospital and administer ether for us. that we may see whether our past dissatisfaction with it may be in any way due to our faults of administration.

Dr. O'Leary observed that many differences in the effect of ether, when administered as an anæsthetic, were due to differences in the purity of the drug. He believed it to be a perfectly safe remedy, provided it be pure.

Mr. Jabez Hogg said that after much experience with various anæsthetics he preferred to operate without any anæsthetic. The resistance of the recti muscles was the best possible aid to extraction.

Dr. Jeffries.—I would say, in answer to Mr. Carter, that the Medical Society in Boston appointed a committee some years ago to investigate all the reported cases of deaths by ether. That was done, and they could not find that any one of those reported deaths was due to the anæsthetic. When the muscles are too tense for the operation I doubt if ether enough has been given.

THE SURGICAL TREATMENT OF OPTIC NEURITIS.

BY DR. DE WECKER, OF PARIS.

The author commenced by referring to the researches of Schmidt and others, by whom it has been shown that the cause of optic neuritis, or choked disc, is often an accumulation of fluid in the space between the inner and outer sheath of the optic nerve. Such an accumulation is often dependent upon a like excess of serosity in the sub-arachnoid space; and it acts mechanically upon the circulation of the disc by compressing the efferent veins of the retina in the region of the sclerotic ring. For this mechanical impediment to the flow of blood a mechanical remedy seemed to be indicated; and the author, after trials upon the dead subject, determined to attempt the incision of the nerve-sheath and sclerotic ring in the living patient. He selected a case in which the compression, presumed to be due to intra-cranial tumour, had already produced blindness, and in which there was much suffering from headache. An incision was made through the conjunctiva in the interval between the inferior and external recti, at a centimetre from the margin of the cornea. Tenon's capsule was carefully divided, and the globe luxated upwards and inwards. Dr. de Wecker could then feel the distended nerve-sheath. With a guarded knife made for the occasion he punctured the sheath a short distance behind the globe, and drew the knife forwards, dividing the sheath in its course, until he had incised the edge of the sclerotic ring. The eye was then replaced, the lids were closed, and a compress was applied. The operation was followed by no reaction, and the wound healed readily. The patient was at once greatly relieved from severe headache on the affected side, and the circulation in the disc could be seen by the ophthalmoscope to be more free. Dr. de Wecker had operated also in a second case of a similar kind, with the same result. In neither had there been any improvement of vision, nor could any such improvement have been expected; but he brought forward the cases in order to prove the practicability and harmlessness of the procedure, in the hope that, these once established, it would be employed at an earlier stage of choked disc, and with more decidedly curative results. He exhibited the instruments employed, and said that they would be placed in the hands of Messrs. Weiss.

In the discussion which followed two or three of the speakers condemned the operation on the general ground of the uselessness of interference with a case of cerebral tumour. They rather missed the point of Dr. de Wecker's paper, which was to demonstrate, *in corpore vili*, the practicability of affording mechanical relief to compression of the nerve, and perhaps also to distension of the subarachnoid space. The relief afforded to headache is a very important fact; and the experience we have of the good results of relieving distension in cases of local inflammation in other organs, gives some ground for the hope that incision of the nerve-sheath might exercise an influence for good in any case in which a choked optic disc accompanied meningitis. At all events the experiment is one that both might be and ought to be attempted.

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BORDEAUX.

(*From an occasional Correspondent.*)

SESSION OF THE FRENCH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

In the Medical section, presided over by Professor Bouillaud, Dr. Ollier, of Lyons, communicated the results of his more recent experiments on the process of growth in long bones. He supported the theory of peripheric growth, combated by German *savants*, such as Wolff, Culmann, and Meyer. M. Ollier, after having investigated the question of normal growth, turned to that of morbid growth. Already in 1867 he had shown, he said, that irritation of the diaphysis of a long bone excites the elongation of the bone, whereas irritation of the conjugate cartilage brings on an arrest of growth, and that not by the more rapid soldering of the epiphysis, but simply through disturbance of the process of ossification. Clinical facts, he said, had since confirmed these views. Another fact on which M. Ollier laid great stress was the unequal part played by the two conjugate cartilages in the development of bones. The humerus, for instance, grows especially through the upper conjugate cartilage, the ulna and cubitus through the lower cartilage, so that the parts forming the elbow are only of very limited utility in the general development of the upper limb. It is just the reverse with the knee and the bones which form it. He drew attention to the importance of these facts in connexion with the results which are to be expected from articular resections practised on children. Since 1861 he had made known that after resection of the elbow in children the upper limb continues to grow, whereas the lower limb scarcely grows after resection of the knee. The deformity which results from this retardation of development becomes more and more marked as the limb acquires its normal development. M. Ollier said that after resections, just as after certain inflammatory lesions, an increase in length of the affected limb may be sometimes observed. This elongation was not accompanied by hypertrophy of the bone, which, on the contrary, became lighter. He therefore has given the name of atrophic elongation to this sort of growth. M. Ollier explains this anomaly through the diminution of pressure which the bones exert upon each other. The decrease of this pressure induces an atrophic lengthening, as it is easy to observe on the bones of a paralysed limb, provided we do not wait for that general atrophy which so soon follows functional inertia of the limb.

Dr. Papillaud, of Saujon, read a paper "On Variola, Vaccine, and Post-vaccinal Inoculation." The author concludes, from various facts observed in practice, that vaccine, which exerts a sufficiently powerful preservative action in sporadic variola, becomes insufficient in epidemic variola. In the latter revaccination affords neither complete nor certain preservation. Dr. Papillaud advocates variolic inoculation performed after inoculation with vaccine, and calls it, therefore, post-vaccinal inoculation. He believes that it corroborates and achieves the prophylactic action of vaccine, and thus preserves completely from any attack of small-pox.

M. Alphonse Guérin, surgeon to the Hôpital St. Louis, then made a most interesting communication on Transfusion of Blood. After reviewing all that had been written of transfusion of blood, he proceeded to investigate the mechanism of the operation. It was a difficult one. Air might enter the veins, and kill at once. Venous blood had a marked tendency to coagulate, and a mere clot might stop circulation. The blood was previously defibrinated, it was true; but then might not the fibrine be necessary to the tissues? Besides, the heart and lungs, entrusted with elaborating this venous blood, were in a morbid condition. To obviate these difficulties, attempts had been made to put an artery in communication with a vein; but then the blood, being already transformed, reached the lungs quite uselessly, and so the animals died. M. Guérin's plan is to bring artery and artery together. He divides similar arteries in two animals, and by means of an india-rubber tube brings into communication the central end of the artery in the healthy animal with the distal end of the artery of the sick animal. In order to prevent the latter from receiving too much blood, and dying from plethora, M. Guérin connects by a second india-rubber tube the peripheric end of the artery of the healthy animal with the central end of the artery of the dying animal. After some time the two bloods become completely mixed, so that between the two animals there really exists a community of blood. M. Guérin mentioned that in searching among previous documents he had found no relation of experiments like his. Those of Brown-Séquard had consisted in causing the two arteries to communicate by means of a T-shaped tube, which conveyed the blood into the sick animal in a direction opposed to the current of circulation, as well as parallel to it, so that the action of the valves soon ceased, and the beatings of the heart were stopped. All M. Guérin's experiments have hitherto been conducted on animals, and have all been successful. There is no fear of any clot forming; and in respect of the introduction of air, M. Guérin thinks that the small arteries do not permit of such an accident. The tied ends of the arteries fall only after eight or fifteen days. The speaker pointed out the various modifications which the sick organism would undergo if the community of blood were allowed to subsist for that space of time. Alluding to the *modus operandi*, M. Guérin recommended operators to uncover the artery over a rather large extent, and to prevent the blood from returning to the heart by compressing with pincers the artery and its anastomoses.

Dr. Fleury (of Bordeaux) read a paper in which he investigated the dynamism of the two cerebral hemispheres. The anatomical study of the brain and clinical facts have shown the functional inequality of the two hemispheres, the left hemisphere being superior to the right. Dr. Fleury explains this by a greater flux of blood to the left hemisphere. This fact he has discovered by numerous measurings of the jugular veins, and by inferences from physical laws.

Dr. Leudet (of Rouen) read the notes of a case of "unilateral ephidrosis," and related three other cases on record. All four were patients suffering from nervous disorders. Dr. Leudet thought ephidrosis depended on the vaso-motor system.

Dr. Bitol read a paper "On the Action of Veratria in Cardio-vascular Affections which have not yet reached the Cachectic Stage." The paper related various experiments carried on in the laboratory, and numerous clinical observations, from all of which Dr. Bitol draws the following conclusions:—1. Veratria is an invaluable agent in vascular disorders. 2. It is especially suitable in the disorders which accompany functional hypertrophy of the heart. 3. In respect to the heart (unlike digitalis) it is atonic and hyposthenisant. 4. It does not exert a spoliating action like digitalis, therefore its protracted use does not entail the same dangers. 5. Its office seems to be one of direct compensation. In exciting the sensibility and contractility of animal life, it lulls the morbid activity of the nervous system and of the contractile fibres of vegetative life. 6. Its action is quite distinct from that of digitalis; consequently, when digitalis is powerless veratria must be used. 7. Like digitalis, veratria should not be employed in the ultimate stage of cardio-vascular affections. 8. It should be tried in all diseases which affect the nervous system.

Professor Trélat communicated a note "On Lymphosarcoma, or Lymphadenoma," in which, after having described these lesions, he concluded that surgical interference should not be employed.

Dr. Liétivant (of Lyons) related several cases of "Substitution of Mobility and Sensibility after Nerve-sections." After relating five cases bearing upon the subject, the orator concluded thus: "Surgeons will be less timid when they have to deal with cases which require neurotomy, since they now know that, notwithstanding the section of the nerve, mobility and sensibility will persist, and will acquire, by use, a high degree of perfection."

Foreign Gleanings.

PARALYSIS OF VARIOUS ORGANS, DUE TO DISEASE OF THE HEART.

At the last meeting of the Société Biologique de Paris, Mr. Joffroy, house-surgeon to La Pitié, presented a patient suffering from paralysis of various organs, due to disease of the heart. The patient's history is briefly this:—He is seventeen years old. In March, this year, he suddenly lost the sight of one eye; and a few days after he showed confusion of mind, difficulty of mastication, and almost complete hemiplegia. He was then taken to the hospital, and the following lesions were observed: complete paralysis of the lower part of the face; tightness of the jaws; almost complete immobility of the tongue, whence the difficulty of mastication, deglutition, and the articulation of words. The eye was affected with amblyopia, and on being examined with the microscope was seen to present considerable anæmia of the retina. Hemiplegia was very marked in the limbs. On examining the heart an intense bruit de soufflé, during the first sound, was discovered. This bruit disappeared and came according to the various attitudes which the patient was made to assume. Pulse and respiration normal. The various symptoms increased in intensity, and it was quite impossible for the patient to take food. Finally, by means of the œsophageal sound, food of various sorts was introduced into the stomach. The patient has since that time gained strength, and the symptoms have amended; contraction of the buccal muscles can be excited; deglutition is now possible; the tongue, however, is still immobile, flat, and smooth, and suggests the idea of atrophy.

M. Joffroy thus explains the causation of the various symptoms just described. In the situation of the left auriculo-ventricular orifice there is a floating polypous production giving rise to the variable bruit de soufflé; embolisms thrown into various parts of the brain; through the vertebral arteries into the region which commands the movements of the mouth, tongue, and pharynx; through the arteries of the brain into the hemisphere opposite to the hemiplegic side; through the ophthalmic into the retina.

GASTROTOMY IN CASES OF INTERNAL STRANGULATION.

Dr. Delaporte, in an inaugural thesis just published in Paris, having carefully compiled and investigated a large number of cases bearing upon the above subject, comes to the following conclusions:—Gastrotomy, far from being an invariably fatal operation, has afforded as numerous successful results as other major operations. Gastrotomy, which is pointed out by theory as the best surgical treatment of internal strangulation in the majority of its forms, should therefore hold its place in practice. Gastrotomy, which offers as many chances of success as enterotomy, and which gives more complete results of cure, should always be preferred to enterotomy in cases where some doubt exists as to the cause of strangulation or the choice of an operation. Gastrotomy, when once seen to be necessary and decided upon, must be performed as soon as the symptoms leave no doubt as to the existence of internal strangulation, and as early as possible in order to frustrate the development of peritonitis, the formation of adhesions, gangrene of the intestine, and exhaustion of the patient. The opening of the abdomen must be made in the mesial line, and largely, so as to permit free access to the cause of the strangulation. If the strangulation cannot be reduced the operation must be terminated by opening the bowel in the most favorable situation, and the establishment of an artificial anus through the primary incision. Gastro-enterotomy would thus be performed.

THE NATURE OF MELANOTIC TUMOURS.

In a paper published in *La Gazette Médicale de Paris*, Dr. Nepven, summing up the results of his investigation of melanotic tumours, concludes that the melanic matter is immediately derived from the blood. This is proved by the fact of the existence of coloration varying from yellow and brownish-red to black in melanotic tumours; the presence of yellow matter and blackish granulations in the cells of the tumour (plasmatic cells), and even in the capillaries, the glands and cutaneons epithelia being perfectly healthy. The alteration of the blood takes place locally in the tumour itself. Diffusion and hæmorrhage are the two causes of the coloring matter from the bloodvessels. It is probable that the elaboration of the cells themselves accounts in some degree for the intensity of coloration, and the rapid metamorphosis of coloring matter. Melanosis is, therefore, merely an accident in certain tumours, and this would explain how some of these tumours are only partially melanotic. Part of the melanotic substance may be found in the blood, urine, and principal viscera in the form of coloring matter or a finely granulated substance. Several experiments of inoculation failed to reproduce melanosis.

ASPIRATING PUNCTURE OF THE BLADDER IN CASES OF IRREDUCIBLE HERNIA.

Aspirating puncture of the bladder in cases of irreducible hernia is one of the important medical novelties now being investigated in France. Several strikingly successful cases have already been published, and THE LANCET in one of its late numbers contained the original relation of a case operated on by Dr. Léon Labbé, of Paris. Dr. Demarquay has just presented a paper on the subject to the Academy of Medicine, wherein he states his intention to carry out the application of the proceeding to the following cases:—(1) To all congenital herniæ, and to recent herniæ which become strangulated at the time of their formation; (2) to old cases of hernia perfectly reducible a few days before the strangulation, and in umbilical herniæ recently strangulated. Dr. Demarquay adds that this aspiration of liquids and gas, with the object of rendering taxis more easy, must be practised at an early time, when we are almost certain to put back into the abdominal cavity an intestinal fold which is not yet altered and is still capable of resuming its functions.

CASE OF SUDDEN OBLITERATION OF THE CENTRAL ARTERY OF THE RETINA.

In one of the recent numbers of the *Archives de Phys.* Dr. Sichel records a very interesting case of the above lesion. The patient, aged fifty-four, had been suffering for a long time from insufficient action of the auriculo-ventricular valves, when, after a sharp attack, she became completely blind in the right eye. Examination with the ophthalmoscope showed various small hæmorrhagic spots scattered over the fundus of the right eye, and a large hæmorrhagic patch in the left. The right, which at first did not permit the patient to distinguish light from darkness, improved after a few days, and then again got worse. Examination of the eye at various intervals showed a commencing atrophy of the optic nerve, and then the conversion of the artery into a whitish and opaque cord. Eighteen months after the outset of the disease the patient died; and Dr. Sichel, on examining the eye, observed an obliteration of the central artery of the retina by a clot which entirely filled up the calibre of the bloodvessel, which he believed was embolism due to disease of the mitral valve.

NEW RESEARCHES ON THE PROPERTIES OF ABSINTHE.

M. Magnan, of Paris, is continuing his researches on the composition and properties of absinthe, and has just communicated his latest results to one of the medical societies of that city. By means of successive distillations he has been able to isolate various products: (1) a blue oil; (2) a yellowish oil; (3) an oxygenated product. There was besides a yellowish residue left in the glass. These various substances were tried on animals: ten grammes of the yellow sediment given to a small dog produced no effect. Thirty centigrammes (five grains) of the blue oil produced from eight to ten epileptiform attacks. The oxygenated product proved, however, the most powerful toxic agent. Fifteen centigrammes of it injected into the veins of a large dog caused the most violent epileptic attacks, which followed in rapid succession, and ended in death. There was an extraordinary rise of temperature from 39° centigrade to 42°. The post-mortem showed various apoplectic centres.

CHARACTER OF SCURVY.

During the siege of Paris the medical men of that city had numerous opportunities for the investigation of scurvy. Dr. Leven has just laid before the *Académie des Sciences* the notes of his experience of an epidemic of scurvy which he observed at the Hôpital Militaire of Ivry. The disease, he says, is not due to the absence of vegetatables, since cure could be effected by the use of raw meat, but it was the result of insufficient food and of bad hygienic conditions. In scurvy the fatty tissues do not disappear, but the muscular tissue becomes fatty, the muscular stripe disappears, and is replaced by fatty granulations; fatty degeneration affects the muscles according to their degree of activity; the heart first becomes fatty, then the muscles of the back, the thighs, the arms, &c. In respect to the blood, the quantity of fibrin increases, whilst the number of the globules diminishes by half.

FIVE CASES OF HEMIANÆSTHESIA.

At a recent sitting of the Biological Society of Paris, Dr. Charcot, of La Salpètriére, gave the details of five cases of hemianæsthesia with unilateral trembling which he had observed. The patients had been all of advanced age, and had had a more or less sudden attack of apoplexy, after which hemiplegia, hemianæsthesia, and hemichorea had supervened. In three cases a post-mortem had been made, and Dr. Charcot had stated the existence of a sclerous or hæmorrhagic lesion of the optic layer, radiating more or less towards the cerebral peduncles and the corpora striata. Dr. Charcot said he did not pretend to furnish the definitive explanation of hemianæsthetic phenomena, but thought he might rationally conclude, from his cases, that there existed in the nervous centres a special situation, the lesions of which produced hemianæsthesia of general sensibility.

ON THE VALUE OF SULPHATE OF CHINCHONA.

M. Briquet, the well-known author of an exhaustive treatise on chinchona, advocated the properties and uses of sulphate of chinchona at a recent meeting of the Paris Academy of Medicine. His conclusions were based upon 893 authenticated cases of cure by the sulphate, from Magendie and Chomel to our days. Its success was especially great in cases of intermittent fever of middling intensity. Furthermore, it arrests the paroxysms of typhoid, amends the symptoms of intermittent neuralgia, and is of great benefit in acute articular rheumatism. Dr. Briquet lays great stress on the mode of administering the drug. It should be given in a watery solution, in doses of from fifty centigrammes to one gramme (eight to fifteen grains), according to the intensity of the fever. The whole dose must not be given at once, but must be divided over five or six hours, and it is extremely important that the substance should be taken during the apyretic interval, and at least eight or ten hours before the return of the fit.

ON MYOCARDITIS AND ENDOCARDITIS IN CASES OF DIPHTHERIA, CROUP, AND MEMBRANOUS ANGINA.

In a lecture recently delivered at the Hôpital des Enfants, Dr. Bouchut laid stress on the frequency of disease of the heart in cases of diphtheria and other septic diseases. The endocarditis, he said, was valvular and parietal, of a proliferating character, and was accompanied by fibrinous deposits on the diseased walls, and of large ventricular clots. The fibrinous deposits adhering to the diseased valves may become loose, and be carried away by the blood into the aorta or pulmonary artery, and there give rise to embolisms. These embolisms form infarcti of the lungs, followed by metastatic abscesses and cutaneous infarcti, with or without consecutive abscesses and infarcti of the brain, which may produce cerebral softening.

SIMPLE DISLOCATION BACKWARD OF THE FIFTH METACARPAL BONE AT ITS CARPO-METACARPAL ARTICULATION; REDUCTION.

Dr. Mason, of Brooklyn, in the August number of the *New York Medical Journal* records a case of this kind. On examination, a distinct bony elevation was observed on the back of the hand, in a situation corresponding to the carpo-metacarpal joint of the fifth metacarpal bone. Forcible flexion of the metacarpal bone increased the deformity. The patient was anæsthetised and reduction easily effected by fixing the thumbs firmly against the articulating surface and pressing steadily forward, at the same time extension being made at the distal extremity of the bone. The first effort proved successful.

THORACOCENTESIS IN INFANTILE PLEURISY.

In the course of the discussion still going on at the Paris Academy of Medicine on empyema, Dr. Roger, physician to the Hôpital des Enfants, had occasion to relate the results of his vast personal experience of empyema in children. Referring specially to the question of thoracocentesis, and basing his opinion upon a great number of facts which he related, Dr. Roger thus tersely summed up his conclusions:—Operate *always* in cases of *purulent* pleurisy in children; operate *rarely* in cases of abundant serous effusions; *never* operate in cases of slight effusion.—*Bulletins de l'Académie de Médecine.*

EXTRAORDINARY CASE OF ABDOMINAL MALFORMATION IN A YOUNG GIRL.

This case is described by Dr. W. A. Freund in a recent number of the *Archiv fur Gynäkologie.* He considers it to be an instance of double bowel. There existed in front of the bladder a sort of diverticulum of the large intestine, which opened on the umbilical region in the centre of a tumour, whilst below the pubis, between the labia majora, and a little above the meatus urinarius, were to be found the rectum and anus.

TREATMENT OF BULLAR PEMPHIGUS.

M. Hillairet, of the Hôpital St. Louis, struck by the analogy which exists between the lesions of pemphigus and those of burns in the second degree, has imagined that the same method of treatment would be suitable for both. He is therefore now trying applications of cotton wool, and the oil and lime-water liniment for cases of bullar pemphigus in his wards at the hospital for skin diseases.—*France Médicale,* August 24th.

News Items, Medical Facts, &c.

The Bile Pigments.—By the action of sodium amalgam on an alkaline solution of bilirubin, out of contact with air, Maly (*Ann. Chem. Pharm.*, clxi., 368) has obtained a yellow coloring matter of feebly acid character, which he names *hydro-bilirubin*, and believes to be identical with the coloring matter obtained by Scherer from urine, with that obtained from fæces by Vanlair and Masius, and also with Jaffe's *uro-bilin*. He assigns to it, in a later paper, the formula $C_{32} H_{40} N_4 O_7$ (new notation). This direct conversion of bile pigment into urine pigment is highly interesting.

Chinchona in India.—The reports of the progress in India of the cultivation of chinchona continue to be of the most satisfactory description. A knowledge of the species best suited to the different localities of the peninsula is, according to the *Planter's Price Current*, being rapidly perfected, so much so as to warrant the hope that in the course of a few years an abundant supply of the invaluable bark will be obtained throughout the whole country.

PRINTED AND PUBLISHED BY
Wm. C. HERALD, Agent, No. 52 JOHN ST., NEW YORK.